

Paper by Späth et al.

This is a good paper and, to my opinion, deserves to be published. However, some important and substantial, but hopefully straightforward modifications are required before the final acceptance and publication of the paper.

General points

First of all I would like to point out that the paper has a demonstration goal, which is a noble goal, but it is really at the border of the scopes of this Journal, which is more focused on the "... studies investigating the Earth's atmosphere and the underlying chemical and physical processes... with general implications for atmospheric science rather than investigations that are primarily of local or technical interest". Among the Copernicus Publications options, a better choice would have been Atmospheric Measurements Techniques (AMT), which has a clear technical demonstration scope. I understand that the choice of the authors for ACP is related to the presence in this Journal of an ongoing Special Issue dedicated to the HOPE field effort and a paper describing the UHOH DIAL upgrades for the purposes of HOPE is certainly suited within this Special Issue. However, a certain effort has to be dedicated to modify the paper to make it more suited to the scopes of ACP. In this direction, I don't agree on a mere illustration of some measurement examples without any attempt to substantiate these examples with a description and interpretation of the observed phenomena. This is even more necessary as in fact, thanks to the described new measurement capability and this unprecedented observation capability, this can be carried out based on the availability of high resolution and small scale information on a variety of atmospheric parameters.

Additionally, while this is never explicitly specified in the text, the goal of the present paper is **not to present the UHOH scanning DIAL** of the University of Hohenheim, as stated in the abstract (as this was in fact already done in a variety of previous papers, based on the use of data collected during previous field efforts, e.g. COPS, 2007, DFG Transregio, 2009), **but to demonstrate the recently earned scanning measurement capability, especially the fast scanning**. More **fast scanning** measurements and emphasis on the achieved results should then be provided.

Specific points

Page 29061. Authors introduce the DIAL equation (2) starting from the Lambert-Beer's law which describes *the exponential decrease of the intensity of a beam passing through a gas*, with S indicating the transmitted power. I don't understand why here they are using the Lambert-Beer's law instead of the monostatic single-scattering lidar signal equation to derive the DIAL equation. While the final DIAL equation has the same form, the use of the Lambert-Beer's law is misleading as in fact elastic backscattering phenomena from atmospheric species (described by the single-scattering lidar equation) make the DIAL measurement technique possible. If the lidar equation is not considered it is not even possible to understand why it is necessary that the on and off line wavelengths are close.

Page 29065, line 20. Concerning the sentence "If nearby radiosonde measurements are available, we use these if the deviations to the standard atmosphere are significant." Authors should comment on the fact that, if simultaneous radiosonde data are used, the system can no longer be considered strictly self-calibrating."

Page 29067. Why you specify different wave-numbers and absorption cross-sections in both fig. 5 and table 1 if these different selections are not used nor tested in this work ? Has any of these different wavelengths been tested during the measurements ?

Page 29069. Authors write: “As discussed above, the overlap effects cancel (see Eq. 2) when the system is well aligned.” However, this is the first time in the paper they mention the overlap effects. Additionally, authors should probably better explain what they mean here. Shouldn’t the overlap effect be the same for both the on- and off-line signals as these signals are both directed to the same optical channel and detected by the same APD ?

Page 29070, line 16. Authors write: “In the following we show vertical measurements taken in autumn 2013 in Stuttgart-Hohenheim”. This observation period in autumn 2013 is considered several times in the paper and it is important for the purposes of the paper. Authors should include an explicit mention to this observation period both in the abstract and in the introduction.

Page 29071. Line 16. Authors specify that: “ $\Delta q/q$ is smaller than 0.1% up to 2500 m AGL (above ground level) for all cases; and smaller than 0.5% up to 3000 m AGL”. However, when I observe the figures in detail (may be I am misunderstanding something), I realize that the absolute error at 2500 m is in the range 0.25-0.4 g/m³ (see figure 9a), while the corresponding q values are around 4-5 g/m³. From these values, the quantity $\Delta q/q$ at 2500 m seems to be 5-10 %. Similarly, the absolute error at 3000 m is in the range 0.5-0.9 g/m³ (see figure 9a), while the corresponding q values are around 2-3 g/m³. From these values, the quantity $\Delta q/q$ at 3000 m seems to be 16-45 %. These values are in disagreement with those reported by the authors and are also quite large to achieve some of the scientific goals pursued by the authors (among others, see next point).

Page 29072. Line 2. Authors write “We get a relative noise error up to 2500 m a.g.l. < 1% for 10 s time resolution instead of 0.1% for 20 min resolution (factor 120^{0.5}).” However, unless I understand something wrong in these figures, the value at 2500 m for 20 min to be rescaled for 10 s seems to be 5-10 % (as specified above), which corresponds to 50-100 % for 10 s. This is a quite large error uncertainty. As the authors state that the 1 % error level “... is very important for turbulence measurements”, I think the authors should comment on the possibility to still use these measurements, which are characterized by a larger error, to infer turbulent fluctuations with an acceptable uncertainty.

Page 29073. Line 23. Authors write: “The atmospheric dynamics captured here are mainly due to the presence of gravity waves above the ABL top and breaking Kelvin-Helmholtz waves in the interfacial layer. The interactions of these processes has certainly influence on the water-vapor entrainment flux and will be a key topic of investigations in the future.” The presence of gravity waves above the ABL top and breaking Kelvin-Helmholtz waves in the interfacial layer is not so evident, especially for a non trained eye. This is one of those cases where some insights into the science of the observed phenomena would be very much appreciated. While a detail dedicated study could well be “a key topic of investigations in the future”, as authors indicate, a preliminary brief description is certainly welcome already now.

Pages 29073-29074. Again, if the real scope of the paper (to be integrated with scientific significance as specified above) is **demonstrating the earned fast scanning measurement capability**, as any other measurement capability illustrated in the paper had already been reported in previous papers, it is not clear why authors decided that: “measurements of the UHOH DIAL based on such fast scans are currently analyzed and will be presented later”. **They must be here because this is the only really new measurement capability illustrated and demonstrated in this paper.**

Additionally, also in page 29074, some details on the considered scanning strategies based on the earned could perhaps be provided. For example, you specify that: “Because of higher extinction of the online signal in the moist boundary layer, the noise in a horizontal profile is larger than in a

vertical profile. Thus we use for larger distances larger SaGo window lengths ΔR to reduce the noise and to keep the angle resolution.” It would be worth here to illustrate, based on this example, but probably considering also the experience gained from other measurement examples, what is a good compromise choice on the vertical/horizontal resolution that can be considered also based on the expected small scale vertical/horizontal variability of the observed parameters.

Page 29074. Line 21. Authors write: “While the humidity in the CBL and the layer around 1.5km is similar for both scan directions, the layer at 1 km shows larger differences between the scans but also within each scan.” This is another of those cases where authors could substantiate the illustrated measurements with a description and interpretation of the observed phenomena.

Page 29074. The Summary seems to have been written quite in a hurry. A reshuffle of it and better summarization of the paper content is required. For example, the goal of the paper appears only at the sixth sentence (In this paper we presented the DIAL setup and demonstrated the scanning capability with examples of slow scanning measurements observing the 3-dimensional water-vapor field and fast scanning backscatter measurements showing the aerosol structure in the boundary layer and clouds up to altitudes of 14 km.) Measurements uncertainties associated with uncertainties in the estimate of the absorption cross-section are reported with specific numbers at the beginning of the Summary even if at this point it is not yet specified that those number refer to the wavelength selection of the present version of the UHOH DIAL.

Page 29085. In the table, two configurations are considered: the first one with an average power of 14 W and an average Ti:sapphire output power of 2 W, the second one with an average power of 20 W and an average Ti:sapphire output power of 4 W. In the first case a 14 % conversion efficiency seems to be achieved, while in the second case a 25 % conversion efficiency is achieved. Could you please spend few words to make this more clear ?

As I have already pointed out in the frame of my access review, 60 % of the references present in paper are from the authors of the present paper. This is specially true for the citations related to the lidar activity. While I recognize to the authors of this paper an enormous scientific relevance and extraordinary outreach, I believe that having few citations from other groups in the field is an undeserved underestimation of the contribution of the rest of the scientific community. As I did in my first review of this paper, I invite the authors to try to further acknowledge this community by citing some milestone lidar papers from other groups. For example, when introducing the Raman technique for water vapor measurements, it is a bit unusual that the paper by Hammann et al. (2014) is considered. This paper is not even published (which makes it a provisional reference for this specific purpose) and, additionally, it is a paper dedicated to temperature measurements. Furthermore, this paper is already cited twice in the paper in other parts where this citation is much more suited, thus there is really no need to cite it also here. When introducing the Raman lidar technique for water vapor, other papers could be more properly be considered to report on state-of-art water vapour measurements (among others, Dinoev *et al.* 2013, Whiteman et al., 2006).

Minor points

Page 29063. In expression 5 the convolution product is not properly indicated.

Page 29065. The sentence in line 16 should read: “The sensitivities to temperature and pressure uncertainties increase with height while the sensitivity to humidity decreases.”

Page 29065, line 25. The sentence should read: “... are small compared to those due to temperature and pressure.”

Page 29066, Line 14. A different wave-number range was considered in page 29064 to correspond to the wavelength range 817.7 to 819.0 nm.

Page 29069. Line 23. Specify if 0.1 mrad is full angle or half angle.

Page 29071. Line 16. Spaces in between numbers and units seem to be missing: “ $\Delta q/q$ is smaller than 0.1% up to **2500 m AGL** (above ground level) for all cases; and smaller than 0.5% up to **3000 m AGL**”.

Page 29073. Line 1. The sentence should read: “For this purpose, we used the DIAL in configuration 1.”

Page 29073. Line 8. The following sentence is not clear: “Due to the position of the sun, only RHI scans in the direction of JOYCE or LACROS were possible in the afternoon and evening.” Probably here you mean: “Due to the position of the sun, RHI scans in the direction of JOYCE or LACROS were possible only in the afternoon and evening.”

Page 29074, Line 9, “... aerosol-laden air” should probably read: “... aerosol-loaded air”.

Page 29092. In the text of the legend you probably mean “data acquisition system”.

Throughout the paper sometimes you use a.g.l. and other times you use AGL. Please homogenize it.

Sometimes figures are referred to in the present time (e.g. “... are shown in Fig. 8.”) and some other times they are referred to in the past time (e.g. “... were plotted in Fig. 10)

References

Dinoev, T., Simeonov, V., Arshinov, Y., Bobrovnikov, S., Ristori, P., Calpini, B., Parlange, M., and van den Bergh, H.: Raman Lidar for Meteorological Observations, 2013: RALMO – Part 1: Instrument description, Atmos. Meas. Tech. **6**, 1329–1346, DOI:10.5194/amt-6-1329-2013.

D. N. WHITEMAN, B. DEMOZ, P. DI GIROLAMO, J. COMER, I. VESELOVSKII, K. EVANS, Z. WANG, M. CADIROLA, K. RUSH, G. SCHWEMMER, B. GENTRY, S. H.MELFI, B. MIELKE, D. VENABLE, T. VAN HOVE (2006). Raman Lidar Measurements During the International H2O Project. Part I: Instrumentation and Analysis Techniques. JOURNAL OF ATMOSPHERIC AND OCEANIC TECHNOLOGY, vol. 23, p. 157-169, ISSN: 0739-0572, doi: 10.1175/JTECH1838.1.