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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Assessment of small-scale integrated water vapour variability during HOPE" *by* S. Steinke et al.

Anonymous Referee #1

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Review of the publication 'Assessment of small scale integrated water vapor variability during HOPE' by E. Steinke et.al.

The publication is an extensive comparison of different sensors to measure the integrated water vapor content (IWV) in the atmosphere and uses those results to evaluate two models, ICON and COSMO-DE, in order to assess if they reproduce the variability in the IWV.

After a detailed comparison of the instruments and the models, including discussion of temporal and spacial matching, the authors proceed to investigate the representativness of the data. The authors discuss one day as an example of the variation of the water vapor content.

The strengths and limitations of the used sensors are discussed and examples of the effect of the filtering due to the limitations of some sensors are given. Most notable are the change of the distribution of the values of IWV and the significant change of the mean diurnal variation of IWV if only measurements during clear sky conditions are used.

Given the importance and problems of measurements of water vapor I consider this study important and worth to be published after the comments raised below are taken into account. The publication is well written.

General remarks:

Nothing about the sensitivity of the instruments in different altitudes has been said. Other instruments also measuring total columns, i.e. FTIR instruments in the TCCON and NDACC networks, have been investigated in this respect (e.g. Ostler, 2014 and Sussmann, 2013) and an introduction of a daily variation due to an altitude depending sensitivity has been found. The altitude depending variability has been traced back to the changing solar zenith angle and the different path of view. This is probably also true for the sun photometer which employs a similar viewing geometry like the FTIR instruments with a changing view path. Such an effect might (partially) explain the differences in the diurnal course of the instruments as shown in figure 2.

Why are BASIL measurements are not used to derive an IWV? Why are the BASIL measurements not compared to a radiosonde profile? This would be advisable, because BASIL measurements are used to explain the properties of the water vapor column, i.e. that it concentrates in the first 1.5 km. I understand the correlation becomes very low if the distance becomes higher, even small distances introduce a comparison error for the IWV, as the authors explain in their study. But is this also true for the free tropopshere?

I think the title does not quite reflect the content of the study. In my view it is an elaborate comparison of several instruments measuring the total water vapor content

of the atmosphere. In order to do this in high quality the variability of water vapor both temporal and spacial has to be taken into account. Examples that the water vapor content can vary quickly both temporally and spatially have been given elsewhere and are not new.

Specific:

Section 4.3 and Figure 9: Would COCMO-DE perform better if it would also be filtered for cloud-free conditions only, i.e. if only coincident values with the sun-photometer are taken into account.

In the summary, measurements of BASIL are not excluded from the statement, that all instruments compare well to each other. However, no IWV has been derived from BASIL measurements, the authors do even state so without giving a reason why it is not done. Either BASIL measurements should be included in the comparison of IWV or it should be made clear, that BASIL measurements have not been compared to other measurements.

Page 22864, line 15: I am not sure if the auto-correlation can be 'lost'. This statement seems a bit off-hand and should be more precise, especially if the information is there. The criterion of 1/e for not being correlated anymore seems quite arbitrary. If there are studies which justify this value, please cite them. Otherwise I would suggest removing or modifying the statement that the correlation is 'lost'.

Minor:

Page 22863, line 13: The statement that the '...high end tail of the distribution...disappears ...' is somewhat unclear. I needed some searching before I could match it to section 4.2. where this is investigated. I would recommend to change this to something like: 'the high IWV values are only measured during clear weather conditions on daytime' or similar.

Figure 2, middle panel: The MODIS values are hard to see, because there are so few

of them. I would suggest drawing them with a different, bigger symbol are increase their visibility.

Figure 3, lower panel: GPS and ICON colors are very similar, I had to look twice to be able to distinguish them

Figure 4: Please encircle the dots with a frame. Especially on the left plot their are to similar to the ICON values to be easily distinguished.

Figure 6: while I quite like this figure quite it is rather small. I would suggest to scale this figure at least to fill the page width.

Figure 8: I am not sure if I understand this figure right. I would expect 4 different bars indicating the 10, 25, 75 and 90 percentiles. However I only see two of them.

Figure 9: The shaded green area is barely visible on my print out. I would suggest to put a dashed line as a frame around it.

References:

Multi-station intercomparison of column-averaged methane from NDACC and TCCON: Impact of dynamical variability A. Ostler, R. Sussmann, M. Rettinger, N. M. Deutscher, S. Dohe, F. Hase, N. Jones, M. Palm, and B.-M. Sinnhuber Atmos. Meas. Tech. Discuss., 7, 6743-6790, 2014

First intercalibration of column-averaged methane from the Total Carbon Column Observing Network and the Network for the Detection of Atmospheric Composition Change R. Sussmann, A. Ostler, F. Forster, M. Rettinger, N. M. Deutscher, D. W. T. Griffith, J. W. Hannigan, N. Jones, and P. K. Patra Atmos. Meas. Tech., 6, 397-418, 2013

Interactive comment on Atmos. Chem. Phys. Discuss., 14, 22837, 2014.