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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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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's # 1 general comments:

0. I suggest modifying the title to emphasize to better describe the content of the paper. As

written it appears to be about a comparison among all three sensors, and there is no hint that the purpose is to evaluate the utility of the wind lidar.

RESPONSE: Thanks, we have decided to follow the suggestion and therefore modified the title into: "An assessment of the performance of a 1.5 μm Doppler lidar for operational vertical wind profiling based on a one-year trial"

1. In both the abstract and conclusion, much stronger statements could be made presenting the quantitative result of the comparisons. The abstract only says the wind lidar is “a reliable

system”, but could say that it agrees with the RWP to within XX m/s. Similarly, the conclusion speaks of “general good agreement” and “confirms previous studies”, but would be stronger by numerically stating how well the measurements agree.

RESPONSE: We agree with the reviewer and haved added quantitative information on the verfication scores in the abstract. Generally, a numerical comparison of our error statistics with the results of previous studies would indeed be very interesting, however turned out to be difficult, due to differences in the length of the respective data sets, and due to differences in the verification scores used. We therefore restricted ourselves to emphasize the qualitative agreements.

2. The comparison of the lidar winds with radiosondes does not have much detail. Please provide more information and explanation. What is the typical horizontal separation of the measurements (as a function of height), since the balloon drifts away from the site? Is the RS92 wind data really at only 40 s time resolution? Its raw data should be at 1 s; please provide more information. What time difference and spatial difference are allowed when choosing which lidar wind profiler to compare with each radiosonde profile?

RESPONSE: More detailed information is now provided in the revised version of the manuscript at the beginning of Sect.2 (page 5, line 2-7 in the revised manuscript version) and in

Sect.2.2.5.(page 18, line 17-26 in the revised manuscript version). Concerning the time resolution, the number of 40 s is correct, see e.g. Dirksen et al. (2014).

3. Please describe earlier in the paper (introduction?) that processing choices have been made to most closely match the radar wind profiler parameters, in order facilitate this comparison. But in more routine operations there may be better ways to operate the wind lidar. This was not clear to me until p. 11446, lines 14-16.

RESPONSE: This is a good suggestion, in order to motivate our specific processing method we have

now added a sentence in the introduction of the revised manuscript (page 3, line 1-2) which explains the operational time resolution for the RWP. This motivates the particular sampling settings for the Doppler lidar.

4. The error estimates for u and v on page 11450 (line 12) and the text that follows are very good to know. However, they do not account for small scale random motions in the atmosphere (turbulence and thermals, for example). This should be made more clear; unless I misunderstand and the 30 cm/s precision from Halo includes these error terms?

RESPONSE: There is perhaps a misunderstanding. The value specified in Pearson et al. (2009) ($\sigma < 30\ cm/s$) is derived from a theoretical consideration (Cramer-Rao lower bound; CRLB) of the measurement noise which has been calculated using an approximative formula for the CRLB as suggested by Rye and Hardesty (1993). Here, the overall spectral width ($\Delta\nu = \sigma_{tot}$; see also Doviak and Zrnic (1993)) enters into the calculations and accounts for both the variability due to instrumental effects and the naturally occuring wind variability over the measurement volume. In particular, Pearson et al. (2009) uses an atmospheric broadening factor σ_a = 1 m/s for his error estimates. Therefore, the error estimate $\sigma <$ 30 cm/s also accounts for small scale random motions in the atmosphere. See also our added explanations in sections 2.2.1 (page 8, line

14-16) and 2.2.3 (Error propagation, page 13, line 3-6) of the revised manuscript.

Errors in the retrievals for u, v, w which arise if the retrieval assumption of horizontal homogeneity is not fullfilled (for instance within a convective boundary layer) are not estimated by this method. This is not necessary, since we use the quality control parameter R^2 in order to identify cases where this assumption is not fullfilled and flag the respective retrievals as invalid (see also Sect. 2.2.4 Quality Assurance)

5. Section 3.2 is the heart of the paper (at least based on the intent from the Title and Introduction). The section describes what was found. It would be very helpful to postulate

possible reasons where (even small) systematic differences are found. An example is the positive ME in the DLWR comparison above 1800 m. Why might this be, and why might the ME increase with increasing height?

RESPONSE: Indeed, the original title of the paper gives the impression that Section 3.2. is the heart of the paper, although a great part of the article focuses also on the retrieval method. That is why we have also changed the title as already written in the reply to question 0.
Possible explanations of the systematic differences are given in the revised manuscript in section 3.2. (page 21, line 12-18)

6. [This comment applies to future use of the wind lidar, rather than this paper]. Figures 4 and 5: The R2 threshold appears to eliminate data in some of the *most* interesting areas – e.g. where there is directional shear (750 m altitude, early in the day), and during the growth of the CBL when there are large vertical eddies. It would be a shame if so much data were filtered out. Better if measurements were made with less averaging to find data segments that are more stationary.

RESPONSE: We agree. Given the purpose of our investigation we leave this investigation for a future study.

7. Figures 10 and 11: The point-to-point variation with height of these profiles looks very large to me for data that is an average of a full year. I would expect this to be quite smooth. Is there an explanation for this? For example, are there really very few points in the average? Rather than discussing the precision of the wind speeds ("not shown"), it would be valuable to show the standard deviation of the data going into each altitude.

RESPONSE: Theoretically, the number of possible profiles for wind retrievals during the measurement period is 17.568. The number of valid profiles, however, for which a DLWR comparison could be made reduced to about 8000 profiles (see Fig. 8, purple line). Additionally the

number of valid data in the profiles decreases with height. That is why we have written: "To get almost representative statistical results for a 'one-year comparison' the comparisons are restricted to heights up to $\sim$ 2800 m for the comparison DLWR and up to $\sim$ 1300 m for the comparison DLRS, which guarantees that the sample size is > 200." In other words, the data higher up with a sample size of around 200 profiles can not be regarded as statistically robust. Concerning the point-to-point variations in the DL profiles we also refer to the explanations added in Section 3.2. (page 21, line 17-18) of the revised manuscript.

Comments related to specific lines:

p, 11440: line 21. "of course" should have - done commas before and after done

p. 11440: line 25. Is there a reference for the IEEE standard?; added

p. 11442: line 4. authors', rather than author's; done

p. 11443: line 6. "such as" rather than "like"; done

p. 11444: line 2. I don't think clouds are a significant source of backscatter at 482 MHz; wording revised and additional reference given (page 6, line:10)

p. 11446: line 2. Is there a reference for the manufacturer claim of the Streamline precision? This number is used later in your error analysis. It is important to know that it is correct.

RESPONSE: The manual for the Streamline wind LiDAR (unpublished) provides information concerning the Streamline precision. In particular, a figure similar to Figure 2 c in Pearson et al. (2009) shows the dependence of the precision as a function of the SNR. From this figure we estimated the approximate value $\sigma <$ 30 cm s^{-1} for SNR = -18.2 dB. The value for the threshold SNR (i.e. -18.2 dB) was confirmed in a personal communication with the manufacturer.

p. 11447: line 5. Directions; done

p. 11448: I think this page is saying that SVD is used, rather than standard least squares. It would be good to say this clearly.

RESPONSE: The used of the SVD in least square fitting of data should always be preferred for numerical reasons.

p. 11450: line 6. This is where we need to "trust" the manufacturer's claim of precision. Please provide documentation of this value if it is available.

RESPONSE: please see above

p. 11451: line 4. routinely should be routine; wording revised

p. 11451: line 13. scannnig should be scanning; done

p. 11451: There is an earlier paper that may be worth referencing, which describes a test for a horizontally homogeneous wind field. (Goodrich,R.K, et al, 2002 in JAOT vol 19).; reference included

p. 11454: line 2+. An azimuthal gap of 240 degrees seems very large to me unless the measurements in the remaining sector are very good. Based on figure 3, I wonder if a CN value of 3-5 would have advantages? The example in Figure 7 does not help me because there is such a large difference between the CN=3 and CN=22 cases shown. I would suggest using a different example.

RESPONSE: In the choice for the CN threshold (CN = 10) we followed the arguments of Boccippio (1995) and Wissman et al. (2007). So far, there is no clear answer to the question whether this is the best choice and it remains for future work to figure this out. Employing this value, however, we made the experience that this value is very effective in detecting erroneous retrievals. A good example is the 23rd profile in Fig. 4 for the wind speed. Here the "red pixel" at about 1.5 km height is obviously an outlier. Therefore we analysed the mean VAD-scan in more detail (see Fig. 7) and noted that that the fit of the radial measurements is nearly perfect with $R^2 = 0.98$. However, there is a large measurement gap of nearly 280 degrees in the radials, resulting in a condition number of CN = 22 (see also the plot on the right of Fig. 5), which

shows that the retrieval is clearly ill-conditioned. Comparing this VAD-scan with adjacent VAD-scans at lower range gates it becomes immediately clear that the condition of these is much improved with CN = 1 or CN = 3, but the turbulent wind field makes the fit more difficult ($R^2 = 0.88$ or $R^2 = 0.82$). It is likely that more radial measurements in the scan at about 1.5 km height would have resulted in a similarely unfavorable result for R^2 than $R^2 = 0.98$. This example clearly shows how the quality control parameters R^2 and CN work.

To give more structured and detailed explanations of Fig. 5 and Fig. 7, a comprehensive explanation has been added to Section 2.2.4. (page 17, line 11-27)

Concerning the question whether a CN value of 3-5 would have advantages we would like to refer to the plot on the right of Fig. 5. This plot shows that for the majority of retrievals the values for CN range between 1 and 4. So, applying CN = 3 (or 5) as a threshold would discard the majority of the retrievals for the whole day.

p. 11457: line 7. I do not think this wind speed precision is meaningful. As calculated, it is the expected precision if the same wind field was measured each 30 minutes for a year. However, the wind field is changing over the year.

RESPONSE: We just mention this value for the sake of completeness, because we also discussed the issues of measurement uncertainty in Sec. 2.2.1 and the issue of error propagation in Sec.

2.2.3.. The wind speed precision is just given as addtional guidance for uncertainty estimates.

p. 11458: line 5. Please clarify what is meant by "cyclic azimuth range"

RESPONSE: wording revised ("2π periodicity of azimuth")

p. 11460: line 2. While changing the PRF can move the unambiguous range to a higher altitude, there is a penalty in sensitivity (unless averaging time or other parameters are also adjusted). Weather radar's address this with tricks in the transmitted waveform. It may be worth making note of the sensitivity penalty.

A paragraph has been added to describe this

effect in Section 4 (page 23, line 2).

p. 11460: line 9. It would be helpful to describe the lower measurement altitude of the 482 MHz wind profiler sooner in the paper.

Done. (page 3 , line 10-17)

p. 11460: lines 12-17. It would be helpful to rewrite the paragraph, making the language more standard.; The wording has been revised.

p. 11461: I do not think Appendix A is necessary. These results for converting from wind components to wind speed and direction are well known, as are the propagation of error formulae.

RESPONSE: Agreed and removed.

p. 11462: I do not think Appendix B is necessary. Doesn't B4 simplify as $\sigma_{speed}/\sqrt{(N)}$? This is also a very well-known result that does not need to be presented.

RESPONSE: Agreed and removed.

p. 11466: Table 1. It would be useful to add more information to this Table. For example the unambiguous range of each remote sensor. The parameters used in finding the winds. The lowest measured range gate. Maybe others. Also, indicate the oversampling that takes place in the RWP.

RESPONSE: Table 1 has been extendend.

Additional information concerning the first range gate and the dwell time are given. The unambiguous range can be calculated from the PRF (see page 22, line 20).

p. 11468: Figure 1. I really like this figure.

RESPONSE: Thanks.

p. 11469: Figure 2. The caption indicates this is from two measurement periods? I don't understand this. Also, is this example representative of the lidar performance? This is important if the result (SNR-threshold) is to be used for all data.

RESPONSE: Section 2.2.1 of the manuscript (page 8, line 20) and the caption of Fig. 2 were modified

accordingly to address the questions of the reviewer.

p. 11471: Figure 4. Please indicate if the time-axis is UT or local.; done

p. 11476. Figure 9. Please use the caption to indicate the purpose of the red lines. Also, please clarify why the words “in principle” are used. ; done

p. 11479. Figure 12. Please consider a more simple way to present this information. This is a lot on this plot, and it is hard to interpret. Also, the detailed information in the figure caption would fit better in the body text of section 4.

RESPONSE: A detailed explanation has been

added to the body of the text and the figure caption has been fully revised.