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Interactive Comment

Interactive comment on "Improved sky imaging for studies of enhanced UV irradiance" *by* J. M. Sabburg and C. N. Long

Anonymous Referee #2

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Review of "Improved sky imaging for studies of enhanced UV irradiance" by Sabburg and Long

General Comments

Overall, this is an interesting paper regarding the effective use of an all sky camera to diagnose the cloud conditions leading to enhanced solar radiation at the surface. I like the discussions of the algorithms and their potential utility in determining the cloud conditions leading to enhanced conditions at the surface, but I do have some issues with the conclusions being reached regarding these enhanced conditions. The largest issue relates to the scan time for the spectroradiometer used in this study. The scan time (2 minutes) is much longer than for spectroradiometers (3-15 seconds) used in previous work cited by the authors. I believe that this difference in scan time is contributing to the differences found in comparing results to earlier work. Also, I disagree with the interpretation given for the findings of several cited references (e.g., Eckstein et al., Crawford et al., Pfister et al., Kylling et al., and Mayer et al.). Specific details related to these issues are outlined below. These problems should be resolved before publishing this paper.

Specific Comments:

1. The abstract states that there are three algorithms, but only two phenomena are mentioned. "Uniformity" appears to the be missing from the list of algorithms to be discussed.

2. On Page 6223, the authors state: "The maximum variation of the data (ś9%) over the complete SZA range was the same as the uncertainty of the instrument. Thus it was not possible to separate the effects of ozone variation on the UVI data, even though the clear sky scans had a similar range of ozone levels as the complete dataset." The range of ozone levels for the complete dataset is given earlier as 248-311 DU. Using the quick TUV online calculations (http://www.acd.ucar.edu/TUV/), this corresponds to a 30% change in UVI. This far exceeds the ś9% variation of the data, so I am surprised that no evidence of influence due to ozone levels could be discerned, even if just to identify some outliers.

3. On Page 6223, the authors state: "The maximum enhancement (1.4) was recorded at 11:00 a.m. local time on 6 February, with a corresponding SZA of 19.4. There was approximately 10% opaque cloud amount with no detectable thin cloud." This is a rather curious result given that previous studies have shown enhancements to increase with cloud fraction and reductions to be strongest for small cloud fractions (see Figure 6 of Pfister et al.). The observations and calculations presented by Crawford et al. also indicate that enhancements increase with fractional cloud cover. Does this maximum enhancement clearly stand out from the rest of the data (especially given the ś9% variation)? Would a plot of enhancement versus cloud cover be more appropriate?

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How does such a plot compare with Pfister?

4. On page 6225, the suggestion is made that Eckstein et al. and Crawford et al. provide similar explanations for the wavelength dependence of enhancements, but this is not true. First, Eckstein et al. performed no model calculations and essentially offered conjecture as to the reasons for the wavelength dependence (i.e., tunneling between clouds). Crawford et al. performed simple model calculations to demonstrate that the wavelength dependence could be produced simply by the transmission of radiation through clouds and the conversion of direct radiation to diffuse radiation. This is not to say that tunneling cannot occur, but since the Crawford et al. calculations were based on a simple 1-D radiative transfer code, no complicated "tunneling" phenomena were necessary to reproduce the observed wavelength dependence.

5. On Page 6227, the authors state: "Thus far only cases of upward trends have been published, based on measured clear scans as the reference (e.g. Eckstein et al., 2003 and Crawford et al., 2003), and downward trends based on model clear scans (e.g. Mayer et al., 1998 and Kylling et al., 1997)." This is not a correct summary of the findings in these papers. First, Crawford et al. found upward trends based on both model and measurement, not just measurement as suggested. Also, neither Kylling et al. nor Mayer et al. were investigating enhanced conditions below cloud. The downward trend reported by Kylling et al. was for conditions above the cloud, not below the cloud, which is a different issue altogether. For the reduced radiation below the cloud, Kylling reported a downward trend in transmittance with increasing wavelength which is consistent with the observations and modeling reported by Crawford et al. and results reported by Eckstein et al. (see Figure 11). Finally, Mayer et al. evaluated conditions of reduced (not enhanced) radiation below cloud which again is a different matter than is being investigated here with respect to the wavelength dependence under enhanced conditions below clouds.

6. Finally, I believe that the wavelength dependences shown in Figure 5 are influenced primarily by cloud changes during the scans. The upward trend seen by Eckstein et al.

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and Crawford et al. were based on spectroradiometers using 3 and 15 second scans, respectively. By contrast, the spectroradiometer used in this study took a full 2 minutes to complete a wavelength scan from 280 to 400 nm. The downward trend shown for case (b) in figure 5 shows significant oscillation (as do the other curves) indicating changing cloud conditions. In the TSI images for case (b), the glint from the sun on the shadow band shows considerable dimming between the beginning and end of the scan. This is not observed in the other images. This dimming would be consistent with less transmission of direct beam radiation at the time that the longer wavelengths were observed at the end of the scan. Thus, I am not convinced that a downward wavelength trend exists. It is more likely an artifact of the extended scanning time.

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