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Atmospheric Measurement Techniques Discussions

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Interactive Comment

Interactive comment on "Evaluation of turbulent dissipation rate retrievals from Doppler cloud radar" *by* M. D. Shupe et al.

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We thank the reviewer for the comments and feedback on this manuscript as they have contributed to important improvements in clarity and error analysis. The first two paragraphs of the review appear to be a summary of the more detailed points discussed later in the review. Therefore this response will focus on the very specific comments made starting in paragraph three and following to the end of the review. One point from the first paragraph does warrant a response - specifically the comment about reproduction of this work. It is the authors' intent to make this manuscript clear enough so that it can be reproducible by others, and attempts will be made to add clarity where needed. However, we feel that the basic elements of the retrieval are well described and the data that is required for the retrieval is well described. The only element that

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might be moderately unclear (which has now been corrected as discussed below) is the description of getting wind speeds on the same time-height grid as the radar data.

The first major comment deals with the two major assumptions in the radar retrieval that the reviewer feels are not fully justified. The reviewer suggests that an examination of example power spectra calculated from Doppler velocities during the case in Fig. 2 could be used to provide the needed justification. Power spectra have now been derived that do indeed demonstrate the clear presence of the inertial subrange, which supports the assumption that turbulence dominates the velocity variance. Moreover, these spectra demonstrate that the largest scales sampled in the retrieval reside within the inertial subrange such that the retrieval is not sampling turbulence scales that are beyond the inertial subrange. A new figure (attached as NewFigure2.png) has been included showing these spectra and a new paragraph of text has been added to the new section 2.2 describing this additional analysis. These additions have provided substantial support for the retrieval assumptions that are made.

Second, the reviewer suggests that there are two major sources of uncertainty in this type of retrieval technique that need to be better discussed:

Uncertainty #1: Estimate of Doppler velocity variance. A new section (2.2) has been added to the manuscript that outlines how noise in the Doppler velocity measurements contributes to the total velocity variance used in the retrieval. This closely follows the methodology outlined by O'Connor et al. (2010), to which the reviewer referred. Noise estimates are included on the power spectra in the new figure, showing that in high turbulence the signal is well above the noise, but under low turbulence conditions the high frequency end of the spectra are characterized by noise. The fact that the actual spectrum derived from the velocity measurements agrees with the computed contribution from noise in the low turbulence case confirms that the methodology for computing the noise contribution is sound. The noise contribution to variance is then used to correct the velocity variance used in the retrieval; having a small effect only at the lowest dissipation rates. This source of uncertainty is combined with the second source (discussed below) to get the total theoretical uncertainty in dissipation rates, as described in the new section. An example time-height plot of this uncertainty is shown in the new Figure 1 (attached as NewFigure1.png) and these errors are shown as a function of the calculated variance in another new figure (attached as NewFigure3.png).

Uncertainty #2: Length scale calculation. First, the reviewer mentions that there is "no indication of how this quantity is estimated." This is not entirely true as there is an equation in the original document on page 752, line 9 that shows how the length scale is related to the horizontal wind. The text also indicates that the wind speed is derived from radiosonde or wind profiler measurements. However, we agree that few details were initially provided on this aspect of the retrieval. The following sentence has been added to bring more clarity to this issue: "Horizontal wind speed is interpolated to the radar time-height grid from collocated wind profiler measurements (449-MHz profiler at ASCOS, 915-MHz profiler at MPACE) with nominal 30-min time resolution; however, if wind profiler measurements are not available or inconsistent in time, winds are derived via interpolation from the nearest in time radiosondings."

The reviewer suggests that under very low dissipation rate conditions the uncertainty of this type of retrieval may be high. In a relative sense this is true, and it has been shown using a new figure that relates the fractional error in dissipation rate to the magnitude of the dissipation rate. However, even for the lowest derived dissipation rates the computed uncertainty is typically smaller than 250%. In comparison to O'Connor et al. (2010) the uncertainties are significantly lower here, primarily because observations are only used when there is at least a signal-to-noise ratio of -13 dB, i.e., relatively large return signals from clouds. While we do not know for sure, we expect that O'Connor et al. are often dealing with much weaker signals (lower signal-to-noise ratio) that come from aerosol particles in the atmosphere.

Thus, while low dissipation rates in this analysis do have a larger error, we feel that they are still reasonably estimated and see no reason to remove them from the data set (they are all less than the 300% fractional dissipation rate threshold that the reviewer suggests). Further, the question is asked about "what is the physical significance and importance of dissipation rates made in areas that are non-turbulent?" Non-turbulent is a qualitative term, as turbulence in the atmosphere occurs over a wide range of values, some of which are very much smaller than others. The primary use of the retrievals here, as stated in the text, is to understand the vertical structure of turbulence and its evolution in time. Thus, the difference in high turbulence and low turbulence is both significant and important for interpreting cloud processes. The low dissipation rates after 6 UTC in the original Figure 2 are specifically mentioned by the reviewer. These rates, and the fact that they are small, are entirely central to understanding the turbulent structure in space and time for this example cloud scene. Furthermore, the new timeseries power spectra analysis and figure that have been added demonstrate that dissipation rates derived during this low turbulence time period do have merit. Ultimately the difference in perspective between this work and the O'Connor et al. work comes down to different signal strengths; the cloud radar is unable to make these retrievals in clear air, while the Doppler lidar is able to (albeit with larger uncertainty) due to aerosol return.

Lastly, the reviewer indicates that differences between techniques of an order of magnitude are not very satisfying unless something can be said about the expected variability. The reviewer specifically refers to the original Fig. 1 and appears to misinterpret the figure, apparently thinking it is a comparison of different techniques. The figure is actually a comparison of two different, sonic anemometer-derived dissipation rates (one from tethersonde, one from tower) that use the same basic technique but are simply separated by some horizontal distance. The point of this comparison is that much of the variability among perspectives is due to the spatial separation and not due to the techniques themselves. This point has been emphasized and clarified in the text, in part using a lag analysis for measurements from a given source (tower or tethersonde) to give one perspective on differences due to spatial separation. Further, the reviewer suggests that if the low dissipation rate values were removed from the comparison, the correlation would be worse. Perhaps that is the case but we have no justification for

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removing low dissipation rates derived from sonic anemometer measurements.

It is not entirely clear what the reviewer intends with the last paragraph of the review. One aspect that seems to ring through is a desire to have more information on the potential uncertainties in dissipation rates that are used to evaluate the radar retrievals. While the intent of the manuscript is not to dive into the intimate details of deriving dissipation rates from these other platforms, some additional information is provided in section 2.3 where the validation data sets are introduced that help in the interpretation of these estimates and the comparisons with radar retrievals.

Interactive comment on Atmos. Meas. Tech. Discuss., 5, 747, 2012.

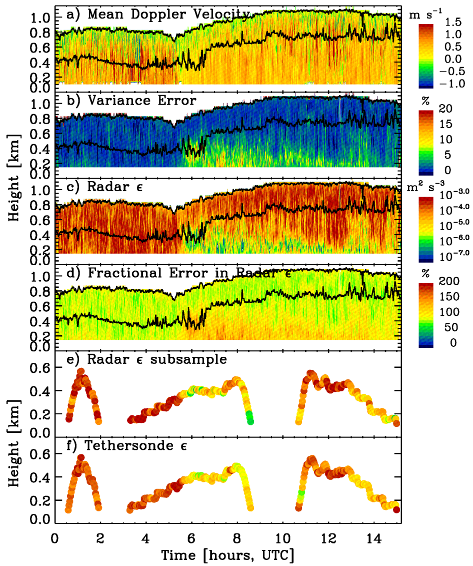

Fig. 1. New Figure 1 based on former figure with some new additions.

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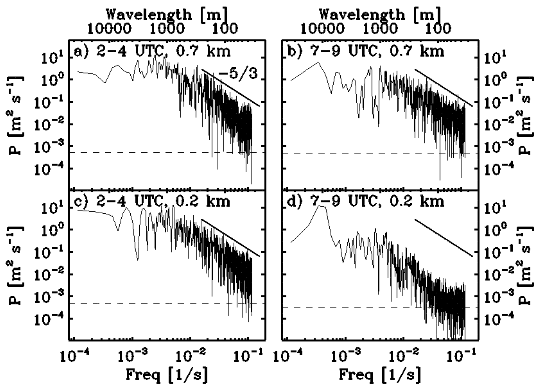

Fig. 2. New Figure showing power spectra computed from 1011 samples (∼ 2 hours) of mean Doppler velocity measurements at two times and two heights during the 28 August 2008 case at ASCOS.

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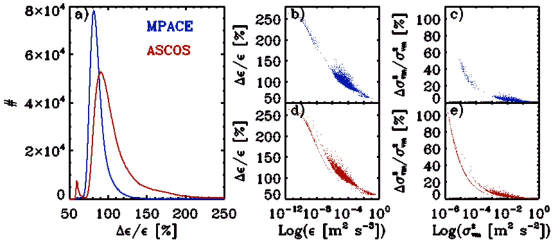

Fig. 3. New figure showing information on the errors associated with the dissipation rate retrieval.