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Atmospheric Measurement Techniques Discussions

Interactive comment on "Combined vertical-velocity observations with Doppler lidar, cloud radar and wind profiler" *by* J. Bühl et al.

Anonymous Referee #1

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Summary:

In this article Bühl and coauthors present two applications of the uncommon combination of an ultra-high frequency wind profiler (482MHz), a cloud radar (35GHz) and Doppler lidar (1.5um). Scattering theory is used to explain the strengths and complementary nature of the different systems. Two cases studies containing warm clouds, convective systems and mixed-phase clouds are presented. For these scenes, a coarse dynamic target classification is produced as well as fall velocity retrievals for both a warm and a mixed-phase cloud. Finally, the necessity of high-resolution observations in turbulent conditions is highlighted and the authors hint to potential microphysical applications of their fall velocity retrievals.

Assessment:

Overall, this article targets a very important topic in atmospheric science research. Vertical velocity observations/retrievals as well as fall velocity retrievals are key to improve numerical simulations. I appreciate the emphasis put on the description of the three systems in terms of the different targets they are able to observe and the different scales they can describe. Unfortunately, this manuscript contains major deficiencies surrounding its methods, the cross-validation/error quantification of its results and the assessment of its realistic applications. I consider this article as a comprehensive introduction to a future more complete technical manuscript. Therefore, I recommend rejection.

Major comments:

1) Unique nature of the observations P.355 L.18 The authors claim that the combined operation of high-frequency wind profiler, cloud radar and Doppler lidar is unique to the Meteorological Observatory Linderberg. Even though I acknowledge that the combination of these instruments has not been presented in scientific literature to this day, this setting is readily available at some sites operated by the U.S. Department of Energy (DOE) Atmospheric Radiation Measurement (ARM) program. This renders this dataset less unique and this should be noted.

2) Missing information about data quality and noise filtering The manuscript exploits data from wind profiler, cloud radar and Doppler lidar instruments. The quality of the measurements collected by these instruments is related to their signal to noise ratio, which the authors have correctly stated in the article. Yet, they make no mention of which technique is used to filter out noise for any of the systems. Doppler lidar is still an uncommon instrument and few articles present its data and even less explain how to distinguish its valid observations from noise. In my opinion, it is essential that the techniques used to filter each dataset be presented in details. In addition, to complement this, I recommend that the entire signal to noise ratio dataset be displayed for all

systems in figure 3. This implies to replace the panel f showing radar reflectivity by radar signal to noise ratio.

3) Lack of descriptions of figure 4 and figure 9 Section 3.1 P.358 L.6 Figure 4 is not discussed properly in the main text. However, more information and explanations are provided for figure 4 top panel in the figure caption. I recommend that this information be moved in the main text. Concerning the bottom panel, the spectra presented are very noisy and do not allow a comprehensive comparison of the three systems. It would be desirable to point to the main significant spectral features observed by each instrument. Then one should attempt to convince the reader that all three instruments capture the same low-frequency features and that the Doppler lidar allows the observation of unique high frequency features. Also, the article should discuss the implications of the -5/3 line included in the plot.

Same comments apply to figure 9

4) The limitations of each instrument are presented in a scattered way Section 3.4 P.359 L.19 It should be established early on in the manuscript that Doppler lidar extinguish quickly when large amounts of liquid droplets are present and as such they can only provide information about clouds at their base. This information should be found as early as Section 3.2 P.358 L.11 where the authors claim that Doppler lidars show a liquid cloud (In fact it shows the base of a liquid cloud).

Section 3.3 P.358 L.23 The limitations of wind profilers are presented for the first time in this section. The authors return to figure 3 to discuss the spectrum width enhancement due to the broad beam of the instrument and due to Rayleigh effects. This impacts the validity of the “air motion” observations provided by the wind profiler. I think such an important caveat should be discussed before any interpretation of the wind profiler data is made in section 3.1.

5) Section 3.2 P.358 L.15 It is suggested that the wind profiler measurements could provide a connection between gravity wave and thermal updrafts occurring between

10:05 and 10:10 UTC. However, this section is concerned with figure 5 which covers observation between 11:00 and 11:15 UTC. As such, I don’t understand which such a claim can be made; I do not see any supporting information nor explanation.

6) Classification of vertical velocity features Section 3.3 P.358-359 This section presents a classification of vertical velocity features. First it should be established that this classification is very coarse. Second, the criteria/characteristics of each structure are not presented. Also, I wonder if this classification was performed using an algorithm or it was done manually. If an algorithm was developed, I would like the authors to discuss its implementability for routine operation. Lastly, due to previously stated concerns, I doubt the claim of the authors concerning the use of such a collection of vertical velocity features for detailed modeling. Overall, I question the sophistication, operational reproducibility and value of such a classification.

Also, the novelty of this article is the integration of Doppler lidar data as a complementary source of information. My understanding is that Doppler lidars provide higher resolution measurements in the boundary layer and at cloud base. This strength is not exploited by this coarse classification. Also, because of the radars capacity to sense the boundary layer (if insects are present) and clouds and the capacity of wind profilers to sense the entire atmosphere, the added value the Doppler lidar is limited to cases with very wispy clouds that cloud radars cannot observe. As a result, I believe such a classification could be achieved without the Doppler lidar and the claim that three distinct instruments are necessary is an overstatement.

7) Terminal velocity retrievals Section 3.4 The authors mention that wind profiler data is a proxy for air motion and its magnitude is removed from both Doppler lidar and radar data to estimate particle fall velocity.

My first concern is about temporal and vertical range discrepancies between the three sensors. It is not explained how the coarse wind lidar data is interpolated to the high-resolution radar and Doppler lidar data. The authors touch on this subject in the caption

of figure 10 but a detailed explanation of this process belongs in the text along with its ramification in terms of precision.

My second concern is about the validity of using wind profiler data as direct air motion measurement. Previous studies (such as Protat and Williams 2011) have extensively discussed the need to remove Rayleigh effects and also the limited precision (0.2m/s) of these measurements. Such a discussion should be undertaken in the current manuscript as well.

My third concern is about validation. No validation or cross-comparison is offered for the terminal velocity retrievals.

8) Discussion about realistic applications P.361 L.3 In the conclusion and discussion section of the article, the authors claim that when connected properly these results may be useful for microphysical retrievals. Yet, no error quantification nor resolution limitation discussions are undertaken.

After reading this manuscript my understanding is the Doppler lidar can provide additional information about the dynamic structure of the boundary layer (when aerosols are present) and about clouds (at their base only). The new terminal velocity retrievals from Doppler lidar at cloud base could be valid only in non-liquid clouds, where turbulence is minimal and that only if the wind profiler vertical velocity is properly corrected for Rayleigh scattering. Thus, I suspect the retrievals would suffer from uncertainties perhaps of the same order of the retrieved terminal velocity of the non-liquid hydrometeors.

Interactive comment on Atmos. Meas. Tech. Discuss., 8, 353, 2015.