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Interactive Comment

Interactive comment on "Improved total atmospheric water vapour amount determination fromnear-infrared filter measurements with sun photometers" *by* F. Mavromatakis et al.

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General Comments

This paper describes an improvement in the derivation of integrated water vapour from sun photometer observations of the atmospheric transmission within a near-infrared wavelength band (˜940 nm) characterized by strong water vapour absorption. The improvement is obtained by better accounting for out-of-band (OOB) transmittance of the sun photometer filter. Such contribution is usually neglected. The authors show that this generates a bias on the order of a few percent, which can be avoided by using a modification they propose to the most commonly used technique. I recommend this paper to be published in ACP as a technical note, after some minor revisions. The following four paragraphs are general comments. Then some more specific comments on particular parts of the paper are given.

Atmospheric water vapour is an important topic because of its effect on the climate. Particularly, water vapour is a major greenhouse gas, and it is anticipated to produce a strong feedback on climate change brought by increasing concentration of anthropogenic greenhouse gases. In the last 10 years, rapid progress have been made on the determination of the integrated water vapour column using the wet zenith delay of signals measured by fixed GPS antenna. Other methods such as GPS tomography are even bringing some knowledge on the elevation profile, eventually 3D distribution, of water vapour in the atmosphere. In comparison, sun photometry is limited to measuring integrated water vapour column, and only when sun is visible, while GPS can measure water vapour at all times.

On the other hand, GPS-based determination of water vapour requires a complex modelling of EM signal detection by the antenna, and can prove difficult in special circumstances (e.g., low amount of water vapour). As a consequence, sun photometer-based determination of water vapour is interesting as a tool for validating GPS, and other method for determining atmospheric water vapour content such as satellite-based radiometry, as well as for measuring low integrated water vapour columns. For this reason, it is important to be as precise as possible in such determination, and this research by Mavromatakis et al. is a valuable contribution appropriate for ACP. However, it is mainly an advance in an already developed and published method and brings few new scientific results. I think this work should consequently be published as a technical note rather than a research article. This would involve shortening the document.

Sections that could be shortened include the introduction that can be made less detailed, especially the part emphasizing the importance of water vapour. Some shortening is also possible in section 2 (method) as mentioned further. Similarly, the discussion of results from other authors (Holben et al., Schmid et al, and Ingold et al.) can also be significantly reduced. Finally, the influence of many model parameters is described in detail, and this can also be summarized.

The paper is applied to a specific absorption band (˜940 nm); however, it may as well be applied to different bands. For instance, Ingold et al. (2000) also used the water absorption bands at ˜720 nm and ˜820 nm for determining total water vapour column. In these later bands, the absorption by water vapour is less important than at 940 nm, and errors linked to not accounting for OOB contribution may be more significant. It would have been interesting to also apply the improvement presented here to one of these two other bands, especially since Ingold et al. found significant differences between the determinations using the different bands.

Specific comments

The abstract begins by describing the work as a study of the effect of OOB contributions to the signal recorded by a sun photometer. It continues by describing the method used. It is only at the end of the abstract, in a single sentence, that it is mentioned that an improvement is introduced in the sun-photometer based determination of water vapour by reducing uncertainties produced by the filter OOB contamination. I think this latter point is the important one and it is the justification of the title. I would have given it more prominence in the abstract and have mentioned it first.

Similarly, at the end of the introduction, the authors explain that “the technique combines the experimental determination of band-averaged water vapour transmittance with preliminary theoretical calculations of the same to derive the desired total amount of water vapour along a vertical atmospheric column.” In plain words, this means that sun photometry only allows determining the loss of transmittance due to water vapour, and that a model is necessary to estimate the amount of water vapour that would be responsible for such loss of transmittance. This work improves such an estimate by including sun photometer filter OOB contributions in the model. I think it would be worth plainly mentioning it, in words similar to those I used, at the end of the introduction to give the reader a quick and plain description of what this work is about.

In the beginning of section 2 (lines 21-24, p 6116), the author indicate they are studying the effect of OOB transmission, in a paragraph which is redundant with information given at the end of the introduction (lines 8-12, p 6116). This can be suppressed. In general, there is a lot of information that is redundantly given in the paper, and that can be used to shorten the text. Similarly, Eq. 2 is not used in the text and can be suppressed as well as the sentence that precedes it (lines 21-22, p 6117).

Concerning the transition from Eq. 3 to Eq. 4, it is indicated that the left term in the numerator can be rewritten in a new form by assuming that absorption by gases other than water vapour is negligible (lines 14-15, p 6118). Actually, the assumptions that are really made are that 1) Rayleigh scattering, scattering by aerosol and absorption by water vapour and ozone are the only terms to significantly influence the transmittance; and 2) that the optical depths due to absorption by ozone, and Rayleigh as well as scattering by aerosol are about constant over the water absorption band. Actually, it is mentioned at the end of the paragraph, after Eq. 4, that these optical depths can be assumed constant over the water absorption band, but they actually are assumed constant, or they could not be extracted from the integration. In addition, it seems the assumption for the right part of the numerator is that this contribution is so small that it can be replaced by its upper extraterrestrial limit (0 optical depth), but it is not mentioned in the text. The text explaining this transition (lines 14-19, p 6118) should be corrected to accurately reflect the assumptions made.

On lines 9-12, p. 6121, it is mentioned filter coefficients depend on the model used. The order of such model-related uncertainty (typically the variations observed when replacing one model by another) should be indicated so that it could be compared with the variations brought by OOB contamination.

On line 19-29, p. 6121, the large variations of coefficient A of model 1 are discussed. One reason given is that the functional form is simple (do the author mean too simple?).

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Actually, the model 2 is even simpler; it is the same as model 1 with parameter A fixed to 0. However, the parameter B also shows more variations in model 1 than 2. What is the reason? This could be investigated and discussed a little more thoughtfully, especially taking into account the fit Chi-squared, which seems to be smaller at small OOB for model 2 than model 1, while the opposite seems to be true for larger OOB. This should lead to a discussion of goodness of fit. In addition, the last sentence of the paragraph "The fitting program optimally varies coefficient (A) to reduce the Chi-squared" is not useful. Any fitting program is supposed to do this.

Technical corrections

Line 13, p. 6122, I would replace "In Fig. 3 the development of the coefficients for Models 1 and 2 is shown" by "In Fig. 3 the variations of the coefficients for Models 1 and 2 as function of OOB contamination is shown".

Line 15-20, p. 6123, it is not clear that the results for the filter of Schmid et al. are commented with respect to results obtained for a Gaussian filter in this study. I would replace "Nevertheless, we tried several different OOB responses and it can be shown that coefficient C does decrease with increasing OOB level as observed" with "Nevertheless, we tried several different OOB responses and it can be shown that coefficient C does decrease with increasing OOB level as observed in Fig. 3".

Figures and Tables:

Figures 1 and 3: each panel can include more than one curve. I think the two figures could be merged in one figure with double y-axis (one on the left and one on the right). This would allow comparing the behaviours for different filter shape, eventually different water vapour absorption bands (although in the latter case the fit parameters may not be in the same range).

Table 2: The fit Chi-squared are very small. Typically one would expect Chi-squared on the order of one per degree of freedom. It would be nice to have an estimation of

the number of degree of freedom. (Can the simulation be considered as independent of each other? are the variation of the model input parameter independent?) If the number of degree of freedom is on the order of the number of model simulations per fit minus the number of fit constraints, the size of the Chi-squared would indicate that the model uncertainty is not correctly estimated.

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