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Interactive Comment

Interactive comment on "A method to generate near real time UV-Index maps of Austria" *by* B. Schallhart et al.

Anonymous Referee #1

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General Comments

Note: I have read the Anonymous Referee Comment RC S145.

This paper describes a method to be applied operationally for generating area maps of reconstructed UV indices (UVI) for Austria. The reconstructed UVI are computed by estimating clear-sky UVI using a simulation tool to which are given as input a certain number of environmental parameter observations or estimations (ozone, albedo, aerosol turbidity) as well as time and location. The clear-sky UVI are then scaled using a cloud modification factor (CMF) accounting for the influence of the clouds on UV radiation transfer. The main development brought by this method is using CMF derived from cloud information retrieved by the MSG satellite SEVIRI radiometer, and correcting the satellite-derived CMF with CMF information derived from ground-base UVI measurements.

Such line of research is valuable. UVI reconstruction technique based on inferring UV radiation at the ground using a mixture of theoretical knowledge of radiation transfer, empirical information, and observations of proxy parameters is popular because of the difficulty and burdens of precisely and reliably measuring UV radiation. These burdens result in UVI measurements being sparse and the data time series being limited. Because radiation is strongly affected by local phenomena (mainly meteorological), and presents large spatial variability, it is challenging to generate accurate maps of UV radiation. Using satellite information about meteorological conditions is a venue that is worth pursuing.

Unfortunately, the present manuscript only describes a method, but fails at demonstrating its validity, or estimating its accuracy. In my opinion, a major flaw of this manuscript is that there is no section devoted to verifying how accurate is the UVI estimation resulting from the method presented. Such a verification step is needed when presenting a new development in a UV reconstruction method (or any other estimation method). One would expect a result significantly better than the cited results of Verdebout (2004) and Arola et al. (2002).

Such verification step should allow verifying the validity of the method in a statistical way for different conditions. This means it should not be restricted to a limited number of comparisons between algorithm estimation and observations for specific cases. A valuable method would be 1) systematically removing the information from the observing stations one at a time from the algorithm; 2) infer the estimated UVI at the station whose observation was not included in the algorithm, and 3) compare the obtained result with the observation at the station. Repeating this procedure for all stations and a representative number of conditions would allow estimating the accuracy of the method. I would advocate performing such verification for daily doses, rather than monthly doses, because I am not convinced of the usefulness of checking the latter (see specific comments).

Specific comments

In the introduction, it is mentioned that a disadvantage of satellite data is the problem of pixel size, and smoothing of atmospheric characteristics over relatively large areas (p 2145, ll. 21-23). While this is a disadvantage, it is not the only one I would mention. Another problem worth mentioning is that there are substantial uncertainties in the cloud characteristics determined by satellites, especially for low clouds or in case of multi-layered clouds, and even more uncertainty in inferring the CMF from these characteristics. The reason I would mention the latter problem rather than the problem of pixel size, is that the method presented (which corrects the satellite-derived CMF with CMF derived from ground-based information) has the potential of correcting part of such uncertainties, but little potential in improving the problem of pixel-size smoothing, except maybe in the very vicinity of the observation.

In section 3.1, it is mentioned that rms value of relative difference between satellite estimates and measured erythemal doses was 29% (bias 3%) for daily doses, and that the rms of relative difference decreased to 5% when comparing monthly doses (p 2149, ll. 4-6). Actually, I wonder if there is anything else than the decrease expected from statistics in this decrease of the rms to 5% for monthly doses. The rms gives information on the squared sum of the bias and standard deviation of the average relative difference on days or months. If there is not too much auto-correlation one expects a reduction by square-root of 30 in the standard deviation when going from daily to monthly doses. Using 29% rms and 3% bias on daily dose, one expects 6% rms on monthly doses just from statistics.

In section 3.2, the fact that the ground observations and satellite observations are not perfectly synchronous is emphasized (p 2150, ll. 27-29) just after the mention of a bias between clear-sky observations and model simulations. The way this information is given makes it look like it could also be a cause for the bias. However, I cannot imagine

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how this could be the case, except if the time difference would systematically lead to a same sign solar zenith difference between the time of simulation and the time when the sky was identified as clear with satellite information. If this is the case it needs to be developed, if it is not this sentence about time synchronization is only confusing.

In section 3.3, scatter plots of ground-based and satellite-derived CMF are presented, and the correlation between both is discussed. Actually, I could not find any computation of the correlation. Since the discussion is about it, I think it needs to be given as a table for the different stations. At present, the discussion of this point is only qualitative, and does not include quantitative information.

In section 3.4, it is mentioned that no information on snow line distribution is available in Austria (p 2152, ll. 1-3). While I did not thoroughly check this claim, there is a lot of work on snow mapping (including by satellite) on the Alps (e.g., Foppa et al., 2004, Operational sub-pixel snow mapping over the Alps with NOAA AVHRR data, Annals of Glaciology, 38, pp. 245-252). In case the information from such work is really not suitable, the method for daily estimation of the snow line altitude should be described, at least cursorily.

In section 3.4, the correction function for cases when snow covered pixels could be wrongly identified as clouds is indicated as being implemented independently of the site pixel correlation of satellite derived and ground measured CMFs (p2152, ll. 19-20). I do not understand what the authors mean or what is the difference with the other cases, since the location of the F(ground) vs. F(MSG) point is used as in the other cases.

Technical corrections

P. 2147, l.14 "Finally the results are transferred in units of the UV-Index". Do the authors mean "Finally the results are translated in units of the UV-Index"?

P. 2150, l. 18 "Albedo and Angstrom coefficient beta are dependent on altitude and on

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the snow line." Altitude and snow line are important for albedo. While altitude may influence Angstrom beta coefficient, there are many other important factors that influence it (weather situation, etc.).

Interactive comment on Atmos. Chem. Phys. Discuss., 8, 2143, 2008.

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