Atmos. Meas. Tech. Discuss., 7, C4413–C4420, 2015
www.atmos-meas-tech-discuss.net/7/C4413/2015/

Atmospheric Measurement Techniques Discussions

Interactive comment on "A one year comparison of 482 MHz radar wind profiler, RS92-SGP Radiosonde and 1.5 $\mu m Doppler Lidar wind measurements" by E. Pschkeet al.

Anonymous Referee #2

Received and published: 4 January 2015

General comments

The authors have carried out a study to compare the wind observations from a commercially available Doppler lidar with those from a wind profiling radar and routine four-times-per-day

rawinsonde launches over the course of a year. The distinctive aspect of this study, which the authors highlight, is that the comparison was carried out using data collected continually for an entire year. In addition, the paper provides a very nice mathematical discussion of their method, including the calculation of uncertainties in the measurements. In particular, the articulation of data rejection based on R2 and condition number, is useful guidance for other users of these sorts of measurement systems.

The authors have fallen short, however, in taking full advantage of their data set. They highlight advantages of the lidar's sampling over 24 discrete azimuth angles, in contrast other systems and to the radar, which has four (not counting the vertical). However, the main focus and conclusion of the paper seems to come down to, in their

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words, "There is a general good agreement in the measurement statistics of [lidar and profiler] and thus confirms previous studies on this issue but on the basis of a much smaller data collection." The authors, with relatively little additional effort, could provide much more insight that would be helpful to users of these systems. In particular, their VAD calculation of winds, including data rejection, requires that the wind field be sufficiently uniform (indicated by high R2) across the scanning area of the lidar. Out of >17,000 possible profiles over the year, the lidar quality criteria were met for less than 10,000 at best. Moreover, their Figure 6 suggests more rejection under convective (daytime) conditions than at night. The question that should immediately follow is whether profile rejection arising from weather conditions introduces a significant bias in

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annual, seasonal, or other average winds that might be derived from the lidar using the VAD method. By using the complete concurrent radiosonde data set, the authors should be easily assess whether there are biases that arise from sampling that favors time of day (stratification) or particular classes of weather conditions. To my knowledge, this has not previously been done and it would be very useful.

There is also one use of terminology throughout the paper that should be corrected. The authors have used "inhomogeneous" or "homogeneous" to describe wind fields that vary or do not vary significantly across the lidar sampling area. In the boundary layer in particular, these terms refer to the spatial variability of statistics of fields, not to the variability of the fields themselves. Thus a perfectly horizontally homogenous convective

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boundary layer might well fail to pass the VAD criteria that the authors have established.

Specific comments

Page 11443, line 6: The authors may wish to use "droplets" rather than "clouds".

Page 11443, line 12: "PRF" should be defined.

Page 11444, line 1: This sentence seems to imply that the radar backscatter comes in part from particles. They should revise it to indicate turbulent (or small-scale) variations of temperature and humidity as the source of backscatter for the radar.

Page 11444, line 26: The authors should supply the typical rise rate of the radiosondes (4 m/s?), which would allow conversion of the sampling rate to a vertical resolution for this system.

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Page 11445, line 2: It is not clear why the authors chose to use an index for the azimuthal dependence of Vr but not for R or t. In this application, range and time are both just as discrete as azimuth.

Page 11446, line 7: The authors should state clearly why they added 1 to the SNR values.

Page 11446, lines 25-27: It would be helpful to know the nature of differences that actually result from the two different approaches to calculating the winds from the lidar.

Page 11450, lines 5-12: What is the source of the numerical values provided? Is it the full year of data? A subset for this example?

Page 11452, line 4: Is there an objective basis for the selection of 95% as the criterion for R2? The authors should state why this specific value was

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adopted.

Page 11452, line 12: "it was found. . ." On what basis? (This is related to comment above.)

Page 11453, lines 24-26: A gap size of 270° is what occurs in wind profilers and sodars operating in 3-beam mode. This statement could be read as invalidating that measurement method. Do the authors wish to say that?

Page 11455, section 2.2.5: Given the small number (two, I think) of lidar points that are re-gridded to the profiler, how do the authors justify a spline? This would seem to imply zero error in the lidar values. Why not a simple linear interpolation?

Page 11457, line 14: Suggest "data sets fall very close to the identity line. . ."

Page 11458, line 1: What are "minor" data pairs? Do the authors mean a small fraction of the data

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pairs?

Page 11458, line 14: "assuming that the RWP measures 'truth'. . ." This seems like a dubious assumption, given susceptibility of the radar to the same VAD issues as the lidar and known phenomena such as biases arising from wind shear within the radar sampling volume. Using the term "reference" rather than "truth" would be less distracting.

Page 11466, Table 1: It would be helpful to have a row providing the dwell period.

Page 11472, caption: "The latter ensures no more than a moderate degree. . ."?

Page 11479, caption: The caption is too long. This quantity of information should be presented in the text.

Interactive comment on Atmos. Meas. Tech. Discuss., 7, 11439, 2014.