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Interactive comment on "MAX-DOAS observations of the total atmospheric water vapour column and comparison with independent observations" *by* T. Wagner et al.

T. Wagner et al.

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Reply to Reviwer #2

General comments This paper presents a unique method for retrieving the H2O column amount and H2O layer height from MAX-DOAS observations. These quantities are then compared with independent observations. The methodology is new and the subject of this paper is appropriate for AMT. However I am unconvinced with the authors' argument that the retrieval method does not depend on a priori information. Also, I identified some places that need much more quantitative discussions. After ad-

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Interactive Discussion

Discussion Paper

equately addressing these and other concerns described below, I recommend that this manuscript will be published.

Author reply: We thank this reviewer for the positive assessment of our manuscript. We agree that our statement on the use of a-priori information was misleading, and we changed the mansucript at several places. We agree that several assumptions made, e.g. on the H2O profile shape, constitute indeed a-priori information. The important point of our retrieval, however, is that it does not depend on explicit a-priori information for individual observations. We added this information at several places in the revised version (abstract, introduction, conclusions).

We also added a detailed error discusion in the revised version (new section 2.8). Before we give our detailed replies to the individual reviewer comments (see below), we give a short overview on several major changes of the revised version of our manuscript.

A) We implemented a new cloud discrimination scheme based on clearly defined conditions and quantitative thresholds. This scheme is similar to that presented in Wagner et al., 2011. Since no zenith view measurements are made in the current study, we used those made at an elevation angle of 70°. However, since the observed radiances and also the retrieved O4 DSCDs for this elevation angle not only depend on SZA but also on the relative azimuth angle, the calculation of the normalised radiance and O4 AMF is more complicated than in Wagner et al. (2011). Thus we decided not to apply a radiance threshold, because of the strong depenence of the radiance on the relative azimuth angle. For similar reasons we relaxed the threshold from 0.7 to 1 for the normalised O4 AMF. These changes affect the separation between 'thin' and 'thick' clouds. Because of the strong dependence of the radiance on the relative azimuth angle, we also slightly modified the discrimination scheme between clear sky and 'thin' clouds. Instead of investigating the temporal variation of the normalised radiance, we investigated the temporal variation of the radiance after applying a high pass filter to the diurnal variations for individual days. We expect that this change has only a small impact on the discrimination between clear sky and ‘thin’ clouds. Overall, the application of the new cloud classification scheme led to substantial changes of the classification results. The fraction of clear days increased from 20% to 38%, the fraction of ‘thin cloud’ cases decreased from 74% to 44%, and the fraction of thick cloud cases increased from 6% to 18%. The large change in the frequencies of clear sky and thin cloud cases is caused by the rather coarse criteria for the detection of cloudy situations: In many cases, when the discrimination scheme indicates clear sky conditions, the presence of clouds is clearly obvious from inspecting the diurnal variations of cloud sensitive quantities, especially the colour index. In spite of these differences, the results of the correlation analyses for MAX-DOAS measurements made under clear sky or thin clouds are only slightly affected by these changes. We introduce the new cloud classification scheme in section in section 2.7 (old section 2.6) and explain the resulting differences. We also added a new figure (Fig. 9) illustrating the different steps and conditions of the cloud classification scheme. We updated the correlation analyses (Figs. 14 – 17) for the new sub-sets for clear, thin cloud and thick cloud conditions.

B) We added a detailed discussion of the error budget in the new section 2.8. The total error is based on uncertainties of the spectral retrieval and the treatment of the atmospheric radiation transfer. In most cases the latter dominates the total error. We add a formula (new equation 8) for the determination of the total error of the H2O VCD and display these errors in Fig. 11 (old Fig. 10).

C) We added clear criteria for the selection of wavelength ranges and elevation angles. These are based on fit errors in both spectral ranges for the whole time series of measurements. Here, for O4 smaller values are found for the green spectral range, and for H2O smaller values are found for the red spectral range. For the selected combination of elevation angles (20° and 70°), the standard deviation of the derived O4 VCDs is smallest for the whole time series. We also added the respective information in sections 2.2 and 2.3. We added a new table showing the standard deviation of the O4 VCDs derived from the different combinations of elevation angles (new table 2).

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D) Detailed discussions of H_2O profiles and the variability of the layer height We added a figure showing the average diurnal variation of the H_2O layer height (new Fig. 19). A systematic increase is found for the combination of MAX-DOAS and in-situ observations as well as for ECMWF data. We also investigate the variability of the H_2O profiles over the measurement site based on ECMWF data (new Fig. 20) and the suitability of an exponential function to describe the height dependence. It is found that above the boundary layer, the exponential function fits well to the observed profiles. However, close to the surface, the H_2O concentration is typically smaller than the extrapolated values of the fitted exponential function. This explains the systematic difference (about 400m) between the fitted scale height and the layer height calculated according to equation 9 (in the resvised version). We discuss these finding in section 3.8 (in the resvised version).

E) We also added a new figure showing the average diurnal variation of the H_2O VCD found in different data sets (new Fig. 13). A systematic increase of the H_2O between 7% and 11.4% is found for a period of about 12 hours.

Specific comments The authors' claim that their retrieval does not depend on a priori seems unfair. While it is unclear to me what assumptions are actually made, at least it seems that a fixed exponential shape of the H_2O vertical profile is assumed (for example, as systematic differences are discussed with the fixed H_2O vertical profile in the sections 2.4 and 2.6). This assumption is identical to using a fixed H_2O vertical profile as a-priori information. In addition, since the fixed vertical profile is assumed, a possible temporal variation in the H_2O vertical profile has been ignored. An additional systematic uncertainty due to the variation of H_2O vertical profile in the real atmosphere should arise. These points should be discussed quantitatively in the paper.

Author reply: We agree that our statements on the use of a-priori information was misleading, and we changed the manuscript at several places. We agree that assumptions made, e.g. on the H_2O profile shape, constitute a-priori information. The important advantage of our retrieval, however, is that it does not depend on explicit a-priori information for individual observations. We added this information at several places in the revised version (abstract, introduction, conclusions).

I think that discussion should be more quantitative throughout the paper. For example, on p.6247 (L1-3), the combination of 20 and 70 degrees has been chosen as the resulting VCDs typically show the lowest scatter. However, it is unclear what this argument is based on. How significant are systematic differences between H_2O VCDs from this combination and the other combinations? Quantitative discussions should be made here based on the statistics for the entire time period of measurements from March to August 2011.

Author reply: We agree that the decisions on the chosen combination of elevation angles (and also wavelength ranges) should be based on quantitative measures. As discussed in point C) above, we added clear criteria for the selection of wavelength ranges and elevation angles based on the statistics for the entire time series of measurements and added the respective information in sections 2.2 and 2.3.

Also, on p.6253 (L9-15), the authors describe how to categorize the measurements. As stated in this paragraph (as "visual inspection"), the categorization has been made in a subjective way. I strongly suggest the authors introducing an objective way to fit more to quality of AMT. Specifically, the words "rapid", "smooth", and "strong" used in this paragraph should be replaced by quantitative phrases.

Author reply: As explained in point A) above, no zenith sky observations are available for our measurements. This complicates the application of the original cloud discrimination scheme introduced in Wagner et al. (2011). Thus in the revised version we applied a slightly modified version of this scheme. We also included a new figure (Fig. 9) illustrating the individual steps and conditions of this modified scheme.

In the section 6, correlation analysis is made using daily average values. It is expected, however, that the H_2O vertical profile varies with time, especially according to the change in the boundary layer height within a day. So, I think that it would be better to add discussion about the ability to detect such short-time variations by MAX-DOAS and other data.

Author reply: As discussed in point D) above, we added more discusion on the variability of the H2O profiles and the respective impact on the H2O retrieval in the revised version of the manuscript (sections 2.8 and 3.8). We investigated the influence of variations of the H2O profile on the retrieval using ECMWF data and found that most profiles (>95%) have scale heights between 1.5 and 3km. The resulting errors of the retrieved H2O VCD are <15%. We added this information in the new section 2.8. We also investigated the variability of the H2O profiles over the measurement site based on ECMWF data (new Fig. 20) and the suitability of an exponential function to describe the height dependence. It is found that above the boundary layer, the exponential function fits well to the observed profiles. However, close to the surface, the H2O concentration is typically smaller than the extrapolated values of the fitted exponential function. This causes the systematic difference (about 400m) between the fitted scale heigth and the layer height calculated according to equation 9 (in the revised version). We discuss these finding in section 3.8 (in the revised version).

Concerning the short term variability we added the following statement to section 3.8: 'In spite of this good agreement for the average values, the day to day variation of the layer height is often different in both data sets (r^2 = 0.13). For a few occasions, however, a strong diurnal variation of the layer height is simultaneously found in both data sets (e.g. an increase from about 1.5 km to 3.5 km on 5 May).'

p.6249, L5-9: The value of 1.25 is derived here. Does this derivation depend on the SZA and the H2O vertical profile? The latter is related to the above comments.

Author reply: While the dependence on the SZA is very small (a few percent) for SZA between 20° and 80°), the dependence on the H2O profile can be up to 15% (for layer heights between 1.5 km and 3 km). We added this information to the new section 2.8.

Technical corrections p.6243, L5-6: Would it be better to include the balloon borne

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platform to measure water vapour?

Author reply: We changed ‘airborne’ to ‘aircraft and balloon borne’

p.6245, L1-4: Would it be reasonable to include NO_2 in DOAS analysis for the wavelength regions used in this work?

Author reply: We made test analyses including an NO_2 cross section. The changes of the DSCDs fof H_2O and O_4 were typically <1%. We included the following statement to section 2.2. ‘Note that including also a reference spectrum for the atmospheric NO_2 absorptions has only a very small influence (typically <1%) on the results for O_4 and H_2O.’

p.6248, L15-16: The phrase "relative azimuth angles" should be "RAZI", as it is already defined earlier.

Author reply: Corrected.

p.6251, L11: Does the cloud bottom mentioned here correspond to the CTH in Fig. 8? I guess the CTH means the cloud top height rather than the cloud bottom height. In Fig. 8, perhaps "CHT: 6 km" should be "CTH: 6 km".

Author reply: Many thanks for this hint! We corrected the text (3, 6, or 10 km => 2, 5, or 9 km)

The caption of Fig. 7 should explain what the color indicates.

Author reply: Probably the Fig. 6 is meant here? There we added information on the colours in the figure caption.

p.6251, L27-29: The authors state here that a deviation can occur in the case of rapidly varying cloud cover. I expect, however, that this effect should be small, if the conversion to VCD is made using H_2O and O_4 DSCDs that are retrieved "simultaneously".

Author reply: The correction based on the retrieved O_4 VCD (Eq. 6) leads in general

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to more accurate H2O VCD. However, from radiative transfer simulations it follows that the deviation of the (corrected) H2O VCD from the true H2O VCD increases for cases of ‘complicated’ radiative transfer, e.g. for small RAZI, high aerosol loads or broken clouds. In such cases, also the deviation of the retrieved O4 VCD from the true O4 VCD increases. Thus the magnitude of this deviation is a well suited quantity for the error of the H2O VCD (caused by radiative transfer effects). We added this explanation to the new section 2.8.

p.6256, L21: Should "4 April" be "4 May", according to Fig. 11?

Author reply: Many thanks for this hint. We corrected "4 April" to "4 May",

p.6258, L19: Do the authors mean that the layer height is identical to the scale height? In the lower troposphere, how well can the H2O profile be regarded as an exponential shape? Adding quantitative discussions would be helpful for the readers to understand.

Author reply: Many thanks for this suggestion! As discussed in point D) above we investigated the variability of the H2O profiles during the measurements at Mainz based on ECMWF data (new Fig. 20) and the suitability of an exponential function to describe the height dependence. It is found that above the boundary layer, the exponential function fits well to the observed profiles. However, close to the surface, the H2O concentration is typically smaller than the extrapolated values of the fitted exponential function. This causes the systematic difference (about 400m) between the fitted scale heigth and the layer height calculated according to equation 9 (in the revised version). We discuss these finding in section 3.8 (in the revised version).

p.6259, L1-9: I cannot evaluate how good the agreement between the layer heights estimated from MAX-DOAS and ECMWF is. Please add discussions, for example, about uncertainties of respective estimates and the expected natural variability.

Author reply: We added the following text to section 3.8: ‘In spite of this good agreement for the average values, the day to day variation of the layer height is often different

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in both data sets. For a few occasions, however, a strong diurnal variation of the layer height is simultaneously found in both data sets (e.g. an increase from about 1.5 km to 3.5 km on 5 May). In Fig. 19 the average diurnal variation of the layer height derived from combined MAX-DOAS and in-situ observations as well as ECMWF data is shown. Both data sets show a systematic increase of about 300 m and 190 m, respectively, during the day. From the time series of ECMWF model data we find that the variation of the H2O layer height is typically <1 km during one day (for 85 % of all days during the MAX-DOAS measurements).'

Also, what applications do the authors expect to use the MAX-DOAS layer height product for?

Author reply: We added the following statement to section 3.8: 'The calculation of the H2O layer height can be used as a simple quality indicator of the MAX-DOAS H2O VCD measurements: systematic errors of the retrieval will directly lead to unrealistic average H2O layer heights or diurnal variations. In addition, improved future MAX-DOAS retrievals might allow to monitor the diurnal variation of the H2O layer height.'

p.6260, L1-23: Please state that the results are all based on daily average values.

Author reply: Done.

In Fig. 2, a result for an elevation angle of 15 degrees is shown. Instead, a result for an elevation angle of 20 degrees should be shown here as the H2O VCDs derived using an elevation angle of 20 degrees are mainly discussed in the paper.

Author reply: We replaced the fit results by those for a 20° elevation spectrum.

In Figs. 7, 10 11, and 16, I understand that text such as "17.3" represents a date in a form of "dd.mm". However, I was little confused, when I first saw these figures. I suggest revising the figures for the readers to read them more easily, for example, by using labels only for the first day of the month.

Author reply: Corrected

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In Fig.14, the left-top panel should use a range of x axis to be consistent with other panels (0 to 1.5E+23).

Author reply: Corrected.

Interactive comment on Atmos. Meas. Tech. Discuss., 5, 6241, 2012.

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