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Interactive comment on "Optimizing UV index determination from broadband irradiances" *by* Keith A. Tereszchuk et al.

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Responses to Anonymous Referee #1

Interactive comments on:

Optimizing UV index determination from broadband irradiances
by
Keith A. Tereszchuk et al.

Provided by: Anonymous Referee #1
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Preamble: In addition to the changes made to the manuscript following the suggestions of the two reviewers, we also identified three minor issues that required performing a re-analysis of the data and also modifying comments regarding the comparison to Brewer measurements of Section 2.2. These changes did result in a small correction in the scaling and fit factors, but results remain essentially the same (other than improving the agreement with Brewer UV irradiances). These points are as follows:

1. An adjustment of the broadband wavelength boundaries from 280, 294, 310, and 400 nm to 280.11, 294.12, 310.70, and 400.00 nm (this was an error)
2. A correction in applying a moving boxcar averaging window covering $\pm$0.25 nm about sampling points at intervals of 0.5 nm
3. A correction in the calculation of differences with the Brewer UV spectra in Section 2.2, resulting, most significantly, in a reduction of the mean percent differences for the 311-330 nm band from 7.5% to 2.9% (and implying related text changes).

The correction of few other typographical errors were also made.

The following are general comments and questions:

I am not accustomed to seeing correlations expressed as percent, rather as a unitless values ranging from -1.0 to 1.0.

The root mean square relative error calculation provides a more tangible, quantitative, overall representation of the errors and their scatter. Percentages were used to determine the absolute range of values that were encountered. While the Pearson correlation coefficient, R (ranging from -1.0 to 1.0), which provides a value which represents the "goodness" of the fit in a linear correlation, could also have been calculated and shown, the root mean square errors (and mean differences) were considered sufficient and more appropriate for our purpose, in addition to presenting the scatter plot themselves.

This is a rhetorical question: Which is preferred : 'UV index' or 'UV Index'? Even the WMO web page has a mixture of both. But its acronym is 'UVI' implying that 'index' is capitalized.

While, we are not aware of a definitive preference, one could choose to follow the convention of a capitalized 'index' considering the often used acronym 'UVI'. The format for using the lower case i in this work was simply done to avoid any possible conflicts with the manuscript composition guidelines with regard to figures, section titles, etc that have been detailed by the publisher, while trying to maintain consistancy throughout the paper. Having said that, we see now that there are 2-3 times where consistancy was not maintained. The manuscript has therefore been edited so that 'UV Index' appears using only the uppercase I and the term is treated as a proper name where the word index should be capitalized.

Comments, questions:
P2, L20: Should be ‘oxygen (O2)’.

Correction made.

P2, L27: Expand on what is meant by ‘more sensitive population.

Changed to more photosensitive populations. This refers to people who have extremely little melanin/skin pigmentation which provides a natural UV barrier. There are also a number of ailments where minimal doses of UV radiation can cause allergic reactions and severe burns. Increased photosensitivity can also present itself as a side effect for a variety of medications.

P3, L4: ‘erythemal action spectrum (EAS)’

Correction made.

P3, L15 The UVI does not have to be an integer.

The word ‘integer’ has been removed.

P3, L17 The tropics have high UVI values not just because the SZA is small, but also because the total column ozone there is also low compared to higher latitudes.

Commentary appended to the manuscript as ‘... solar zenith angle and the total column ozone are small.'

P3, L24 I would also cite the two WMO reports addressing the UVI and the 'Global Solar UV Index' publication by the WMO.

Citations added in a new paragraph immediately following equation 3. The bulk of that paragraph was previously in the abstract and moved following a request by the second reviewer to shorten the abstract.

P4, L 6 Is there a reference for the GEM?

Citation of 'Charron et al. (2012), and references therein,' added.

P4, L11 Provide a reference here at the initial mention of the Cloud-J model.

Citation of Prather (2015) added.

P4, L20 Is there a reference for the cccmarad RTM?

Citation of Scinocca et al. (2008) added.

P4, L34 Is the ozone (total and mixing ratios) really generated separately? The next paragraph discusses how LINOZ scheme is used 'within' the GEM to generate ozone forecasts.

Yes, the ozone analyses (ozone mixing ratio analysis fields) were generated separately from the weather analyses. This just means that a separate application of the variational assimilation with GOME-2 ozone data (described in the next paragraph of that section) was applied for ozone relative to the application of variational assimilation with weather data. This was done since the weather analyses had been generated previously. The ozone forecasts are generated by LINOZ within GEM. The process of assimilation improves on these forecast fields at regular time intervals of 6 hours, these improved fields are called analyses and are fed back to the model (to serve as new initial conditions) for it to generate the forecasts for the following time periods. The total column ozone is calculated (also in LINOZ) from vertically integrating the ozone mixing ratio profiles.

P10, L7 Can you determine the elevation adjustment per kilometer to determine if the difference between the grid point elevation and the actual elevation is the reason?

This could be performed but was not done. It is just one factor among many others that could have affected the comparisons and results over the different stations and there was no clear evidence to suggest that it was a comparatively important attribute to this effect. A small change in UV index in the order of roughly 1.5% only is estimated for a difference of 150 m. A related statement has been added to the text.

P10, L7 While doing the above can you determine if the elevation adjustment is equal at all UV wavelengths or wavelength specific?

An adjustment for elevation would require a wavelength specific correction. To demonstrate, the surface pressure was modified in Cloud-J for seven wavelengths in the 280-400 nm range in a given geographical location (Toronto, 29 Aug 2015) to determine the percent difference in the surface irradiance contribution for each of the wavelengths at two different surface pressures. Pressures of 995 hPa and 1013 hPa were used, which correspond to an altitude difference of ∼150 m. The percent differences in irradiance are as follows: 280 nm (1.55%), 300 nm (1.05%), 320 nm (0.68%), 340 nm (0.54%), 360 nm (0.45%), 380 nm (0.38%), 400 nm (0.32%). In all, a disparity in altitude of up to 150 m would generate an error no greater than ∼1.5%.

P10, L12 Is there a plan to bias adjust the GOME observations to bring them more in line with the Brewer observations?

Yes. That work has been done (using another satellite data source that is in better agreement with Brewers) and is the topic of a separate journal paper to be submitted.

P12, L4 Typing error : "Simpson's rule"

Text Corrected.

P12, L15 Do you plan to 'correct' the GEM equivalent broadband absorption cross-section and the TOA solar fluxes to agree with the Cloud-J?

We only plan to scale the GEM broadband irradiances to emulate the general tendency of Cloud-J output as a function of the irradiance value as done in this paper and indicated at the end of Section 3.1.1. This takes the position that the OMI TOA solar spectrum in Cloud-J are deemed 'correct'. The sample effective broadband cross-section value was derived for irradiances at the surface and would not be valid for other levels (nor necessarily all differing ozone profiles). As well, choosing to revisit or not the TOA solar fluxes (and or the applied cross-sections) would be the prerogative of those responsible for the model.

P13, L20 All of these adjustments or scalings will need to be revisited every time the GEM's radiation code is modified. Communication and collaboration between the authors and the GEM modelers needs to be strong such that these differences can be addressed and best corrected in the GEM so that the number of adjustments in the UVI computations is limited or eliminated.

Yes, should there ever be future pertinent modifications to the GEM radiation code, adjustments to the scalings and fits for UV Index determination would have to be made or considered depending on the significance of the change. We do not anticipate our work affecting the GEM radiation code development as such. A suite of programs has been created with a users' manual so that other collaborators/modelers can rerun the programs using updated GEM output files containing data using a newer/modified radiation scheme so new sets of scaling functions can be generated and the broadbands re-weighted accordingly.

P13, L31 The I294-310 is where the bulk of the erythemal weighted values come from. I would think that the coefficient (11.03) would be total column ozone and solar zenith angle dependent.

Considering Figure 9 and the related results, it was a pleasant surprise that constant scaling factors were sufficient for that equation to provide good UV Index values (e.g. errors/differences in UV Index of typically still within 0.2-0.3 with some exceptions), this in light of the erythemal weight varying significantly with wavelength for the two central bands. So the dependence of the UV Index on SZA and ozone with this equation is sufficiently well captured through only the broadband irradiances themselves - as the calculation of broadband irradiances is dependent on SZA and the ozone profile (and, as such, total column ozone).

P14, L18 I presume that the actual total column ozone was used during this comparison and not the OMI climatology, then why wasn't the GEM's albedo used instead of the OMI albedo climatology? Does the GEM's albedo need to be corrected to the OMI's for 100% snow cover? Using the GEM's albedo would then eliminate the 'cold spot' discussed in the following paragraph. The purpose of these two difference plots should be to show the differences between the integrated and linear solutions, not the differences between each and the GEM.

The OMI albedo climatology was used because, with it, wavelength specific global fields for surface reflectivity could be used by Cloud-J to perform the high spectral resolution irradiance calculations. GEM uses a different approach for surface reflectivity where global albedo fields are represented by broadband (UV-NIR) effective values for reflectivity that have been differentiated within the model for specific surface types, of which contain the albedos for soil, glaciers, water, ice, and the aggregated value. For this work, it was therefore deemed more advantageous to use the OMI climatology for our purposes. The resulting larger differences such as in the 'cold spot' would have been removed/reduced using the same albedos. We were willing to accept retaining these differences considering the good agreement of the results elsewhere.

Whether the GEM albedos would need to be corrected to (or account for) the OMI-based values for 100% snow cover would then be the prerogative of the GEM model developers.

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Discussion paper

We think the purpose of the Fig. 9 needs to cover both aspects (differences with GEM-based values and differences between the methods). While this is implied at the beginning of the second paragraph of Section 3.1.2, a sentence has been added to mention that the integration approach fairs a bit better, i.e. 'The integration approach provides better agreement to Cloud-J, this by up to about 0.1-0.2 for some locations.'

P15, L8 What is meant by 'short term forecasts'? 6, 12, 24, 48 hour?

For this particular section, it refers to daytime 7.5 minute time steps (corrected from 12 minutes - a mixup between two forecast setups) covering up to 24 hours. The text at the beginning of this section has been changed to 'GEM model 24-hour forecast output at 7.5 minute intervals over successive twelve hour forecasts'. A similar change has been made in the last sentence of the Fig. 11 caption. In the section the data spanned solar zenith angles from morning to night. Other references to 'model short-term forecasts' in this section were changed to 'model output'.

P15, L30 Instead of just using the 18 UTC observations and model output, other times of the day could have been used to generate additional UVI and solar zenith angle determinations. Additionally, the range of total ozone values over Canada during July and August are rather small. Comparisons between model and observations could have also been done for April or May when the sun is relatively not too low in the sky but range of total column ozone values is much greater.

The 18 UTC field alone were used only in the other sections as clarified for the above point. While we did have forecasts relying on assimilated ozone data for the Summer already available, we did not have such forecasts for the Spring.

P16, L31 As % cloud amount increases so does the variability of transmission through the clouds. Such a plot in place of the density plot may better show the differences between the Cloud-J and the GEM all sky values. It would be interesting to note if, via the Cloud-J model, there is a spectral dependence of UV adjustments upon cloud amount, type, altitude. The point of the text discussion is that the GEM and the Cloud-J produce reasonably similar results under all-sky conditions. Is it known whether either is correct against real world observations from the Brewers or solar radiometers?

The purpose of the density plot was to make the point that there were many more occurrences of reasonably good agreement than large disagreements, this not being evident from panel of Figure 12. The previous P16 L32-33 lines were modified to '... along or near the regression line, largely, but not entirely, represent those surface irradiances under cloudless or light-cloud, conditions. The probability of deviation from the regression line typically increases with increasing cloud amount and opaqueness.' This is further illustrated by Figure 13 which shows the reduced agreement with increasing ECC (cloud fraction X (1- cloud transmittance)). The last sentence of that paragraph was not clear and out of place. It has been moved following the next paragraph introducing Figure 13 and clarified.

A plot of cloud amount itself versus variability (and or differences) of transmission through the clouds would provide a demonstration of the difficulty of correlating cloud amount alone to the impact of clouds on the UV Index - at least when cloud amounts are not that small, e.g. $\gtrapprox$30-50%. While we prefer not embarking on this for this paper, it is worth considering in further examining/qualifying the impact of clouds (and model clouds) on the UV Index.

It should be noted that the Cloud-J calculations do take into consideration cloud scattering and absorption that is wavelength dependent and also accounts for water droplet size and ice crystal type. Values are provided through the use of look-up tables.

We have not evaluated the accuracy of the model clouds in their resulting impact, characteristics, and coincidence of occurence relative to ground-based measurements (or even satellite base cloud measurements). We do not known the level of correctness of either cloud models. This is something of interest that is beyond the scope of this study.

P18, L10 I gather this answers my previous question about spectral impacts upon cloud amount. Or else the impacts are accumulated in the band coefficients.

The CloudJ calculations for cloud scattering and absorption are wavelength dependent using interpolated data obtained from look-up table parameters for water droplet size and ice crystal type.

P18, L22 There are only so many aspects of solar radiation that can be accounted for. Hopefully, these additional aspects are second order and fall within the error bars of the UVI values.

Hopefully, these geometry considerations do fall within the current uncertainty levels associated to the representation of spatially extended overhead clouds and their impact on the UV index. The treatment of water/ice clouds, particularly for non-uniform opacity and scattering within clouds, is one of the most challenging aspects to correctly manage within radiative transfer models.

Figure 11 The symbols and line need to be identified in the figure caption. The Y axis caption also needs to have ‘% difference’ in it.

Correction made.

Figure 13 Add ‘effective’ to cloud cover (ECC) in first line of figure caption.

Correction made.

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