



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

## **Anonymous Referee #2**

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### General comments

This paper presents a unique method for retrieving the H<sub>2</sub>O column amount and H<sub>2</sub>O 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 adequately addressing these and other concerns described below, I recommend that this manuscript will be published.

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## 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<sub>2</sub>O vertical profile is assumed (for example, as systematic differences are discussed with the fixed H<sub>2</sub>O vertical profile in the sections 2.4 and 2.6). This assumption is identical to using a fixed H<sub>2</sub>O vertical profile as a-priori information. In addition, since the fixed vertical profile is assumed, a possible temporal variation in the H<sub>2</sub>O vertical profile has been ignored. An additional systematic uncertainty due to the variation of H<sub>2</sub>O vertical profile in the real atmosphere should arise. These points should be discussed quantitatively in the paper.

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<sub>2</sub>O 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. 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.

In the section 6, correlation analysis is made using daily average values. It is expected, however, that the H<sub>2</sub>O 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.

p.6249, L5-9: The value of 1.25 is derived here. Does this derivation depend on the

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SZA and the H<sub>2</sub>O vertical profile? The latter is related to the above comments.

Technical corrections

p.6243, L5-6: Would it be better to include the balloon borne platform to measure water vapour?

p.6245, L1-4: Would it be reasonable to include NO<sub>2</sub> in DOAS analysis for the wavelength regions used in this work?

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

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

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<sub>2</sub>O and O<sub>4</sub> DSCDs that are retrieved "simultaneously".

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

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 H<sub>2</sub>O profile be regarded as an exponential shape? Adding quantitative discussions would be helpful for the readers to understand.

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

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

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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 H<sub>2</sub>O VCDs derived using an elevation angle of 20 degrees are mainly discussed in the paper.

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.

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).

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

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