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Atmospheric Chemistry and Physics Discussions

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Interactive Comment

Interactive comment on "Solar irradiance at the Earth's surface: long-term behavior observed at the South Pole" *by* J. E. Frederick and A. L. Hodge

Anonymous Referee #1

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Review of Solar irradiance at the Earth's surface: Long-term behavior observed at the South Pole by J. E. Frederick and A. L. Hodge.

This work presents very interesting results analyzing 17 years of data from spectro-radiometric measurements at the South Pole. It is a very interesting study providing solid results, combining measurements and radiative transfer calculation in a unique environment. I suggest the publication of this work in ACP with minor changes. These changes together with some questions/suggestions for clarifications of some aspects on the analysis are reported below:

P25872 Introduction:

The introduction is very short. Personally I do not have a problem presenting the factors

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affecting UV with only 3-4 references as it is but a reader would like to know, based on other studies, why this study is important. Are there any other related studies for the area? Are there any other so long term studies using the specific instrumentation and methodologies ?

P 25873 The dataset: To understand the data more in depth, I would suggest to include a paragraph describing the instrumentation and methodology (mentioned here only with references Booth (1994) and Bernhard (2004)). Although instrument and quality control details are mentioned in the referred publications, I think it would be informative for the reader to have a small summary of such issues.

Page 25876: About the angular error and its effect on the calculated series.

Are the numbers in lines 21 and 22 (1% and 2.8%) referring to the relative contribution of the angular response uncertainty (Bernhard, 2004 technical section of the referenced paper)? In a relative sense and when looking for trends on the 17 year measurements, this relative uncertainty can be small as reported. However, when comparing R values measured at UVA and visible wavelengths, the effect of the diffuse sky isotropy assumption in the cosine correction could be large (as reported in detail in the above mentioned Bernhard manuscript) for partial cloudy (or overcast) conditions. So this can affect the discussion on the wavelength dependent enhancement on the irradiance due to partial cloudiness. (e.g. figure 6).

As an example case: for a day with a partial cloudiness but with the sun visible where corrected measurements show e.g. RVis>1 and RUva<=1: Based on the data processing/correction methodology, model calculations having irradiance input from Visible wavelengths would suggest that for the cosine correction we have a cloudless sky case. The isotropy assumption of the diffuse light leads to an overestimation on the correction of the visible irradiance, compared with the one at UV wavelengths, especially for high SZA's where the diffuse component in the total irradiance is high. The result will be an overestimation of the Visible irradiance post-corrected data compared

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with the UVA ones and the tendency for RVis>RUva.

Figure 10. What does exactly mean fractional cloudiness = 0 and the sun is obscured by (what portion ?) of clouds ? If the portion of the sun covered in the analysis for fig. 10 is proportional to the cloud fraction then R has to be 1. Clarify if by "sun covered" you mean no direct beam contribution to the total irradiance.

I would find it very interesting if you could use the conclusions of your study in order to comment on the new-published work by Haigh et al., 2010 –" An influence of solar spectral variations on radiative forcing of climate" doi:10.1038/nature09426. Reporting that mainly that: over the sun declining phase of the solar cycle there was a four to six times larger decline in ultraviolet than would have been predicted on the basis of previous understanding.

Interactive comment on Atmos. Chem. Phys. Discuss., 10, 25871, 2010.

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