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Atmospheric Chemistry and Physics Discussions

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

Anonymous Referee #2

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The relevance of the paper on the discussion of climate change and the monitoring of trends in the total column water vapour is high. However, the paper can be very much improved in four points:

1. The repetition of the theory of the Weatherhead et al. paper is not necessary. The actual paper is not improving this technique it is only using it. So the parts on the method can be shortened or passed on to the Appendix. 2. The connection between the scenarios defined in the introduction and the results presented later is not convincing. For instance, it is not clear in the introduction if the scenarios are meant to be of global or local character. From figure 1 I would conclude its global character but then the question arise why should one consider no or decreasing trends. The connection to the scenarios is only given very briefly in the last section by stating that deforesta-

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tion leads locally to lower water vapour contents. An interpretation of trends in other regions and over the ocean would be interesting. Additionally, it is not discussed how such local effects are connected to the globally increasing trend. This has to be much clearer throughout the paper. 3. The presentation of global trend patterns is not very convincing. As wished as a technical correction the figures should have a colour scale that allows for easy detection of positive and negative trends. Far more important is that a discussion with findings from other trend studies, e.g., Trenberth et al. (2005) from SSM/I data, is almost missing. The only reference used is one to work of Wagner et al. (2005) who uses the same instrument. One big difference between the current paper and the Trenberth paper is for instance that trends over the North Atlantic are totally different. Here we see negative trends over the whole region from the Labrador Sea to Iceland whereas the Trenberth paper shows strong positive trends in this region. So what are the reasons for the differences? 4. The explanations for the reasons for the level shifts are not very convincing. In 4.2 different instruments and calibrations are mentioned but in 5.3 equator overpass times and cloud statistics are made responsible. Unfortunately, the whole issue of cloud detection especially in GOME data and its influence on sampling and trends is not discussed in the paper. This should be improved.

In summary the paper would benefit a lot from extending the discussion of the results and decreasing the lengthy description of theory used.

Specific comments on the text:

page 11762, Abstract: This should be more to the point, i.e., what trends have been found and over which regions are those trends significant.

page 11762, Introduction: The importance of water vapour for ozone chemistry is high but here we are dealing more or less with a boundary layer estimate. So change order of argumentation.

page 11762, Introduction: What is the importance for climate models? Expand here!

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page 11762-11763, Introduction: Please add “global mean temperature at the surface”. For the citation you should better use a textbook of Meteorology. This finding is not from 2005!

page 11763, Introduction: It would be better to use a standard name as Integrated Water Vapour Content in [kg m-2] instead of the not well defined H2O content.

page 11763, second paragraph: “warming of the atmosphere” better change to “atmosphere” to “troposphere”.

page 11763, second last paragraph: The description of the geographical distribution of water vapour is too simplistic. The IWV over the Sahara is almost of the same size as over Central Europe, it is not only a latitudinal distribution!

page 11764, numbered items: How is this scenario discussion motivated? It appears a bit out of the blue.

page 11764, last para, second sentence: 1. Water vapour content is a mean to describe climatic state, so what is the link? 2. Water vapour is not the dominating factor for vegetation type, its more precipitation.

page 11764, last paragraph, last sentence: What is the meaning of this sentence? The understanding is constrained? If you mean that processes are constrained then describe how!

page 11765, first paragraph: Why are this local effects discussed here? It is unlikely that GOME data can contribute here. Also flood forecasting is not relevant for this paper. Please clean up these parts.

page 11765, second paragraph: Work of Good et al. (2007) on SST trends also using the Weatherhead trend model indicated that the El Ninos can clearly be seen in the data but that they do not influence the trends significantly.

page 11765, on Fig. 3: The close connection of IWV and SST has already been

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shown by many authors, so give credit to original work instead of papers from your own community.

page 11766: “Sciamachy data have been validated with Ĕ and ECMWF.” Is it not critical to validate measurements with models? Then why not using the model data for all this studies?

page 11766: This section can be improved a lot by showing a summary table from the cited papers to quantify the information. What are the quantitative results of the comparisons? The reader can’t go back to 5-7 papers to look that up.

page 11767, citation of the Noël paper: Is a difference time series shown in the Noël paper? If not show it here, because a global annual mean can be good for many reasons.

page 11768, bullet 1.: Why do you think that 30 minutes difference in equator crossing time can cause a jump in the time series? This would require a substantial diurnal cycle in the water vapour.

page 11768, grid resolution: I think 0.5°x0.5° is inadequate for GOME. How do you bring the data to the grid, one GOME measurement for many boxes? What about clouds? What about day-night differences?

page 11768, on data coverage: During winter you can’t cover from 70° north or southward. I would not call this the poles. It would be good to include monthly number of observation plots for SCIAMACHY and GOME to demonstrate the different sampling. Can those instruments really sample the extra daily variability in a month?

page 11768, on advantages: The 25 years record is not so impressive as other systems look forward to 40 years, so stick to the most important advantage of the same retrieval over land and ocean. You may also say that you have the disadvantage of not having any profile information.

page 11769, first paragraph of 4.1: Why are “level shifts” between instruments not

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resolved before the time series are merged together?

page 11770, first line after equation (4): How is sigma_e2 determined?

page 11770, third line after equation (4): Replace “probably” with “likely”.

page 11770, end of second paragraph: Why are we interested in lag one and not two or three? Please explain better.

page 11770, second last paragraph: About what gaps are you talking here? Those should have been described in sections 2 or 3.

page 11770, end of second last paragraph: How big are the gaps? You may also use temporal correlation to interpolate the data.

page 11771, equation (6): How is sigma_N2 determined?

page 11771, equation (10): Use other symbol for standard error. S has already been used for the seasonal component.

page 11772, equation (11): What is At? What happened to the measurement vector Yt?

page 11772, after equation (13): What will happen if you add a third instrument with another “level shift”? Is this model still valid? This will happen if you include GOME-2.

page 11773, paragraph after equation (13): Replace “talking about probabilities” with “in a probabilistic sense”.

page 11773, paragraph after equation (13) : Why is the assumption of a Gaussian distributions of trends correct?

page 11773, end of second paragraph of results: How does this result compares to findings by other scientists, e.g., Trenberth et al. (2005)? Trenberth’s trends have different sign over the North Atlantic!

page 11774, first paragraph: Again, this consideration is hard to understand until you

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have documented where and how big the gaps in the time series are.

page 11774, second last paragraph: Still it would be better to understand the differences between the sensors and to intercalibrate the sensors beforehand.

page 11775, first paragraph, sentence: “For instance increasing H2O contents over a limiting time increase the autocorrelation.” I do not understand this, a time series that has no or a decreasing trend can also be perfectly autocorrelated.

page 11775, last paragraph: El Ninos disturb your trend calculation only because the time series is too short, see Good at al. (2007)

page 11776, third paragraph: The difference in trends of the series with or without El Ninos is 42% (from 0.14% per year to 0.20% per year). This is not small!

page 11776, starting with “To demonstrate”: Make a new subsection here.

page 11777, end of [delta, phi]: In what trend should I believe now?

page 11779, end of second paragraph: I do not understand this! In section 4.1 level shifts were attributed to instrument and/or calibration changes - here it is the different measurement time during the day. The time delay can only be responsible if there is a diurnal cycle as for temperature. As this may locally be the case I would not expect it for the global mean. Unfortunately, the whole cloud issue is not discussed here. GOME has a spatial resolution that it is hard to find any cloud free measurement. What is the consequence of non detected clouds on the algorithm results, the averages and the computed trends?

page 11779, end of second paragraph: What do you mean by “thick cloud covers”, high cloud cover or large optical depth or both?

page 11780, Conclusion: The human impact on water vapour trends is not studied in this paper. So you cannot draw conclusion about it.

page 11780, Conclusion: You can use the considerations on local effects like defor-

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estation, etc. as motivation for the study of water vapour trends but I doubt that this is the only reason for decreasing water vapour over the North West USA or the Amazon regions. Thus, unless you are sure that changes in the large scale dynamics are not responsible for changes in the water vapour one should be very careful in drawing this kind of conclusion.

page 11780, Conclusion: You should not explain your future work program in the conclusion. The work on correlation analysis between water vapour and other atmospheric variables is quite commonplace.

page 11781, last paragraph: What is meant by “more than one trend in a time series”? A distinct time series can only have one trend.

page 11781, last paragraph: What is meant by “changing trend over time”? Do you mean variability?

Fig. 1: Please add a figure showing the variability.

Fig 4: As said for technical corrections: This figures must be bigger and a colour scale that allows for a clear distinction of positive and negative trends should be used.

Literature:

Good, S.A., G.K. Corlett, J.J. Remedios, E.J. Noyes, and D.T. Lleqellyn-Jones, 2007: The global trend in sea surface temperatures from 20 years of advanced very high resolution radiometer data. J. Climate, 20, 1255-1264.

Trenberth, K. E., J. Fasullo, L. Smith, 2005: Trends and variability in column-integrated atmospheric water vapour. Climate Dynamics, 24, 741-758, 10.1007/s00382-005-0017-4.

Interactive comment on Atmos. Chem. Phys. Discuss., 7, 11761, 2007.