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Interactive Comment

Interactive comment on "Analysis of global water vapour trends from satellite measurements in the visible spectral range" *by* S. Mieruch et al.

S. Mieruch et al.

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We thank the referee for the critical and detailed view on our manuscript entitled "Analysis of global water vapour trends from satellite measurements in the visible spectral range". The comments initiated the authors to fruitful discussions which resulted in an improvement of the paper. In the following we will discuss all points, mentioned by the referee.

1. The method used by (Weatherhead et. al.(1998)) is adapted to our requirements and modified at two positions:

    - page 11770, Eq. (3) and line 3: η is introduced as an amplitude change, which is not used by (Weatherhead et. al.(1998)).
    - page 11770 and 11771: Differing to (Weatherhead et. al.(1998)) we use

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the discrete correlation function developed by Edelson and Krolik(1988) to calculate $Corr_{N_t N_{t-1}}$.

Thus, to describe the performed changes it is necessary to show the respective parts from the trend calculation. However we checked the theory part carefully and decided to remove Eqs. 9 and 10, because they are not necessary for the trend calculation.

2. With respect to some misleadings produced by Fig. 2 and its kind of trivial character, we decided to remove this simplistic view on water vapour trends. We will reject the four "scenarios", but retain the argumentation of scenario 2.

   We agree with the referees comment, that an interpretation of trends in other regions and over the ocean would be very interesting, but this task lies beyond the scope of our paper. As an example for a trend interpretation, a possible connection of deforestation and decreasing H_2O is discussed. Also the influences of local trends on the global trend is not in the scope of our paper. To calculate the impact of e.g. decreasing trends in Amazonia to the global H_2O distribution and trend an extensive analysis is needed, which is not the aim of this paper.

3. We think the colour scale we used is quite appropriate for our purposes. Trends around zero can clearly be seen in green colour, whereas negative trends are bluish and violet and positive trends are yellow and reddish. Furthermore, we need an additional colour to indicate non-significant trends in Fig. 5, where we used white.

   We agree with the referee that the Trenberth et al.(2005) - Paper is important and we will consider it in the revised version of our paper in the results section. There we will discuss differences and agreements.

4. Sec. 4.2 is the "statistical modelling" section and there is nothing said about the causes of the level shift. In Sec. 4.1 several general causes for level shifts are

itemised, here we do not mean our level shifts explicitly. We will improve the discussion on instrumental differences related to the level shift and the cloud issue (especially with respect to different resolution) in Sec. 3. Here we will more definitely point out, that there is in principle no level shift between both instruments (on a global mean), but locally there are level shifts. Then we will improve the two "considerations", and discuss the causes of the level shifts and refer yet to Sec. 5.3, where the statistical analysis of the level shifts is presented. Furthermore we will explain shortly how the retrieval algorithm handles clouds, but for more detailed information on the cloud issue with respect to the retrieval we refer to the respective literature.

Specific comments on the text:

- page 11762, Abstract: We will expand the abstract with the information about the magnitudes of the trends and the significant regions.
- page 11762, Introduction: OK, we changed the order of argumentation.
- page 11762, Introduction: The importance of an accurate knowledge of water vapour for climate models is given in the two sentences above. Water vapour is a greenhouse gas, shows feedback properties and is involved in chemical processes. A more detailed discussion on this issue is not needed for our paper.
- page 11762-11763, Introduction: We will add "at the surface". We use the citation from a meteorology textbook (Häckel(1999)) in the revised version.
- page 11763, Introduction: As the referee suggests, we will change to a more standard name for the water vapour in the revised version and use "H_2O column". Therefore we will list several water vapour units used in the relevant literature and state that we will use from that point on the unit "H_2O column". Furthermore, to be comparable to Wagner et. al.(2006) we will hold on to the unit g/cm^2.

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- page 11763, second paragraph: OK, changed.
- page 11763, second last paragraph: We will cancel “rain forests” and “deserts” and go with the tropics, medium zones and poles in the revised version.
- page 11764, numbered items: We have removed Fig. 2 and adapted the corresponding parts in the revised version.
- page 11764, last paragraph, second sentence: We do not say, that water vapour is the dominating factor for vegetation type, but that there does exist a link between water vapour and vegetation (Melillo(1999)).
- page 11764, last paragraph, last sentence: This is an error, we meant “supported”.
- page 11765, first paragraph: OK, will be removed.
- page 11765, second paragraph: We thank the referee for this information and cite Good et al.(2007) here.
- page 11765, on Fig. 3: We explicitly wanted to refer to the GOME instrument and added “for GOME measurements” to the sentence.
- page 11766: The best way of validation would be with independent data, but this is very limited on global scale. These kinds of comparisons have been performed by e.g. Timmermans et al.(2004) for a previous version of our SCIAMACHY data. Validations on a global scale were performed with SSM/I data for the oceans. ECMWF water vapour are modelled data based on various data sources and can fill the gap of landside comparisons. Thus there are of cause restrictions, however such comparisons can reveal new information on how to improve the data sets, model data as well as measurements. Trend calculations with model data can also give interesting results, but real data always contain information which is

not included in the model, therefore a trend analysis of real measurements is of utmost importance.

- page 11766: Most of the referenced papers of comparisons between SCIAMACHY, SSM/I and ECMWF were based on a previous AMC-DOAS data set. The present study uses the AMC-DOAS v1.0 data which shows an absolute offset with respect to the previous data sets related to updated HITRAN as described in Noël et al.(2007). Therefore we think, that it is not useful to put all the individual results in one table, because they are not directly comparable. The relevant issue for the current trend analysis is the quality of the version 1 data product, which is documented in Noël et al.(2007). For the combination of the data sets the most important factor is the -0.01 g/cm^2 $\pm$ 0.25 g/cm^2 difference between GOME and SCIAMACHY for the overlapping time (cf. page 11767 line 21).

- page 11767, citation of the Noël paper paper: Yes, a difference plot between GOME and SCIAMACHY measurements for the overlapping time period is shown. By now the paper is published, so there is no need to include the figure from this paper.

- page 11768, bullet 1: The effect of the diurnal cycle is quite small, more important are clouds and wind (fluctuations in the water vapour column), local events on fast time scales. Consider Fig. 8 in the paper. The level shift δ is narrowly distributed around zero, i.e. mostly there is no level shift. The 10 % and the 90 % quantiles are quite small and make up only -4.7 % and 8.0 % of the mean H_2O column (cf. page 11779 line 11).

- page 11768: The referee is right, we gridded the GOME data to $0.5° \times 0.5°$ by interpolating one GOME measurement to more than one grid pixel. This is for the advantage of merging GOME and SCIAMACHY measurements together, because SCIAMACHY provides such a good resolution. Clouds are considered by the AMC-DOAS algorithm, which in principle provides a cloud free water vapour

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climatology. We will add this argumentation to Sec. 2. For our used monthly means this should not be a problem because of oversampling. GOME and SCIAMACHY are only measuring during daylight, thus there is no data during night.

- page 11768, on data coverage: We will replace “poles” by “high latitudes”. As said in this paragraph, GOME and SCIAMACHY provide daily data with a global coverage of 3 and 6 days, respectively, thus resolving extra daily variability in a month is not possible. Monthly number of observation plots are not necessary for this study, because the GOME and the SCIAMACHY sampling differs relative constant by a factor of two. Sampling is quite regular over space and time, so we think the monthly means are representative.

- page 11768, on advantages: OK. We will mention, that GOME and SCIAMACHY cannot retrieve profile information from the nadir view geometry in the revised version.

- page 11769, first paragraph of 4.1: Level shifts cannot be corrected before merging the datasets together, because they are not known. They have to be estimated by the least square fit.

- page 11770, first line after Eq. 4: See Appendix Eq. A2. There are the ϵ_t calculated from the N_t, which are estimated in Eq. 5 by the difference between the data and the fit, and σ_ϵ^2 is the variance of ϵ_t.

- page 11770: We will replace “probably” by “likely” in the revised version.

- page 11770, end of second paragraph: Considering higher orders of correlation will not improve the method. We calculated autocorrelation functions for all global grid pixels and on a mean, the autocorrelation has droped down to zero at lag 2. We will mention that in the last sentence in the paragraph after Eq. 4.

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- page 11770, second last paragraph: The gaps are described in Sec. 3 on page 11768 from line 18. The gaps are usually small, except at high mountain areas and during the polar nights.

- page 11770, end of second last paragraph: Interpolation would be possible, but it would falsify the data. For example, the error of the trend Eq. 13 depends on the number of data points. Interpolation would simulate that we have more data than we really have. Furthermore it is not necessary, all used methods can deal with data gaps.

- page 11770, Eq. 6: N_t are estimated in Eq. 5 and σ_N^2 is the variance of N_t, page 11770, line 9.

- page 11771, Eq. 10: To reduce the theoretical part of the methods section we remove Eqs. 9 and 10 in the revised manuscript. However the used symbol in Eq. 10 is the calligraphic $\mathcal{S}$, whereas the seasonal component is denoted by the italic S.

- page 11772, Eq. 11: A_t is $Y_t - \hat{S}_t$, i.e. the observed data Y_t is adjusted from the seasonal component $\hat{S}_t$. A_t are the deseasonalised data. It is decribed on page 11772 first paragraph and we expand Eq. 11 by $A_t = Y_t - \hat{S}_t$ in the revised version.

- page 11772, after Eq. 13: If we would include a third instrument, we would have to introduce an additional level shift. This would practically be done in Eq. 1. Denoting the new level shift for the GOME-2 data with κ.

$$Y_t = \mu C_t + S_t + \omega X_t + \delta U_t + \kappa V_t + N_t\,, \qquad t = 0,\ldots,T\,, \tag{1}$$

  where V_t is a step function which is zero before the GOME-2 data and unity from the first point to the end of the GOME-2 data.

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- page 11773, paragraph after Eq. 13: OK.
- page 11773, paragraph after Eq. 13: This assumption comes from standard statistics (least square method): Suppose the simple trend model:

$$Y_i = \alpha + \beta x_i + \epsilon_i \,. \tag{2}$$

In the following a normal distribution will be denoted by $N(\mu, \sigma^2)$. The ϵ_i are independent and identical distributed (i.i.d.) random numbers with $\epsilon_i \sim N(0, \sigma_\epsilon^2)$.

Now the **assumption of a normal distribution** is applied, i.e. $Y_i \sim N(\alpha + \beta x_i, \sigma^2)$. With the least square estimators:

$$\hat{\beta} = \sum_{i=1}^{n} b_i Y_i \qquad , b_i = \left(\frac{x_i - \overline{x}}{\sum_{i=1}^{n} (x_i - \overline{x})^2} \right) \tag{3}$$

and

$$\hat{\alpha} = \sum_{i=1}^{n} a_i Y_i \qquad , a_i = \frac{1}{n} - \left(\frac{x_i - \overline{x}}{\sum_{i=1}^{n} (x_i - \overline{x})^2} \overline{x} \right) \tag{4}$$

where $\overline{x}$ is the mean of x_i. Since $\hat{\alpha}$ and $\hat{\beta}$ are linear functions of Y_i it follows that the least square estimators are also normal distributed:

$$\hat{\alpha} \sim N(\alpha, \sigma_{\hat{\alpha}}^2) \quad , \hat{\beta} \sim N(\beta, \sigma_{\hat{\beta}}^2) \tag{5}$$

Standard rules of statistics reveal the mean values α and β and the variances $\sigma_{\hat{\alpha}}^2$ and $\sigma_{\hat{\beta}}^2$. For clarification we expand the paragraph on the significance of the trends.

- page 11773, end of second paragraph of results: In the revised version we will include the discussion on Trenberth et al.(2005) who calculated H_2O total column trends from SSM/I data (no measurements over land) for the time span from

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1988 to 2003. Although, Trenberth et al.(2005) analysed a different time interval, several similar patterns to ours are observed on the global maps, e.g. negative trends at the east coast of Australia, positive trends in the south west Pacific, positive trends covering a band from the east coast of India over Southeast Asia until the open Pacific. Of course there are differences, for instance positive trends at the west coast of Peru spread far into the ocean seen by Trenberth et al.(2005), whereas our trends are also positive at the coast, but zero and negative in the ocean. However, the Trenberth et al.(2005) SSM/I trends are about one magnitude smaller than our trends, which is most probably due to the difference in the used time interval. Therefore an intercomparison of the North Atlantic region is quite hard. The trends seen by Trenberth et al.(2005) in the North Atlantic are up to 0.0125 g/cm^2 per year. In Fig. 4 in our paper, this trend is small and coded with green colour. Our trends in this region are near zero and non-significant, thus they are afflicted with a high error, which make them not enough confident to draw conclusions about increasing or decreasing H_2O. We suggest to discuss the significant trends on a 95 % confidence interval for more reliable results. We will discuss the Trenberth et al.(2005) paper in Sec. 5.1.

- page 11774, first paragraph: With the criterion of having at least 2/3 of the data points it is clear that no bigger gaps than 44 months can occur in a time series with significant trend. The 2/3 criterion takes only very few effect, mostly for time series at the poles. For example, at the north pole, no data is available in December and January. On other places, for instant at the equator it is possible that no measurements are possible in a month due to thick clouds. Thus there are missing months. Summarising, the more gaps are in a time series, the less is the number of data points, the higher is the error of the trend Eq. 13, the less likely is a significant trend. Furthermore, the 2/3 criterion protect for denoting a trend significant with few data.
- page 11774, second last paragraph: Since the reason for the level shift is an

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atmospheric effect, most likely due to the 30 minutes time delay and the variability of H_2O (cf. Seq. 5.3), and no calibration problem ,there is no possibility to correct for these shifts in advance, they have to be estimated from time series.

- page 11775, first paragraph: We hope to clarify the autocorrelation issue by noting, that we are talking about autocorrelation of the noise and if the noise contains systematics, the autocorrelation will be increased.

- page 11775, last paragraph: Agreed, El Niño is influencing the trends and we ask the question if it can be removed from data. If the dataset is long enough, El Niño should not be a problem, if not, as in our case, we have to investigate the influence, what we have done.

- page 11776, third paragraph: We do not say, that the difference in the trends from the global mean time series with and without El Niño are small. On line 7, we say that “The differences on the **global map** between the complete data and the data where we removed El Niño is quite small ...”. However the 0.14 % trend is not significant.

- page 11776: No, this paragraph is related to the global trends and it is important for interpretation of the global trends. We will try to insert a blank line.

- 11777, end of $[\delta, \phi]$: After a detailed discussion on this paragraph, we updated the argumentation and hope that this leads to a better understanding. Generally, removement of data is very critically, unless for having very good reasons. For the global data believe in the full fit applied to the complete data set without removing any measurements. We will support this statement by showing a global map of the differences between the significant trends calculated on the basis of the different data sets (with El Niño and without El Niño data) in Sec. 5.2. If you are interested quantitative in a single time series, be it for a single grid pixel or for spatial averaged data (like the whole globe), you have to look carefully into the

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measurements and check for potential El Niño influenced data. This is the case for the globally averaged time series, where a strong El Niño signal is observed. In this special case removing the El Niño data is required, and the full fit for the data with removed El Niño gives the best results.

- page 11779, end of second paragraph: In Sec. 4.1 several general causes for level shifts are itemised, we do not mean our trends specifically. As said above it is not the diurnal cycle, it is more attributed to local variability. As can be seen from the histogram Fig. 8, the level shifts are distributed around zero. For the cloud issue we have to refer to the AMC-DOAS algorithm which can be applied to partially cloudy scenes. We will discuss the influence of the clouds in Sec. 3 in the numbered items 1 and 2 in the revised version.

- page 11779, end of second paragraph: With "thick cloud covers" we mean high cloud fraction, however, we will remove this sentence in the revised version.

- page 11780, Conclusion: We do not draw conclusions about the human impact on H_2O, cf. page 11781 first sentence "The H_2O content is changing and the human impact on this in **not clear**."

- page 11780, Conclusion: We do not say, that the itemised anthropogenic interventions are the reasons for our observed trends. We just report on findings from other studies (deforestation, consumption of groundwater etc.) and our observed trends.

- page 11780, Conclusion: OK, removed.

- page 11781, last paragraph: The approach of Dose and Menzel(2006) is based on Bayesian statistics. They describe time series (from observation) in a functional form and calculate change point probabilities and rates of change for individual years, i.e. a non-linear "changing trend over time".

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- Fig. 1: OK.
- Fig. 4: We used the LaTeX scaling commands “\columnwidth” and “\textwidth” for the used figures. The ACPD style is a one-column document, hence both scaling commands give equal results and scale the figures to the textwidth/columnwidth. For this reason Fig. 1 is too large in the ACPD version and Figs. 4 and 5 are too small. In the final ACP version (which is a two-column document) Fig. 1 would be scaled to the columnwidth and Figs. 4 and 5 would be scaled to the textwidth, therefore all figures would have the same size. However, with respect to the ACP standard width of one-column figures of 8.3 cm, we will split each two-column figure to two one-column figures, all with the same size. As discussed above we will keep the used colorscale.

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