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Atmospheric Chemistry and Physics Discussions

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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.

F. Mavromatakis et al.

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A comment by the authors:

1. **Please note that in line 15, p. 6118 the text "Eq. (4)" should be replaced by "Eq. (3)".**

2. **A typo was detected in Table 2 and has been corrected (Value of Coefficient B of Model 1 when OOB is equal to 50E-6).**

The referees' comments are in bold face.

Referee 1: Laurent Vuilleumier.

We thank the reviewer for his careful reading of the manuscript and his numerous suggestions. We agree on most of them and have improved the manuscript accordingly. Our detailed response on each comment follows.

“However, it is mainly an advance in an already developed and published method and brings few new scientific results.”

We do not completely agree on this. This is incremental research, like many of the research articles in reputed journals (JGR, AO, . . .) that have preceded it.

“I think this work should consequently be published as a technical note rather than a research article. This would involve shortening the document.”

This view is not shared by the other reviewer, to the contrary. Anyway, we have tried to do that as much as possible, and indeed applied some of the changes requested. However, reaching the length of a technical note proved to be impossible without removing essential discussions and findings, which would have ultimately killed the originality of the paper. Moreover, the two reviewers have asked for additions, which we think justified. We do not feel that the length of this article is too long compared to others that preceded it. (Most are 10–20 pages long.) Finally, by streamlining the manuscript per this reviewer’s comments, we are confident that the scientific value of our results and findings are up to par with those in the related literature.

“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.”

We have generally tried to summarize results wherever this was possible. However, additions were also applied following the referees’ comments.

“The paper is applied to a specific absorption band (˜940 nm); however, it may as

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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."

This would be interesting indeed, but would have increased the length of the paper significantly. From our own investigation and discussions with colleagues in this field we have concluded that (i) most sunphotometers (e.g., those from AERONET) do *not* have filters for water vapour bands outside of 940 nm, so that the reviewer's proposed method would be applicable only at a few sites in the world; (ii) the lower signal in these weaker bands than in the 940-nm band constitutes an experimental problem (lower signal/noise ratio); and (iii) the spectroscopy of these water bands is still in a state of flux. In any case, we agree that a separate study should be devoted to this subject.

"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."

We agree and modified the text accordingly.

"Similarly, at the end of the introduction, the authors explain that "the technique

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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."

We agree only partially, since the work is also about the effect of altitude, and about proposing an alternative to the existing transmittance models. We made this sentence clearer anyway.

"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."

We agree and modified the text accordingly.

"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)."

Text has been removed.

"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, scatter-

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ing 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."

We agree and modified the text accordingly.

"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."

We make no assumption for the right part of the numerator. In general, the numerator involves "terrestrial" quantities, while the denominator involves extraterrestrial quantities. Note that the right term in the numerator in Eq. (3) reads Integral[I$_o$ T$_F$ T$_D$ exp(-m tau)], while we simply rewrite the same term in Eq. (4) as Integral[I T$_F$ T$_D$] since I=I$_o$ exp(-m tau) (originally Eq. 2, now removed). We hope that this didn't lead to a misinterpretation.

"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."

Below these lines we state that the filter coefficients depend upon the atmospheric code used to relate Tw and W theoretically. The filter coefficient dependence on the specific model is not discussed in these lines, but later on. In Lines 7-8, p. 6120, we explicitly state that we will be reporting results obtained with the LBLRTM code only.

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However, we included a sentence about the uncertainty in the PW retrieval associated with the three possible models (paragraph 5, Discussion section). We also included a comment about the effect on PW when not taking into account the site's altitude (paragraph 8, Discussion section).

"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?). 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."

The parameter A may indeed vary by almost a factor of 3 but what is actually important is exp(-A) since Tw = exp[-(A+B*(m*w)ˆC)] = exp(-A)*exp(-B*(m*w)ˆC). In this case, exp(-A) ranges from exp(-0.0267)=0.974 to exp(-0.0762)=0.927, which represents a much smaller variation, of only about 5%. Both Models 1 and 2 are trying to account for a wide range of values for both W and Tw, and this is not an easy task. Under the conditions reported in Table 1, the LBLRTM code predicts very low transmittances under the assumptions used here, down to Tw=exp(-5.1)=0.6%. The low transmittances are mainly conditioned by the tropical atmosphere, with the highest water vapour content and largest zenith angles. The statistical software runs as a robust estimator and this explains the small variations in the typical chi-sq. values originally reported in Table 2. Because these chi-sq. values might introduce confusion and have never been reported before in similar contributions, we have taken them out of the picture and streamlined the text and Table 2 accordingly.

The variation in parameter B. Model 2 has only two free parameters and thus the coefficients B and C cannot vary by much since the shape of the T_w vs. W (=m*w) does not

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change substantially as Fig. 2 shows. Note also that when W=m*w=1, the C parameter does not affect the calculation of the transmittance (T_w), which is then primarily determined through the B coefficient as T_w=exp(-B). Since the data for different OOB responses do not differ a lot for relatively low W (e.g. < 10 as in Fig. 2), the B and C coefficients are not expected to vary a lot. Thus, the increasing contribution of high OOB responses can only lead to worse chi2 fits as our analysis has shown.

Model 1 has three free parameters and the statistical software varies them in order to obtain the lowest possible chi2 and simultaneously utilizing the maximum possible number of data. Under experimental conditions, the total recorded signal increases (for any given atmospheric profile and W) as a function of increasing OOB response and this implies that the sum A+B*WˆC must decrease accordingly. Again, for W=m*w=1 and given the fact that at low W the OOB effect is not as strong (Fig. 2), the sum A+B should vary little and with the tendency to decrease. As a result, the A and B parameters interact in a subtle way, and this is captured by the fitting software. However, the final effect (on Tw) is not as strong as the variation in the individual parameters would suggest.

The Table below (for W=1) and Model 1 shows that although parameters A and B display a certain degree of variation, the final result which is the transmittance (exp-(A+B)) varies much less. Of course, these results would be different for higher values of W.

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A	B	A+B	exp(-(A+B)) for W=1
-0.0317	0.7430	0.7113	0.491
-0.0267	0.7370	0.7103	0.491
-0.0294	0.7400	0.7106	0.491
-0.0327	0.7430	0.7103	0.491
-0.0416	0.7510	0.7094	0.492
-0.0542	0.7620	0.7078	0.493
-0.0660	0.7730	0.7070	0.493
-0.0762	0.7810	0.7048	0.494

To summarize: the text related to this point has been rewritten and moved to section 3.2 (last paragraph), which is of importance here. Despite the necessary additions, we have tried to keep the text as short as possible.

"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"."

Text has been rephrased.

"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"."

Text has been rephrased.

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“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).”

Figures 1 and 3 have been merged.

“Table 2: The fit Chi-squared are very small. Typically one would expect Chi-squared on the order of one per degree of freedom.”

The fit chi-squared are not very small. They are of the order of 0.7 and higher, i.e. very close to unity. Of course, smaller errors would increase the chi-sq., while higher errors would simply improve it.

“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.”

Details about degrees of freedom can be deduced from our reference to the approach described in Lines 3-24 p. 6119 of the original manuscript.

The degrees of freedom for each case results from the number of data points minus the number of fitted parameters. For a single fit, this would imply 3024 data points minus 2 or 3 (accounting for the number of free parameters and depending on the model). In the case of fits for any single aerosol value, we have 252 data points minus the appropriate numbers of fitted coefficients. Each simulation is based on a specific set of input parameters as described in Table 1 and is assumed independent.

The approach we followed is typical of the method used to determine filter coefficients.

Data are generated with a specific atmospheric code for a variety of atmospheric profiles and geometrical conditions and are subsequently fit with appropriate software to determine the corresponding filter coefficients.

Interactive comment on Atmos. Chem. Phys. Discuss., 7, 6113, 2007.

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