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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. Pschke et al.

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Interactive Discussion

Discussion Paper

Final author comment for "A one year comparison of 482 MHz radar wind profiler, RS92-SGP Radiosonde and 1.5 μm Doppler Lidar wind measurements"

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The referee # 2 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

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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 words, "There is a general

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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 annual, seasonal,

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or other average winds that might be derived from the lidar using the VAD method.

RESPONSE: Thank you for these remarks, which we have taken into account in the revised version of the manuscript.

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.

RESPONSE: We fully agree with the reviewer and have indeed firm plans to address these points in a further study. We speculate that the RWP may

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have a very small, albeit measurable bias which clearly needs further attention.

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

RESPONSE: In general, the term homogeneity describes translational invariance of a field. In the context mentioned by the reviewer, it is applied to

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statistics of random fields, like mean values or covariances. It is no contradiction to use this term also for instantaneous fields. The reviewer clearly touches upon a very important question, namely to what extent it is possible to derive meaningful mean wind profiles from wind measurements of a single monostatic instrument such as radar, lidar or sodar. However, this would need to be investigated with additional data from simulations for it appears to be impossible to answer this question solely on an experimental basis. We do not think that our use of the term "homogeneous" is inappropriate as
we use it in the most general sense of its definition.

Comments related to specific lines:

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

RESPONSE: Thanks. We prefer to use the term particles since the term droplets would exclude ice clouds.

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

RESPONSE: Done. (page 5, line 21)

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.

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RESPONSE: We have included a reference (page 6, line 11) which elaborates on the two practically relevant atmospheric backscattering mechanisms for radar wind profilers, namely Bragg scattering from fluctuations of the refractive index at half the radar wavelength as well as rayleigh scattering due to sufficiently large particles (e.g. ice particles, precipitation,...).

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.

RESPONSE: The typical ascent rate of the radiosonde was 5 m/s, this information was added in the manuscript (page 7, line 9). Thanks !

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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.

RESPONSE: Corrected.

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

RESPONSE: The term "intensity"defined by 1+SNR is a more convenient parameter, which is meanwhile widely established.

Page 11446, lines 25-27: It would be helpful to know the nature of differences that actually result

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from the two different approaches to calculating the winds from the lidar.

RESPONSE This is an interesting question which clearly deserves to be further examined. However, this was not within the scope of our work since the intent was to derive a data processing as similarly as possible to the processing employed in the RWP. We speculate, however, that the method of averaging could improve the basic retrieval conditions with regard to homogeneity.

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?

RESPONSE: The values describe the uncertainties for the u, v, w retrievals from one 30 min averaged

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VAD scan (page 12, line 18).

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 adopted.

RESPONSE: We have revised the manuscript to make it clear, that this value is ad-hoc since it is based on the analysis of a number of seemingly representative examples (page 14, line 25-29).

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

RESPONSE: Please see above.

Page 11453, lines 24-26: A gap size of 270 is what

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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?

RESPONSE: Of course not, for this would be a misunderstanding. The wind retrieval in the example discussed is based on radial velocity data from within a sector of 75 degrees. In other words, the gap size in this case is 285 degrees. This sampling geometry leads to a condition number of 22 which illustrates that the wind retrieval is no more well-conditioned. Indeed, this case is a striking example of how errors in the radial data may amplify under such circumstances. In any case, the authors are of the opinion that a (minimal) three-beam DBS sampling configuration should be avoided for a greater

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robustness of the wind retrievals. This is in line with investigations that have compared the accuracy of 3-,4- and 5-beam DBS configurations. Please see *Ahoro Adachi, Takahisa Kobayashi, Kenneth S. Gage, David A. Carter, Leslie M. Hartten, Wallace L. Clark, and Masato Fukuda, 2005: Evaluation of Three-Beam and Four-Beam Profiler Wind Measurement Techniques Using a Five-Beam Wind Profiler and Collocated Meteorological Tower. J. Atmos. Oceanic Technol., 22, 1167–1180. doi: http://dx.doi.org/10.1175/JTECH1777.1*

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?

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RESPONSE: The results of the paper should not depend on the interpolation method used.

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

RESPONSE: Suggestion included, thanks !

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

RESPONSE: Corrected.

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

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phenomena such as biases arising from wind shear within the radar sampling volume. Using the term “reference” rather than “truth” would be less distracting.

RESPONSE: The revised manuscript now uses the correct terminology, that is "reference" instead of "truth".

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

RESPONSE: Information included in Table 1.

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

RESPONSE: We have modified both the

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manuscript and the figure caption for improved clarity.

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

RESPONSE: Thanks, this was modified in the revision. A detailed explanation has been added to the body of the text and the figure caption has been fully revised.

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