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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Personal UV exposure on a ski-field at an alpine site" *by* A. M. Siani et al.

Anonymous Referee #3

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General comments:

This is a well written paper providing new and interesting information. However, as already noted by the other reviewer, there is too little and partly unclear information about the calibration procedures and its uncertainties. Considering the uncertainties of well-maintained spectroradiometers on which the calibration of well maintained broad-band instruments is based, it is extremely unlikely that an uncertainty analysis would give uncertainties of about 10% only. Instead much higher uncertainties must be assumed. On page 2750, line 20, or on page 2751, line 21, there is no hint how the estimate of 10% or 20% may have been assessed. In addition the dependence of c on altitude, orientation and shadow dependence suggest severe calibration problems that are probably caused by the spectral mismatch between the broadband meter and polysulphone badges. Since these calibration issues partly touch the validity of the results, a major reanalysis of the calibration procedure and its uncertainty analyis is

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recommended. Equation (1) can only be used if the spectrum remains constant, which isn’t the case here obviously. In the absence of such a detailed analysis, which is probably beyond the scope of this paper, much more careful statements about the uncertainties are required.

In addition the physical quantities are often not clear:

- Page 2746, line 23 and page 2748, line 16: What is a “UV intensity”? Irradiance, radiance or something different? - What is an ambient dose (from the context it can be guessed it the erythemally weighted irradiance on a horizontal surface)? - How is exposure defined? On page 2756, line 20, there is a statement that the exposure is higher on ski-fields than on the beach. However, for the actual exposure (at least in the sense where term in usually used) the exposed area plays a role as well. The quantities need to clarified.

Specific comments:

Page 2746, line 20: the abstract should be written in a way that it is understandable for those who do not know what L* and b* is, or it should be properly explained

Page 2747, line13: the statement is correct, but the old references from the 60s (Bener) are missing

Page 2747, line 16: the effect of clouds below the observation site is another important factor (e.g. published by McKenzie et.al., or Seckmeyer et.al. 1997)

Page 2752, line 12 and line 15: it would good to give references, where these tests are described in the scientific literature

Page 2752, line 22: replace “median” by “average”, or is something else meant?

Page 2753, line 1: how were the skin types determined?

Page 2753, line 22: this is what the authors can hope, but how can they know that?

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Page 2754, line 2: it must be doubted that the conditions were identical. If there is a strong influence of snow albedo, it should be noted that the snow albedo is changing drastically with snow age (e.g. Wuttke et.al. 2006) and this in turn changes the UV exposure

Page 2754, line 7: the dependence of UV irradiance on altitude cannot be described by a single number. It depends on many parameters and is a function of wavelength. This should be mentioned (reference: Seckmeyer G., Mayer B., Bernhard G., Albold A., Erb R., Jaeger H., Stockwell W.R.: New Maximum UV Irradiance Levels Observed in Central Europe, Atmospheric Environment, Vol. 31, No 18, pp 2971-2976, 1997)

Page 2757, line 21: this is an important message for the public, which may be emphasized in the abstract

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Interactive comment on Atmos. Chem. Phys. Discuss., 8, 2745, 2008.

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