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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.

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We thank reviewer 1 for the comments, which we considered very helpful. In the following we reply to these comments and describe revisions proposed in the manuscript.

Reviewer: 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).

Authors: We begin our reply with comments for the sections 4.1-4.2. Please notice that we have collected comments to certain figure, table or topic in one reply or have reorganized comments to be able to reply to one topic in subsequent answers. We have revised Sections 4.1-4.2 according to replies to specific comments below.

Sect. 4.1-4.2

Reviewer: 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).

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.

Authors: We have tested different ways to improve data acquisition and learned that there is a possibility to misconfigure data acquisition in the way that data will not be processed as expected but to our knowledge no loss of signal/profiles occurs. We think that it is important to discuss this topic in the manuscript. We clearly state that we have not used data that are possibly affected by misconfiguration of data acquisition in our quantitative analysis in Sect. 4.2 (AMTD manuscript page 7275, lines 23-24). To avoid misunderstanding, in Table 3 we have removed details of instrument configuration from the test periods that were not analyzed in detail in this manuscript.

Reviewer: 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).

Authors: We agree with this comment and have revised analysis and subsequent discussion in Sect. 4 accordingly. We start the section with an investigation of the raw signal from three collocated instruments in the first inter-comparison campaign. All three instruments had the focus set to infinity for the initial period. A single profile of raw signal from each instrument clearly displays that two instruments, no 32 and 33, compare very well, both in signal strength and in profile shape, and that the third, no 34, clearly has an incorrect focus setting. As no scaling was required for 32 to agree with 33, this implies that their calibration factor is the same. To check that infinity was the correct focus setting for two of the instruments, a range-corrected parameter analogous to PR2 or uncalibrated attenuated backscatter was then derived and compared with the attenuated backscatter profile taken from a collocated ceilometer. The ceilometer is expected to provide the correct shape for such a profile, although noisy, above the range where complete overlap of the telescope and laser beam occurs (very close to the surface for Vaisala CL31). Thus, the agreement between ceilometer and two of the Doppler lidar range-corrected profiles validates the telescope setting of focus at infinity. A new focus correction was applied to the Doppler lidar (no 34) that was found to have an incorrect focus setting. The analytical function used to calculate a focus correction for arbitrary focus values was provided by the manufacturer and is undergoing more detailed evaluation. Two methods were applied to determine an appropriate value for the focus: 1) apply a series of focus corrections using the analytical function for a range of focus values and select the value that provided a range-corrected profile with the closest correspondence to the other two instruments, 2) point the scanner head horizontally into an assumed homogeneous atmosphere and select the focus value for the analytical function that provided a range-corrected profile with the closest correspondence to a profile exhibiting a constant optical depth (e.g. Campbell et al. 2002).

After selecting a value of 1000 m for the focus, rather than infinity, and applying a scaling factor, the derived range-corrected profile for the third instrument (no 34) agrees well with the other two (32 and 33) throughout the entire range (within the estimated signal uncertainty), both for boundary-layer aerosol, and in cirrus. The necessary scal-

ing factor suggests that instrument 34 is somewhat more sensitive than 32 and 33, and can be directly utilized within the overall calibration factor. With the necessary corrections now applied to a larger dataset, inter comparisons over a longer period can now be performed.

We have removed those data that were affected by precipitation and clouds from analysis in revised figures 7-8.

Reviewer: 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.

Authors: We chose to show above described approach with corresponding figures in revised manuscript.

Reviewer: 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.

Authors: We agree that it should first be proven that the heterodyne detector has a linear response function. To first order, this has already been demonstrated (see the response to the comment above), in that focus corrections for two instruments with different focus settings have yielded the same profile within acceptable limits. However, it is certainly true that this should be investigated further, and acceptable limits defined properly, together with full uncertainties provided for any focus correction. This is the subject of a current campaign, and an EU COST Action. This will include full characterization of focus settings, and comparisons with other lidar instruments. A number of comparisons have shown already that the retrieved attenuated backscatter profiles from these particular Doppler lidars do agree well with other lidar systems and ceilometers, for an infinity focus setting.

Reviewer: 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.

Authors: This has now been answered in the above responses. We start with SNR, then show range-corrected signals (essentially uncalibrated attenuated backscatter). There is also a short discussion on the sensitivity. The signals have been normalized relative to each other as discussed earlier – this is in essence similar to setting all signals to 1 at some specified reference height.

Reviewer: Pg. 7272 Line 14: What is the standard focus of each Halo Streamline?

Authors: There is no standard telescope focus length for these instruments, as the choice depends on application. Typically, low-elevations scans and cloud applications are performed with the focus set at infinity, whereas boundary layer studies may prefer a focus set somewhere between 1000-2000 m. Our particular instruments allow the focus setting to change with scan type. We initially began with focus set to infinity for all scans, but now have different standard focus settings depending on scan type. This information is now included in the text (AMTD manuscript page 7272, line 16).

Reviewer: Line 26: What exactly is one ray? One processing cycle? Which steps - exactly - does it include?

Authors: We propose the following new paragraph in the manuscript AMTD manuscript pages 7272-7273:

"The instrument has user-selectable temporal resolution. One ray is then defined as a single profile obtained by accumulating all available pulses within the time period selected. Thus, given a selected time resolution of 5 seconds, and a pulse repetition rate of 15,000 Hz, one ray is obtained from the accumulation of 75,000 pulses. Computation of the velocities is performed in real-time. Additional integration across a number of rays is then possible to increase the sensitivity. This method of ray-integration allows a simple method for different scan sequences to accumulate across separately specified multiples of the selected temporal resolution; e.g. vertical stare recorded at 5-second resolution and wind measurements recorded at 20-seconds resolution. Selected temporal resolution and ray-integration is dependent on the site, since a humid marine location does not require the same sensitivity as a clean-air Arctic location for the derivation of winds."

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

Fig. 7-9. Instead of scatter plots the authors should consider to plot 2D-histograms (density plots). The reader cannot 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?

Authors: Table 5 displayed correlation coefficient, slope of linear fit and root mean square error for data shown in Figs. 7-8. Statistical values were displayed in the Fig. 9. In the text we have now further clarified that agreement is determined based on the statistical quantities and figures 7-9 revised based on this comment. Figures 7-8 are additionally revised by removing data affected by clouds and precipitation. An example of proposed revisions in Fig. 7 is shown in Fig. 1 of this reply.

In the figure wind speed and direction from Doppler lidar no 33 (a and c) and no 32 (b and d) were compared against those from lidar no 34. Color indicates frequency of values in each pixel (with width 0.5 x 0.5 ms-1 for speed and 5 x 5 degrees for direction) normalized by maximum number in a single pixel. Due to exclusion of cloud and precipitation, number of data points decreased and we averaged wind profiles over 15 minutes (instead of 30 minutes). Statistical parameters in Table 5 have been revised based on made reanalysis.

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

Authors: We have checked that each clearly scattered point results from times when wind direction changes suddenly. We hesitated leaving times of heavily changing wind direction out from analysis, but it would be justified if requested by reviewer.

Reviewer: 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.

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). - 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.

Authors: Non-optimal positioning means that Doppler lidar bearing was off by 12.3 degrees. This difference was taken in account in the data analysis. In the Table 6 we compared all available wind speed and direction data points (variable All in table) and data not affected by precipitation (variable No precipitation). We additionally checked if analyzing only data measured during precipitation (variables rain and snow) affected comparison. As Table 6 indicates, a small decrease in correlation can be observed

when data collected during rain is considered. However, statistical values for comparison of data measured during snowing do not show any change. We believe that showing this comparison, although more quantitative analysis of effect of precipitation is required in future, is useful for AMT readers because wind-profiling techniques assume that atmospheric conditions do not change in the scanning volume, which cannot always be satisfied and hence causes uncertainty in derived wind profiles. Of course, it is well known that wind speed and direction have spatial variability.

Reviewer: Fig. 4: The comparison with cloud radar data (if available) would be nice to illustrate the amount of precipitation during that period.

Authors: Unfortunately, we had problems with the cloud radar during this period and no data is available.

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

Authors: This sentence was revised in the following way: *"Lidar data from three near-horizontal (with 1 ° elevation) beam directions (91 °, 179 °, 196 °) were analysed."*

Other Sections

Reviewer: 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.

Authors: We agree with this comment and have included discussion of cloud radar applications at the end of Sect. 3.3. Cloud radar is important especially for the Cloudnet retrievals. Therefore, we moved and revised discussion of the Cloudnet scheme from AMTD manuscript page 7271, lines 13-17 to page 7269, line 14:

"Cloud radar observations alone provide a useful basis for cloud research (e.g. Tonttila

et al., 2011), however, the sensitivity of cloud radar to low-level liquid clouds can be limited. Cloud radar is a key instrument in multi-sensor synergetic-retrievals and analysis of clouds. As an example, Cloudnet (A network of stations for the continuous evaluation of cloud and aerosol profiles in operational NWP models) developed a scheme to quantitatively analyse cloud types, microphysical properties of ice clouds and drizzle flux, and cloud fraction, by combining data from microwave radiometer, ceilometer, cloud radar with radiosonde or model profiles of temperature and humidity (Illingworth et al., 2007). This scheme will be implemented at Sodankylä within the ACTRIS framework. In addition, the inclusion of Doppler lidar observations allows the investigation of cloud base and below cloud dynamics, and identifying whether clouds are coupled to or de-coupled from the surface (Hogan et al., 2009; Harvey et al., 2013). When clouds are coupled with the surface the inclusion of in-situ observations in the analysis is justified."

Reviewer: 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.

Authors: In this manuscript we had two aims: 1) to introduce our remote sensing infrastructure and show the potential of the network via case studies and discussion, and 2) investigate Doppler lidar performance and research potential. The latter aim is interesting for the AMT readership and is not previously published to this extent. As replied also to reviewer 2 and in addition to above mentioned new paragraph, we propose following revision in the manuscript:

Last paragraph of introduction (AMTD version, page 7257, lines 14-19) is revised: *"In this paper, we introduce Finland's ground-based remote-sensing network (Sect. 2), the instrumentation deployed, discuss the measurement strategies at each location and present selected case studies of research potential (Sect. 3). The Halo Photonics Doppler lidars are key instruments in the network. To our knowledge this is the world's first meteorological Doppler lidar network. Therefore, we also focus on the per-*

formance of Doppler lidars in challenging environments, by displaying results from two Doppler lidar inter-comparison campaigns performed in Helsinki, discussing the operational reliability (Sect. 4.1-4.2) and presenting case studies (Sect. 4.3). In addition, we discuss the research potential for a network of remote and in-situ sensors (Sect. 3 and 4.3)."

We have included discussion of instrument synergy potential in Sect. 3.2 Raman lidar (AMTD version page 7267, line 7): *"Indeed, Doppler lidar aerosol attenuated backscatter profiles were available up to 400-500 m during 06:00-10:00 UTC on this day, whereas the air was too clean for sufficient data quality during the rest of the day. A combination of water vapour and aerosol particle microphysical retrievals from PollyXT, together with mixing layer evolution and winds from a Doppler lidar enables a more comprehensive and detailed investigation of the aerosol and boundary layer than from either instrument alone."*

Sect. 4, the first two sentences were added: *"The strategy behind Finland's new remote sensing network is to co-locate an additional advanced instrument, such as a Raman lidar, cloud radar, or weather radar, with each Doppler lidar, where possible. Therefore, in this section we concentrate on evaluating the performance of the Doppler lidar and applicability via case studies."*

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

Authors: We had already provided Table 1 which contained detailed information of remote sensing instruments at each site. We agree that models of Doppler lidars should be in the table and have added them.

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

Authors: We have added the following paragraph in the Sect. 3.1:

" As standard, the Doppler lidar provides profiles of signal-to-noise ratio (SNR), un-

calibrated attenuated backscatter coefficient (β) and radial Doppler velocity (v). Post-processing then applies background and focus corrections to the signal and provides calibrated attenuated backscatter coefficient profiles, together with uncertainties in signal, attenuated backscatter and Doppler velocity derived using an approximation to the Cramer-Lao lower bound method (Rye and Hardesty, 1993) given in O'Connor et al. (2010). Data availability is determined based on SNR (after applying the background correction); the threshold being determined based on the acceptable uncertainty for a given application. For vertically-pointing data, our selected threshold of -21 dB for SNR is equivalent to an uncertainty of about 0.05 m s-1 for the Doppler lidar instrument. Lowering the threshold to -25 dB theoretically only increases the uncertainty to 0.1 m s-1, potentially still suitable for horizontal wind measurements, but may now be within the noise floor of the instrument."

The last sentence has been expanded upon elsewhere (response to 3rd comment on this reply) with reference to the sensitivity and noise characteristics of each instrument.

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

Authors: typo corrected.

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

Authors: Currently, there is no direct calibration method for depolarization ratio from such an instrument. The two channels share the same optics and are assumed to share the same noise characteristics and sensitivities. This must be tested by comparison with another instrument, such as Polly XT, which is ongoing.

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

Authors: No, this is not sufficient. But researchers measuring fluxes require infor-

mation on the turbulent mixing in the atmosphere (dissipation rate can be derived at 3 minute resolution or better from vertical velocities, O'Connor et al., 2010) or on the evolution of the mixing layer (10-30 minutes resolution from vertical stare, e.g. Barlow et al., 2011; Wood et al., 2013) or with varying resolution based on scanning data (e.g. Banta et al., 2006).

Reviewer: 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?

Authors: Uncertainty in the calibration is about 20 % when using liquid-water clouds.

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

Authors: We agree and have revised this sentence as suggested.

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

Authors: This sentence is revised in the following way:

"The depolarization channel (532 nm) allows separation of spherical and non-spherical targets since round water droplets give depolarization ratio of about 0 % and complex ice crystals value of about 40 %. A value somewhere between indicates a certain mixture of water and ice which the ratio can be estimated."

Reviewer: 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?

Authors: We meant that observed depolarization ratio is due to mixture of different particle types.

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

Authors: We have revised sentence as suggested.

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

Authors: We agree with this comment. We have an aim for each measurement strategy. We provide some of these aims in the revised paragraph:

" An advantage of the network is that at all our stations have a clear line-of-site of at least 90° in azimuth direction, down close to the lidar horizon. Vertical azimuth display (VAD) and range height indicator (RHI) techniques are used, along with different combinations of custom designed azimuth, elevation and temporal settings. In general our long-term aim is to develop new operational scanning strategies –and subsequent data-analysis methods– to be used with characterisation of ABL phenomena and meteorology, air quality monitoring, cloud physics and weather forecasting. As an example, we have started 24-beam VAD wind scanning with the aim of improving accuracy of wind profiles. In future, we may synchronise our wind measurement routines with other European sites. The paper by Banta et al. (2006) presents a technique to determine low nocturnal mixing layer heights from RHI scans. It is clear that other research disciplines such as wave or ice researchers (personal communications in FMI and UHEL), and energy industry (Calpini et al., 2011) would also benefit from information on temporal and spatial variation of surface wind field."

Reviewer: Section 4.3.1: Figure 10 should have subfigures denoted. Otherwise it is

not immediately clear which subfigure is dealt with each time Fig 10 is mentioned.

Authors: We agree and have made the suggested changes.

Reviewer: 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?

Authors: We have included the required information in the table.

Reviewer: The Mira36 system usually is called a "Cloud Radar", not a "Doppler Radar". pg. 7265 l. 4 "uncelebrated" –> "uncalibrated"

Authors: These were corrected as suggested.

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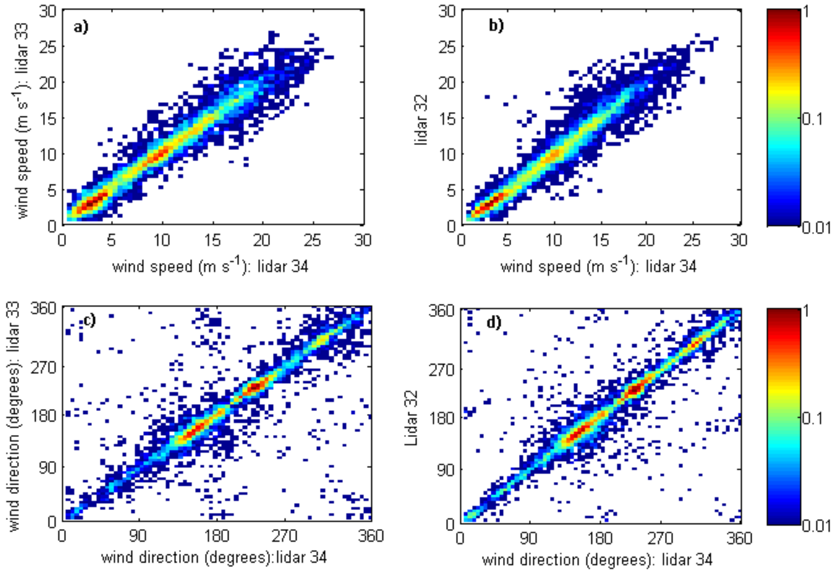

Fig. 1. Frequency of wind speed and direction occurrences during 2.-15.9.2011. Comparisons of lidars 33 and 34 (a & c) and lidars 32 and 34 (b & d) are shown.