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Atmospheric Measurement Techniques Discussions

Interactive comment on "Correcting spaceborne reflectivity measurements for application in solar ultraviolet radiation levels calculations at ground level" *by* P. N. den Outer et al.

Anonymous Referee #1

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This manuscript by den Outer et al., entitled "Correcting spaceborne reflectivity measurements for application in solar ultraviolet radiation levels calculations at ground level", investigates the use of Lambertian Equivalent Reflectivity (LER), from different satellite instruments, as an input to the cloud modification factor (CMF).

The topic is relevant and of interest to those attempting to estimate CMF at UV from satellite data. However, in my opinion there are two different types of major problems in the current form of the manuscript. First, it is essentially impossible to make a full review of the manuscript, since it gives rise to so many questions and thus leaves many parts unclear. Second, it seems that the authors should and could have done a much

stronger effort in order to understand and explain their results. The manuscript requires a major revision, before it can be considered for publication.

From the reviewer point of view, these two above mentioned problems are interrelated; since many parts were either unclear or confusing, without these details and information about what was done, it was not easy either to suggest clear improvements how to analyze the results in more depth. Therefore, my review below is necessarily a mixture bag of comments and questions.

About the Figures 3 and 4. You wrote that "we expect to observe linear relationships". Why should they be linear? You also wrote "Linear relationship is indeed revealed in the EPTOMS and NIMBUS plots". EPTOMS plot is indeed rather close to linear, still it has a hint of similar convex pattern, while NIMBUS is more clearly non-linear. You took the stand that these relationships should be linear and since they are not, you correct for the non-linearity. What I missed, however, was a discussion about what you believe you have to correct for: for errors/instrumental problems or for some real inherent differences between ground- and satellite-based approaches (or both)? Without that, it is difficult to see how the current analysis and discussion advance our understanding of the satellite-based CMF for surface UV estimates.

During days of broken cloudiness, could one get on average Fgb>Fsat (the latter does not account for the enhancement of diffuse irradiance from the cloud edges), which effect therefore should be most pronounced and visible around Fsat $\sim$ 0.5? You said that the relationship should be linear, but please explain the reasons in the text, and particularly the likely reasons why they now are not linear.

About the Figure 4. It would be interesting to know why OMILER is different to EPTOMS and NIMBUS. Now this became very confusing in many ways, since in the text you wrote "The grid size of OMILER, ..., cannot explain this observation", while in the figure caption you gave a contradicting statement "OMILER has a distribution that is more peaked, probably due to the initial 8 times better spatial resolution". Not only two and contradicting statements for OMILER pattern was given, but additionally the reader gets totally confused about the resolution of OMILER data that was used in the analysis shown in the Figure 4.

About the Figure 6. You wrote that the reason for SZA dependence (at large SZA) is that there are cases when RCF=0 that are due to the erroneous assumption of snow. Did you check that this was really the actual reason? Because now it sounds more like a speculation and one can think of other possible reasons too. First, you said (block 75, line 26) that RCF=1 is assumed for snow covered cases, so was not that the case also for RCF data that you used? If yes, then those cases of RCF=0 at large SZA (in the Figure 6) clearly cannot be explained by assumption of snow. Otherwise, please give a reference to confirm that in the data set that you used, RCF=0 instead is assumed over snow/ice covered surface.

You considered the issue of spatial resolution (in the Figure 2), while temporal resolution was not much discussed. Your Fsat is based on overpass conditions, while Fgb is based on diurnal UV data. I think this difference might play a role in some of the plots and would have deserved at least some discussion. For instance, let's assume that in the Figure 6 (particularly for large SZA cases), there are clear-sky conditions at the overpass time and moreover that Fgb, if estimated at the overpass time only, agrees perfectly with the satellite-based estimate. However, Fgb in your analysis is based on diurnal fluxes; it cannot be higher (than 1), but it can be lower in many cases when the entire day was not cloud-free. Moreover, OMI overpass time is not at noon, which influences most your Fgb, so this time difference can cause a more pronounced impact during these high SZA cases. So perhaps, this effect could explain at least partly the pattern for high SZA in the Figure 6?

I think the authors should have discussed also the other effects than clouds in their approach to estimate the surface UV. Now the following was essentially all the information that was given to the reader "... delivers the clear sky daily UV sum, based on ozone and local ancillary data like aerosol loading, ground albedo, height above sea level

etc." From where do you take aerosol loading (certainly different in Sodankylä, Finland than in Thessaloniki, Greece). How do you estimate the surface albedo in snow cover conditions? Now the possible influence of these other effects was not discussed and moreover the authors seemed to assume that all the other effects are perfectly taken into account, while the only issue that could cause any bias is the cloud effect. It is evident from the Figure 2, that the mean bias from site-to-site can range from 4% underestimation to 10% overestimation. So it was then a surprising approach to assume that the overall bias should be exactly zero; and if it is not the case, then the only possible reason is the CMF that was assumed. Block 76, line 20 gives a good example about this assumption (that one can simply ignore all the possible other error sources, both in the ground-based measurements and radiative transfer modeling inputs): "The best results were obtained ..., which was not anticipated".

I found the description of your "mountain ridges" approach rather unclear. After reading block 74 several times, it was not still very clear what was done, at least to me (not even the starting point: why did you have to "reduce the number of data points"?). Please try to clarify.

Block 77 is just one example of the statements that seemed vague to the reader. After a lengthy discussion about the corrections that were required, you wrote "and as it turns out, can be used without correction". It is not only that the discussion about the reasons for these corrections were missing, but the reader (who would like to understand them) is even more confused here: if they moreover depend on the algorithm versions, what was then the difference between the "earlier version of the LER algorithm" and the more recent one?

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