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.**

J. Badosa et al.

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(– Referee ++ Our responses)

–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.

++We appreciate the thoughtful comments raised by this reviewer.

–Page 1508, Abstract: I suggest to add a sentence that the aerosol load was very small

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at the four sites. Much larger differences are to be expected for polluted conditions.

++One sentence has been added at the end of the abstract: "For more polluted sites (the four considered locations show in general small aerosol load), the uncertainties in the aerosol input parameters would lead to less confidence on the modelling approach."

–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.

++The sentence made reference to the study by Koepke et al (1998). Now the emphasis is set on the modelling (that is, the input parameters chosen) rather than the radiative transfer models: "So, absolute radiative transfer modelling errors could be larger."

–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.

++Thank you very much for the reference. It enhances the context of our study. It has been added and now, the second half of that paragraph is as follows: "Kylling et al.˜(1998) showed relative mean model-measurement differences within $\pm $5{\%} for UVB irradiances from two sites in Greece using two different instruments. Special attention was paid on the aerosol optical input values and their effect on UV. As a main issue, all these studies highlighted the important influence of the uncertainty of aerosol input information on the accuracy of UV calculations."

–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.

++To set a better context for the list of points, the sentence has been changed for the following one: "The originality of the paper comes from the sum of the following points:"

–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.

++The paragraph has been changed. Now we first present the Dobson and TOMS measurements and then Stamnes data: "As input for the UVI modelling, daily values of TOZ from Dobson Ozone Spectrophotometers (hereinafter called TOZ$_{D})$ and from the Total Ozone Mapping Spectrometer (TOMS) version 8 (hereafter called TOZ$_{T})$ are considered. These sources of TOZ correspond to measurements around noon. However, the TOZ$_{D}$ available from the CMDL website (see Table~2) does not always correspond to noontime due to quality criteria. Nevertheless, we found that there were no differences between these values and the actual noon values for the majority of days at each site. Moreover, for more than 80{\%} of days, the differences were of less than 3\,DU and, in any case, the differences were not larger than 5\,DU, which are within the uncertainties of the Dobson measurements (see discussion below).In addition to these ozone measurements, an estimation of TOZ is also available from the UV spectral measurements using a previously discussed method (Stamnes et al., 1991) (hereafter called TOZ$_{S})$."

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

++The AOD measurements at all sites were made to BSRN standards, but the instruments themselves varied from site to site. In all cases, they are multi-filter sun photometer (not AERONET). At the New Zealand and Australian sites, the instruments are 4-channel SP02 sunphotometers, manufactured by CSD Middleton Instruments in

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Australia. Similar instruments are used at Boulder and Mauna Loa. See Table 2 for further details.

–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?

++It is true that several of the NIWA spectrometer systems are based on the Bentham DTM300 monochromators. However, other NIWA systems are based on different spectrometers. For example the UV4 spectrometer used at Boulder in this study is based on the Acton 275. The monochromator is only a small component of the total systems, which includes: diffusers, fibre optics, detectors, temperature-controlled enclosure, controlling electronics, calibration lamps and power supplies, logging and analysis software. None of these components were manufactured by Bentham. Most of the purchase costs for these systems is from NIWA in-house contributions.

–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.

++The word "random" has been deleted

–Page 1517, line 5: Setting the altitude of the site to 2.4km at Mauna Loa and considering 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!

++The paragraph has been changed to emphasize that there are more ways: "The ground level in the model was usually specified as the altitude of each site (see Table~1). However, in the case of Mauna Loa Observatory, the ground level was set to 2.4\,km and the output was taken from 1km above as one way of taking into account the multiple scattering between the atmospheric layers underneath and above

the measurement site as suggested by McKenzie et al. (2001)"

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

++Done.

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

++Since we didn't have AOD measurements at more than one wavelength for Boulder and Mauna Loa, we preferred to introduce alpha in the same way for all sites (to homogenize). The best way we found was analysing the ranges of alpha values that we could estimate for Lauder and Melbourne. We saw similar ranges, and we took 1.4 as a reasonable value, being aware of this limitation. We quantified the reasonable maximum uncertainty that we could add in the modelling by performing calculations of UVI with alpha=0.4 and alpha=2.4 for the day with higher AOD values (which was day 63 in Boulder, see the discussion in pag1522 lines 9-14). That lead to a change in UVI of +/ - 3%, with respect to the case with alpha=1.4. We took this as the maximum uncertainty of setting the alpha constant. Since for the most of cases the uncertainty was expected to be much lower, we considered that an alpha constant to 1.4 was a good and simple assumption. It is true that this is a good assumption because we have in general low aerosol loads, and that the stated uncertainty would be sensitively larger for more polluted sites.

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

++Maybe the word "scenario" explains better what it actually is. However, the way we understand it is that each single measurement corresponds to a single scenario that we try to explain (by modelling) in fifteen different ways, which we call cases.

–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 calcula-

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tions to get the upper bound of the irradiance for the observed AOD.

++We set SSA at 0.9 as a typical value (according to Madronich, 1993). We got then the feeling that it would be interesting to include also the value SSA=0.7 since, for some days, this value gives much better results in the modelling-measurement comparison (as for Fig 10). The reason why we didn't consider SSA=1.0 was just to avoid having more modelling cases (so adding complexity). However, Fig. 3 gives a slight idea of which could be this upper bound through the effect of not considering aerosols at all in the modelling with respect to using SSA=0.9.

–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.

++From the Earth Probe TOMS User Guide Section 4.3, page 17 (ftp://toms.gsfc.nasa.gov/pub/eptoms/EARTHPROBE_USERGUIDE.PDF): "The average ground terrain heights are from the National Oceanic and Atmospheric Administration (NOAA) National Meteorological Center (NMC), provided in km for a 0.5-degree x 0.5-degree latitude and longitude grid." And from page 32, this is made clearer when they state "Ěbecause the algorithm retrieves the actual column ozone above the surface, not above sea level." What could have somehow affected specially the measurements over Boulder and Mauna Loa is the criteria of considering the presence of snow and clouds, which are based mainly on reflectivity thresholds and climatological data.

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

++The sentence has been changed for "the sites with larger aerosol load are"

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

++We have replaced it with "accidentally"

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

++Yes, we believe that, as it would be expected, USSA resembles more the measured OP in Boulder than the other locations.

++We agree that it is interesting how the differences in temperature profile and ozone profile have quite different effects on the surface irradiances. For example, changes in the TP have a much smaller dependence on SZA.. An explanation for this is outside the scope of the present paper. However, the importance of the tropospheric contribution is a strong function of SZA (Bruhl and Crutzen, 1989), so its not surprising that there would be a strong SZA dependence on the ozone profile. On the other hand, changes in the temperature seem to have just an offsetting effect caused by the temperature dependence of the ozone cross sections - where the effect of these differences is heavily weighted to the stratospheric component (most ozone, and largest temperature differences occur there).

++Brühl, C.H., and P.J. Crutzen, On the disproportionate role of troposphere ozone as a filter against solar UV-B radiation, Geophysical Research Letters, 16 (7), 703-706, 1989.

++The latter reference is now called in the text and at the end of the paragraph the following has also been inserted: "Further discussion about the dependencies of spectral

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UV global irradiance on ozone and temperature profiles can be found in Schwander et al.~(1997)."

++Schwander, H, Koepke, P, and Ruggaber, A. Uncertainties in modelled UV irradiances due to limited accuracy and availability of input data. Journal of Geophysical Research, 102, D8, 9419-9429, 1997

++Both references are now in the reference section.

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

++Done.

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

++The following sentence has been added to the end of the paragraph: "Larger uncertainties from the AOD profile choice should be 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!

++With the sentence "the ozone amounts derived agree well with those from other sources for which there is no circularity", we mean that there is no circularity in the Dobson measurements. We agree that there is circularity in using Stamnes ozone, although we reckon that it has a low impact on the obtained results.

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++More specifically, We do not state that there is no circularity. Rather we state that there is some circularity, but the effects seem to be rather small, at least for SZA up to about 70 degrees. For larger SZAs there is sometimes a tendency to get larger ozone retrieved as the SZA increases, and that is why we limit the retrieval to SZAs less than 80 degrees. However, if this was the only effect, then the same behaviour would be seen for large SZA and the morning and the afternoon. Any asymmetries from that pattern are due to real changes, such as diurnal changes in ozone. Figure 7, which shows all valid ozone retrievals up to SZA=80 degrees, clearly shows a decrease in ozone over the day. This decrease is consistent with the decreases seen by the Dobson, and is also reflected in the progression of daily satellite-derived ozone from 356 DU on the previous day to 314 DU on the following day.

++We have altered the text on page 1523 (first paragraph) to clarify these issues: "The UV spectrometer results showed also that this trend continued over the early morning and late afternoon periods as well, which is also reflected in the progression of daily satellite-derived ozone from 356 DU on the previous day to 314 DU on the following day"

–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.

++It is true that the use of these abbreviations add initially some complexity for the reader but it also saves many words and makes the analyses more compact and fluent. We believe that the repeated use of these abbreviations in the paper supports their definition.

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

++Information about the single scattering albedo (and even the optical depth itself) is really quite limited in the UVB region. If there are organic aerosols present (e.g. in

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plumes from Denver), then they may absorb strongly in the UVB region, leading to larger optical depth and reduced single scattering albedo. See our response to the Referee #1 comment on Page 1530

++We remember discussing about that and having a look at the wind direction for that day. We saw that the wind was blowing from South, South-East, during the day. Then, one reasonable interpretation could be that the observed AOD increase along that day could be caused by the arrival of more polluted air from Denver, in which case that would be urban aerosols, and thus probably quite absorbing.

–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.

++Boulder becomes Melbourne-like just in terms of SD values, not in terms of systematic measurement-model agreements. It is true that there is a significant change in the average agreement after removing day 361 in Boulder but this day was really an outlier (the only day with snow on the ground). The other reason why the average changes so significantly is because for Boulder we worked with only 16 days, thus each of these days have an important weight on the average.

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

++Behind that, there is a well known structural change in the PTFE. We found an explanation from Billmeyer (1984): "This polymer exists in two helical conformations that may be described as twisted ribbons in which the fully extended planar form is distorted to have a 180žC twist in 13 CF2 units in the more stable (low temperature) form. Above 19žC this form is replaced by a slightly untwisted conformation with 15 CF2 units per half-twist. Above 19žC the x-ray diffraction pattern shows diffuse streaks that are interpreted as resulting from small angular

displacements of molecular segments about their long axes (...)" We even performed DSC calorimetric experiences with the a PTFE sample from a NIWA diffuser. The obtained results are collected in Annex C of the Thesis of the leading author (http://copernic.udg.es/angelets/jordi/Thesis_Jordi_Badosa.pdf).

++Billmeyer, F. W. (1984), Textbook of Polymer science, 608 pp., Wiley-Interscience, New York.

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

++We have changed the sentence "So it could be that the diffuser temperature had responded more slowly to this increase." by "Probably, the diffuser needed more time to adapt to the ambient temperature and could not follow it."

–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.

++This suggestion is based on observations of webcam images for several days, and might not be always true. Moreover, as we mention, the field of view of the webcam images is quite limited. We put this sentence as an invitation to open a discussion about this subject.

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

++The actual effect would depend on the altitude of the cloud below the observatory. Based on a simple TR calculation, for small SZA, an increase in irradiance of 9% at MLO would correspond to an increase in effective surface albedo of approximately

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30%. Assuming the clouds have an albedo close to unity, this could be achieved by a buildup of clouds covering 30% of the area below the observatory.

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

++Done.

–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

++See our comments to Referee #1.

–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"

++Change applied.

–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 particularly hard to obtain in case of clouds.

++The first sentence of the paragraph has been changed as follows: "Under cloudy conditions (which represent most of the time in many sites), very much larger errors are expected when modelling since the cloud optical parameters needed as input information are hardly ever available."

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

++We agree. (To the editor) Could it be possible to have it printed in Landscape in final version?

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

++It has been corrected. Thank you.

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

++The sentence has been changed for "for sites with larger aerosol load like Melbourne and Boulder"

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