

Interactive comment on “Comparing OMI UV index to ground-based measurements at two Finnish sites with focus on cloud-free and overcast conditions” by M. R. A. Pitkänen et al.

Anonymous Referee #3

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Review of the paper “Attenuation of UV radiation by clouds in OMI UV product measured at two Finnish measurement sites”, by Pitkänen et al.

It is my opinion that the present manuscript is not in conditions to be considered for publication. This is due to several reasons as explain below.

I consider that the paper as a whole contains a poor contribution in this topic taking into account the already existent published paper about this subject.

The same title of the paper is confusing not clarifying the developed content. The sentence “attenuation of UV radiation by clouds in OMI UV product. . .” is not adequate

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because according to the abstract the objective is the validation of the OMI UVI index under different cloud conditions, certainly very well defined or classified. Also to indicate an OMI UV product is not a good idea, because it seems to indicate other possible products.

Besides, section 2.2.2 is really difficult to understand. Perhaps the fact that the same Brewer data used for the calibration method, it is also used for the evaluation of the calibration method makes the process very puzzling. I have read lines 130-160 as 20 times and I not sure if I have understood the process. Furthermore, the method seems not to improve the quality data for the case of broken cloud sky (see Figure 1b), which contains the main percentage of cases in this area of study. If I understood well, the ratio shown in Figure 1a, R/ROP, by definition uses the same BREWER UVI data in both R values, and can be simplified as the ratio of the radiometer signals ($SL501_{3min} / SL501_{\pm 30min}$); so, it does not represent the “re-calibration” of the data (if my interpretation is correct).

The UV index is as maximum 6, which certainly is very low considering the wide world application, although the paper is focused on clouds conditions.

Furthermore, the authors work indistinctly with different wavelength intervals: OMI (erythemal range), SL501 (erythemal range), BREWER (290-320 nm). I miss some discussion or a more detailed analysis about this topic. As far as I know, the differences can be below 5% (for certain conditions). But if the authors want to improve the quality of the radiometer data, this fact should be taken into account.

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