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Interactive Comment

Interactive comment on "Temperature profiling of the atmospheric boundary layer with rotational Raman lidar during the HD(CP)^2 observational prototype experiment" *by* E. Hammann et al.

Anonymous Referee #2

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This paper describes the design and construction of a new lidar system used to profile the boundary layer during night-time and daytime, and presents some preliminary results and comparison with radiosondes. It is a carefully-written paper which with minor revision should be suitable for publication in ACP.

My most serious comment concerns the design of the tilted filter which allows the two different wavelength bands to be used for the second rotational Raman filter, depending on the background noise. This is the main result of the paper, but it is simply presented as a fact and not discussed. The calculations and results pertain to the particular lidar used in this study and it is not clear how they transfer to other lidars, and thus be of general use to the community. Basically, the question is whether this degree of complexity in lidar design is worth it, given the result in fig 12 that under conditions of (very) high background noise the high-background system only provides a really significant improvement in temperature error above about 4 km – below that the improvement is either <10 % or not an improvement at all (below 1 km). For a system designed for boundary layer measurements this does not appear to me to be worth it. A section in the concluding section discussing the significance of the results is required.

Minor points (mainly language corrections): l.40 'superior' rather than 'advantageous' l.65 '. . ... DIAL, whose self-calibrating property leads to. . .. . .' l.68 'system already contains' l.73 '. . .DIAL, from which . . . are presented in Muppa et al. (2014) and Spaeth et al. (2014).' l.75 'forecasts' l.79 'The area already had. . ..' l.81 '. . ...and thus provided . . .. data set of thermodynamic properties for the atmosphere.' l.84 Say by how much the site was elevated from its surroundings l.89 At this point you have not introduced the 'novel switch' so this sentence makes no sense to the reader. Better to just say 'It is the goal of this paper . . ..' l.92 I'm not sure what you're trying to say. Is it 'Except for two night-long measurements, the lidar was only operated during daylight hours when there was no rain or continuous dense cloud cover'? l.94 .During one day and one night, the lidar was pointing at a low elevation above the ground' l.95 '..started at the beginning..' l.96 '. . ... – and also . . ...' l.102 delete 'also' l.107 delete 'short time of'. This sentence contradicts line 333 where you say it takes 5 minutes to change the filter position. l.121 '. . .. assumptions about. . .' l.122 'Figure 2 shows how the . . .. depend on the temperature' l.124 'CWL2: one for . . .. (L) and one for. . ...' l.126 '. . .has a different slope . . .. formulae. . .' l.128 'we use mostly' l.129 '. . .RR!, and a and b. . .. .' l.132 '. . .measurements are made over a. . .. .' l.133 delete But l.138 'photon-counting' l.143 Seems to have been left over from a previous draft. Equation 1 has nothing to do with the symbols in equation 5. A proper explanation is required. l.147, equation 6. I don't see where the factor of 2 comes from. A gradient is calculated from (T1-T2)/(r1-r2). In this case T1 and T2 are independent measurements so the error in (T1-T2) is SQRT(deltaT1ˆ2 + deltaT2ˆ2), i.e. the errors

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are added in quadrature. If the gradient is evaluated over a small enough height range the two deltaTs will be the same – and equation 2 should have SQRT(2) not 2. If you have a different error calculation this should be fully described. l.165 Poisson l.169 citations are wrong here – these are in-line citations and should not be parenthesised. l.182 and 186 Nd-YAG laser l.237 photon-counting l.246 'To determine. . ..' l.248 'this scaling' l.250 '. . .. . ..signals, a temperature-independent molecular. . ..' l.252 '. . .as a reference' l.294 'During daytime, S . . .. for a well-designed' p.307 '. . . limited to 354.2 nm and smaller to ensure that the elastic signal is blocked for this channel' l.311 'optimum, and therefore..' l.342 'radiosonde at the same height' l.346 'present at higher altitudes' l.346. This throwaway explanation of the discrepancy in fig.9 is not acceptable. The fact that the performance of the setting is not optimised does not explain why the theoretical curve departs from the measurements, and 3% is a big discrepancy (it corresponds to several K). Perhaps the drift of the radiosonde away from the lidar is a contributor – but in that case why is the agreement so good for the low-noise case? Temperature in the free troposphere doesn't usually vary rapidly with horizontal distance (or radiosondes wouldn't be much use for weather forecasting). l.350 '. . . with the two settings. . .' l.357 'As expected. . .' l.364 night-time l.366 you are arguing here that the H setting shows less uncertainty at low temperatures than the L setting. I think I understand but you need to explain this better, perhaps with reference to fig. 6. l.366 'At low altitudes. . .' l.369 'an advantage' or 'advantageous' l.372 '. . .shows an advantage lies above 1 km unless. . ..' l.387 '. . . UTC, which can be seen. . .' l.393 'A high positive gradient indicates a temperature inversion.' l.395 why is it surprising? l.430 '. . .elevated above its surroundings by. . .' l.431 ' range to height' l.434 '..to the vertical' l.455 'commonly used' l.654 prism l.664 temperature-independent l.770 '..better performance for the' l.772 '..significant at lower. . .'

Interactive comment on Atmos. Chem. Phys. Discuss., 14, 28973, 2014.

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