Response to Anonymous Referee #1

Original References are in italic and with serifs.
Answers are without serifs.

We thank the reviewer for her/his valuable comments. We respond to all comments and modified the paper accordingly.

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:

***RC1:** 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.*

AR1: Indeed, measuring with the sunphotometer at low solar zenith angles in combination with high IWV values, which are more likely at low altitudes, could lead to transmission approaching 0 (Ingold et al., 2000). We added this to the instrument description. Furthermore, we mention the variation due to the changing path through the atmosphere in p. 22853, l. 3 and go more into detail in p. 22861 l. 26: “For the difference between the sunphotometer and MWR, a dependency on the position of the sun is found (not shown). In the morning and in the afternoon, IWV from the sunphotometer is smaller than from the MWR because here the sunphotometer measures under lower elevation angles. At noon it is the other way around.

This could be due to an inaccurate relative air mass (Eq. 1) used by the retrieval or saturation effects due to low elevation angles."

RC2: *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?*

AR2: BASIL is alone not able to measure the IWV because it can only provide profiles from a height of 50-180 m above ground up to a height of 3-8 km. To derive IWV the BASIL measurements must be combined with measurement from other instruments, for example microwave radiometer or tower measurements. However, these IWV measurements would not be independent. To make this more clear in the text, we added: "Due to its limited altitude coverage no column water vapor can be provided from BASIL measurements alone." Since the study focuses on IWV, the profiles are not compared.

RC3: *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.*

AR3: It is true that a large part of the study deals with the comparison of the numerous instruments. However, the instrument comparison serves the investigation of the IWV variability and not the other way around. Within this study we characterize the variability of IWV for different temporal and spatial scales and estimate the ability of different measurements to represent this variabilities and show e. g. that the microwave radiometer is the only instrument to capture the water vapour variability on time scales of a few minutes. Furthermore, the small scale variability is for the first time assessed with 156 m resolution runs by the new numerical weather forecast model ICON. As ICON will become operational in the next years we find it highly important to investigate how water vapor varies on these scales and whether the model is capable of resolving it.

Specific:

RC4: *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.*

AR4: Since COSMO-DE does not have the same cloudy cases as the measurements at JOYCE filtering with the sunphotometer does not necessarily lead to clear-sky-only cases in the COSMO-DE output.

RC5: *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.

AR5: See answer to RC2. We modified the text to make this clear: "Pairwise comparison of the IWV-measuring instruments with 15 min temporal resolution..."

RC6: *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'.*

AR6: We modified the sentence to: "Synoptic influence is mainly responsible for the fact that the e-folding time of the auto-correlation is approximately one half of a day."

Minor:

RC7: *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.*

AR7: We modified this to: "Secondly, clouds and broken cloud fields can cause standard deviations of IWV of over 1.5 kg m^-2 within time intervals of a few hours. These high standard deviations do not occur when only daytime clear-sky IWV estimates are considered (cf. Fig. 8)."

RC8: *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.*

AR8: The figure is modified accordingly (see Fig. 1 in the discussion).

RC9: *Figure 3, lower panel: GPS and ICON colors are very similar, I had to look twice to be able to distinguish them*

AR9: The figure is modified accordingly (see Fig. 1 in the discussion).

RC10: *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.*

AR10: The figure is modified accordingly (see Fig. 2 in the discussion).

RC11: *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.*

AR11: This is due to the layout of ACPD articles. In the final layout of ACP it will be larger.

RC12: *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.*

AR12: The lower and upper end of the thin bar indicates the 10%- and 90%-percentiles, respectively and the lower and upper end of the thick bar indicates the 25%- and 75%-percentiles, respectively.

RC13: *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.*

AR13: The figure is modified accordingly (see Fig. 3 in the discussion).

References:

Ingold, T., Schmid, B., Mäitzler, C., Demoulin, P., Käimpfer, N., Modeled and empirical approaches for retrieving columnar water vapor from solar transmittance measurements in the 0.72, 0.82, and 0.94 µm absorption bands, J. Geophys. Res., 105, 24327-24343, 2000.

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