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Atmospheric Measurement Techniques Discussions

Interactive comment on "Retrieval of characteristic parameters for water vapour transmittance in the development of ground based sun-sky radiometric measurements of columnar water vapour" *by* M. Campanelli et al.

Anonymous Referee #1

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Review of the paper "Retrieval of characteristics parameters for water vapour transmittance in the development of ground based Sun-Sky radiometer measurements of columnar water vapor", by Campanelli et al.

General comments:

In this work, the authors describe a detailed methodology for the estimation of calibration parameters (three in this case: a, b and Vo) for solar-radiometers to determine the columnar water vapour. The methodology was already described and applied in Cam-

panelli et al., 2010, as the authors referenced. Also the authors say that the novelties or improvements of this new article are:

a) The application of a Monte Carlo Method for the evaluation of errors affecting to the "a" and "b" parameters b) The application of the methodology to an entire year of data

The paper cannot be published in the present form because substantially the methodology is the same, and a main part of the paper is repeated. The main problems in the application of the described methodology are not solved in this new issue. Certainly new data are incorporated, i.e, microwave data of water vapour, but the validation of the method is clearly not sufficient. The use of a year of data does not add any valuable improvement in the application of the method. In Campanelli et al., 2010, the parameter a,b and Vo are determined in a monthly basis, but that is not done here. Precisely this was proposed in that 2010 paper but this is not carried out here. Thus, what is the advantage of using 1 year of W data?

On the other hand the new methodology is proposed as an alternative to transmittance simulation in order to avoid errors of the simulation. However if the W values used in the determination of a,b,Vo have low quality, that would mean that the a,b,Vo would have low quality too.

It is obvious from the method that a, b and also Vo parameters are in some way dependent on the goodness of the water vapor data used for its determination. This reviewer does not agree with the use of the SHM method to retrieve water vapour values in the atmosphere, to be used in the calibration procedures. The data used for calibration must be of high quality and the SHM method does not provide them. The figure 3 is sufficiently illustrative to show the goodness of this type of data. If at a level of monthly means the disagreement with microwave values (or other type of data) is so high, the values used for near-instantaneous calibration, as it is the case of solar-radiometer, do not accomplish the required quality. Here, we come back to the unclear sentence about its necessity to "initiate the procedure" (see below).

Therefore, some points are not clear in the reported methodology.

1. Why the authors say that they need a previous data set of water vapour to initiate the method (In Campanelli et al., 2010, the authors say “at least a week of data”), in this case the SHT method or other valid data set. Reading the paper, they need water vapour data all the time because the method is an “in situ” method. Thus, what does it mean “to initiate the procedure”? Do they mean to validate?

2. In page 8078, paragraph between lines 5-10 in step (i) for the determination of b parameter. I cannot understand the method. The authors say that they need to form the pairs (x, y) and take the correlation coefficient to optimize the determination of the best b value. How is that made? It seems that only equation (2a,b) may be used to obtain the 3 parameters, a, b and Vo. Please clarify this point.

3. The authors remove when necessary those data that do not fit the requirements to make the results look nicer. This is not a regular way of scientific working.

4. Another problem is that the three parameters (a, b and Vo) are determined for 4 classes of W values. This also complicates the method, because now we have 12 constants of calibration instead of 3. What do the authors try to demonstrate with Figure 6? The dependence of a,b, and Vo with water vapor content? With fixed values of a,b, during a long period, will these variations be translated to the Vo values?. Is not the high variation of a and b for low values produced by the bad W values used for their retrievals?

5. The authors prefer the proposed method instead of simulations because of less associated errors, but the problem in the application of the method is not the associated error but the values themselves of the quantities to be retrieved.

To merit a new publication, I recommend at least a year of comparative work with the water vapour data provided by a Cimel instrument with the AERONET methodology, but at the same time applying the proposed method to the Cimel and PREDE instruments

simultaneously. Furthermore, high quality W data are necessary, such as radiosonde data and the most suitable values of GPS data for a definitive validation of the method, or at least to solve the problem now open in the proposed methodology. I encourage the authors because they are in the deep of the open problems of the method, but it is clear that for a final validation of the method they require a good database of W, provided today by high time resolution GPS data, among others.

Interactive comment on Atmos. Meas. Tech. Discuss., 6, 8071, 2013.