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Atmospheric Chemistry and Physics Discussions

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Interactive Comment

Interactive comment on "High-resolution atmospheric water vapor measurements with a scanning differential absorption lidar" *by* F. Späth et al.

Anonymous Referee #1

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General:

The authors claim to present some sort of a 'proof of concept' for DIAL (differential absorption lidar) boundary layer humidity measurement capability of their UHOH DIAL. The readers are presented with a number of colorful graphs, which suggest that indeed boundary layer humidity can be measured at suitable spatial and temporal resolution with this instrument. However, the discussion seems to suggest that this capability had been available before (see major comment #1). The paper therefore appears to be a mere 'tuning/optimizing exercise'. While this would indeed be valuable in principle, the obtained choices are not substantiated, nor is the impact of the choices on the instru-

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Interactive Discussion

Discussion Paper

ment's performance (major comment #2). The shown examples, finally, which would potentially be of interest to the atmospheric science community (i.e., the readers of acp) are not discussed at all so that they in fact simply demonstrate that the instrument is indeed able to provide the desired spatial and temporal resolution (major comment #3). Overall, the paper that has apparently been written in a hurry (with quite a number of minor deficiencies) appears to be a mere attempt to get some necessary preliminary work published.

Major comments:

1 What is new? In the introduction it is stated that 'The water vapor DIAL of University of Hohenheim (UHOH) is a ground-based instrument which allows for 3-dimensional measurements of water vapor fields in the atmospheric boundary layer with high resolution and accuracy (Behrendt et al., 2009, 2011, 2012). For instance in the COPS field campaign 2007 . . .' (p.3, l. 8ff). So it seems that already for some years this instrument (the value of which cannot be overstated!) and its potential to measure high-resolution (in time and space) humidity within the atmospheric boundary layer has been around. The authors should therefore make it very clear what the additional information is that they want to portray in this paper.

2 What quality improvement has been obtained? In Sections 2 and 3 some parameter choices are presented. With respect to the physical parameters that determine the absorption cross-section, the authors believe that HITRAN 2012 is better than the 2008 version (p.8, l. 25) – but based on what? For the wavelength selection, the authors assume constant temperature, humidity and pressure (!) – without any reasoning (p. 11, l.10) – and then 'we selected the absorption line at a wave number of 12 223 cm−1. ' (p.11, l.15). So why first presenting the whole paragraph when finally simply making a choice? (note that I am convinced that it is unavoidable to make a choice but my point is that both the DIAL specialist and non-specialist only need to be informed about the choice: the former because she knows about the necessity (and the odds) of the choice and the latter because he cannot appreciate the choice anyhow from

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the given information). In Section 3 (Setup) again a number of details and choices are given which are potentially interesting for the DIAL specialist, but certainly of no value to the atmospheric scientist. This starts with Fig. 6 (of which it is said that is 'shows' the difference between configurations 1 and 2 – but this is hardly accessible if one does not already know it anyway). More importantly, while a number of detailed choices are given (that again are of little value to the atmospheric scientist), the relevant question is not even addressed (nothing to say about being answered): are all these adaptations and modifications of any influence to the performance of the instrument in the boundary layer? And if so, what is this influence (is there, e.g., any gain in resolution – spatial or temporal, and how large is it?) In Section 4, finally, some quality assessment is presented (even if it does not assess the gain in data quality through the technical improvements). The authors claim that Δq/q is smaller than 0.1% up to 2500m (p.15, l. 16): if one uses the information of Figs 8 and 9, however, one easily finds that, e.g. at 500m, the absolute humidity amounts to some 6gm-3 and Δq to some 0.2 gm-3. So the relative uncertainty Δq/q is about 0.03, but this is not 0.03% (but rather 3%!). Correspondingly the uncertainty goes up to about 10% at a height of 2500m. It is not quite clear whether this error translates to the further error analysis (and the given numbers) on p.16, l.3. Certainly, however, the given 'mean' errors are at least misleading. Inspecting Fig. 10 one sees that the mean errors are only so small because positive and negative errors are cancelling (and a height range has been chosen over which they are appropriately cancelling each other). So, in fact the interesting quantity would be the MAE (Mean Absolute Error), which amounts to some 0.1gm-3 or some 3%.

3 Given examples. First of all, for none of the shown examples any mentioning of the situation is made – so Fig. 11 can be omitted. Second, the discussion of the figures is minimal. For figure 12 it goes as far as stating 'one can see fascinating changes in the boundary layer structures between the scans' ... Whenever it becomes interesting (e.g., where do the authors see gravity waves? (I don't see any), why (based on what – dispersion characteristics, wave length, shape?) do the authors believe to see breaking

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KH waves? (p.17, l.24) it is stated that this will be the subject of another publication. Similarly, for Fig. 13 the authors offer an insightful discussion of the type âĂŽThe WV field in the two scanning directions shows several similarities but also significant differences'. With the provided discussion (i.e., its absence), the figures indeed reduce to show that water vapor can be measured with this instrument at sufficiently high temporal and spatial resolution.

Minor comments:

P2, l. 8 âĂŽsurface-vegetation-atmosphere exchange': Throughout the paper data is shown that starts at 500m (AGL) or higher. Can it really be claimed to investigate surface exchange processes? P4, l. 10 Muppa et al is only in preparation – this is probably not a valid reference P4, l. 18ff how can papers published in 2006, 2010 and 2010 be used to reference the measurements performed in 2013? P5, l.20 '. . ..because of the derivation'. How can a cancellation occur due to the derivation? Do the authors mean 'derivative'? P6, l.2 . . ..which becomes significant for strong. . .. P6, l. 12 if the authors mean temperature etc. with 'these parameters', these parameters are actually variables. P6, l. 15 wavelengths P7, l.5 eq. (5) does not seem to uniquely define the two functions Lk and Gk. Also it must explicitly be made clear that the Lorenzian profile is denoted with Lk, while the Gaussian one is denoted Gk. Finally the notation *$|\nu$ must be introduced. P7, l.12 R is probably the gas constant for water vapor P8, l.8 what is 'possible radiation'? P8, l. 20 up to 4% includes up to 2% P9, l.16 In Fig. 4 it is not stated relative to what these sensitivities are calculated? If this should refer to the standard atmosphere it should also be stated for which state then the sensitivities are calculated. P9, l.22 what do the authors consider to be 'significant'? P11, l. 17 Different to Fig. 1. . .. P11, l. 26 for 1km a constant pressure might be an acceptable assumption, but for 10km? P12, l.2 I don't think the figure adds anything to the understanding. Nor does the entire paragraph on which laser, and which seed etc. are being employed. The average ACP reader will not profit from this information and for the laser specialist it is probably too little detail (therefore, there are the two references, Spaeth et al. and

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Wagner et al. given, I suppose). P12, l.22 ...much more easily to align.... P12, l.27 I don't see any two 'configurations' in Fig. 6 (also applies to p13, l. 19) P15, l.1 Fig 7 shows...: this is simply reproduced to show that the instruments works. P15, l.7 why a 20 min period? Can the authors substantiate this choice? (since the DIAL information is available at much higher frequency it would probably be instructive to use the time when the sonde had reached a certain height to choose the DIAL profile (i.e. the corresponding height range: does this yield any improvement?). P15, l. 16 this percentage is probably wrong, too P15, l. 18 noise levels....: for the elevated measurements only P16, l. 3 check percentages Eq. (14) I understand the factor of 2 in eq. (15) but not in eq. (14) P17, l. 6 OberservatorY P19, l. 9 check percentages Fig 1,caption 'were plotted' (if at all) → but it seems to be pretty clear that this was plotted if I see it on the figure (this strange way of explaining what we see is in many of the figure captions). Figure 3, caption ....different heights (colored lines, see inlet) ... Fig. 4 all labels much to small. Caption: → relative difference: with respect to what? → 'the main deviation...': this is not part of the caption Fig 7 black column? Fig. 9b. this is not percent Fig 11, caption: what do all the numbers refer to? (but the figure is obsolete anyway) Fig 12 no chance to read ANY of the labels. Also: what is 'partical backscatter'? should it be particle backscatter? Fig 13 labels way too small. Also: a) and b) show (not shows)

Interactive comment on Atmos. Chem. Phys. Discuss., 14, 29057, 2014.