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Atmospheric Measurement Techniques Discussions

Interactive comment on "Observing wind, aerosol particles, cloud and precipitation: Finland's new ground-based remote-sensing network" *by* A. Hirsikko et al.

Anonymous Referee #1

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The paper of Hirsikko et al. aims at the introduction of the Finnish meteorological remote sensing network to the community. In a first part (16 pages) the network and its instrumentation is introduced. The second part (10 pages) concentrates on the evaluation of the Doppler lidar performance.

The style of introduction of the remote-sensing network is appropriate. The scientific content provided in the second part is however weakly presented and lacks quantitative information. I don't hesitate to state that the content will confuse potential readers because the information given is so unspecific and discrepancies between the different instruments may - or may not (as the authors state) - just be explained by differences in

the data analysis/handling, meteorological conditions, or operational setup (which has nothing to do with the instruments alone). E.g., the authors use data collected during snow events to inter-compare retrieved horizontal wind speeds without quantifying the effect of the snow on the comparability (Pg. 7279, Line 29). Also, data is used that was obviously affected by a misconfiguration of the data acquisition software that led to the sporadic loss of profiles (or pulses) (Pg. 7275, Lines 20-24).

The paper yields a lot of information about measurement strategies and procedures. An example is section 3.3. about the cloud radar: The technical measurement procedure is discussed in detail but the meaning of the cloud radar in connection to all other instruments (e.g. Doppler lidars) is neglected. The authors should concentrate more on how all their measurement values can be used and evaluated together.

I propose to reduce the inter-comparison section to examples of quality-assured measurements (no precipitation bias, appropriate instrument position, no data loss due to wrong software setup). In turn, Section 4.3.2 can be extended to show the full applicability of the remote-sensing network. There, the authors only briefly describe the steps they took to investigate the described dust event.

Below you find the major comments:

- For every station the exact instrument list should be specified (which Halo type is located where?)

Pg. 7253: Line 18: - what is 'enough signal'?

Pg. 7256: Line 8: - 'taking place in atmospheric ...' processes? environment? In the atmosphere?

Pg. 7264: Line 5: - How is the depolarization ratio determined with the Halo System? How is it calibrated?

Lines 17-19: - Is 5-s resolution sufficient to do flux measurements in the PBL?

Pg. 7265 Lines 5ff: - Calibrating a 1.5um system with the method of O'Connor produces a large error, which has to be mentioned in the paper. It should be somewhat larger than 30%. What is the resulting error in the determination of aerosol backscatter coefficient?

- If the radar focus is moved during the measurement the beta value is rendered useless because this introduces uncontrollable effects on the range resolved overlap/sensitivity function. The shift of focus in section 4.1.1. is a good idea to increase the system sensitivity at low heights, but its effects on the backscatter coefficient are critical. Even if the focus shift is not used, it has to be proven first, that the heterodyne detector has a linear response function for all targeted signal strengths - especially at cloud bases where the system is calibrated. The HALO lidar is certainly good in measuring wind speed, but backscatter coefficient or extinction is simply out of the design-focus of this instrument. The range-dependent function of heterodyne detection efficiency is needed in order to convert the detected SNR into a backscatter signal, as it is usually dealt with in lidar business (see Henderson et al, 2005). It strongly varies as a function of atmospheric turbulence and focus setting.

Line 25: - The Raman method should also be appropriate for extinction retrieval at both wavelengths.

Pg. 7266 Line 13f: - How can the ratio of liquid/ice be determined from depolarization measurements?

Line 23f: - 10% is quite a lot of particle depolarization. Actually, only dust does produce higher particle depolarization ratios. Is there a paper that describes the decrease of the depolarization ratio of dust with aging? Or do the authors mean, that the dust is mixed with other types of spherical particles during transport?

Pg. 7267 Line 8: - Microphysical properties can only be derived for non-polarizing particles.

Pg. 7272 Line 14: - What is the standard focus of each Halo Streamline? Line 26: - What exactly is one ray? One processing cycle? Which steps - exactly - does it include?

Pg. 7275: Line 21: - Does the data acquisition loose single pulses or whole rays? How many? How does the amount of lost rays/pulses depend on the system setting (range resolution, time resolution, total measurement range). Some quantitative information would be very useful to the reader.

Pg. 7276: Line 28: - It would be very interesting to see how signals correlate after the calibration procedure of O'Conner et al (2004) was applied. See further discussion of this issue in the comments on Fig. 5.

Pg. 7277: Line 25: - What does 'Good agreement' mean? Correlation coefficients or histograms should be shown to quantify the 'good agreement'.

Pg 7278: Lines 6-10: - Is a comparison of the lidars still useful under such conditions? I doubt this.

pg. 7279: Line 21f: - "non-optimal positioning"? If differences in the retrieved data result from the instruments position, the comparison is useless und should be left out.

last sentence: - If falling particles (snow) can pose a problem for the intercomparison, then affected cases have to be checked by eye and left out, otherwise the intercomparison is useless. It should be possible to collect enough clear-sky cases. Most of the velocity deviations between the different systems also seem to be the result of improper data evaluation or missing time synchronization.

Pg. 7280 - The first paragraph mentions some general points but it remains unclear who is interested in which measurement values. Some citations of related work or a more thorough explanation are needed here. Also new scanning strategies must have a certain aim?

Section 4.3.1: - Figure 10 should have subfigures denoted. Otherwise it is not immedi-

ately clear which subfigure is dealt with each time Fig 10 is mentioned.

Table 2: - It would be helpful for the reader according to which publication/techniques the 'Applications' are realized. Or are they 'built-in' to the instruments? - Don't the authors use LWP and IWV from the microwave radiometer?

Fig. 4: - The comparison with cloud radar data (if available) would be nice to illustrate the amount of precipitation during that period.Fig. 7-9. Instead of scatter plots the authors should consider to plot 2D-histograms (density plots). The reader can not see how many measurement points are located in the vicinity of the y=x line. Maybe the correlation is much better than visible on these plots?

Fig. 5: - An uncalibrated, uncorrected signal should not be denoted 'beta'/backscatter coefficient. I would suggest to show the SNR. This would allow to identify differences in the sensitivity of the lidar systems. Also the normalized backscattered signal could be shown (set all Signals to 1 at a speficied reference height). This would nicely illustrate non-linearities between the different systems.

Other issues: - The Mira36 system usually is called a "Cloud Radar", not a "Doppler Radar".

Pg 7278, Line 23: What is 'near-horizonal'? Is there a quantitative value available?

Pg 7280, Lines 1-2: Is the line of sight given in azimuth or elevation direction?

pg. 7265 l. 4 "uncelebrated" –> "uncalibrated" ....further minor issues will follow in a possible follow-up review.

Interactive comment on Atmos. Meas. Tech. Discuss., 6, 7251, 2013.