Review

"Low-level mixing height detection in coastal locations with a scanning Doppler lidar"

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SYNOPSIS:

The paper describes a new method for estimating the height of the mixed layer at levels below the typical minimum range of a vertically looking Doppler lidar.

GENERAL COMMENTS:

The topic is both timely and relevant, and the manuscript is very well written. There appears to be some room for improvement in the theoretical discussion of the method, while the data sets presented are sufficiently illustrative and well analyzed.

SPECIFIC COMMENTS:

- My major complaint is the lacking theoretical justification of the method: Equation (1) comes out of nowhere and is not justified at all. The decomposition of the wind field can be done in various ways, but it is not clear what approach the authors had in mind. It appears to be a mix of the classical Reynolds decomposition applied in statistical turbulence theory (homogeneous or mean term and turbulent term) and a Taylor power series expansion terminated after the linear part (deformation, divergence, but then the rotational term is missing). The additional term due to "surface interactions" appears to be somewhat contrived since it would not fit in either decomposition approach. Furthermore, the notation is non-standard and probably more confusing than helpful. I would suggest that the authors revise section 2.2 to achieve the required clarity and correctness. In view of the further development in eqns. (2-10), a simplification seems to be possible since the authors argue that the deformation term, the divergence term and the "surface term" are more or less negligible in the situations considered.

- Section 2.2, page 7, line 21: The statement that the instrument uncertainty with regard to velocity is only a function of the signal-to-noise ratio seems to be a simplifying assumption. Other dependencies might also exist, like a bias or scanner pointing inaccuracies. Furthermore, while precipitation increases the SNR this may not necessarily improve the accuracy of the wind estimation since there can be a discrepancy between the wind velocity and the velocity of the particles. This is particularly relevant for the determination of the vertical wind component.

- Section 2.2., page 8, line 1: It is not clear how the threshold of 1.58 m^2/s^2 is obtained from an estimated σ^2_{VAD} for a SNR > 0.0025. Should this not be a function of the elevation angle used in the VAD-scan?

- Section 3.1., page 9, lines 3-6: Based on Fig. 3 it is stated that the relationship between σ^2_{VAD} and σ^2_w is *reasonably linear*, but especially the data from Loviisa do not strongly support this claim. It is also not very obvious that Fig. 3 supports the argument that the relationship between σ^2_{VAD} and σ^2_w is independent of the VAD elevation angle. Given the importance of these assertions, the authors should discuss any possible limitation of their conclusion.

- References: The authors should use standard abbreviations for Meteorologische Zeitschrift, Boundary-Layer Meteorology, Quarterly Journal of the Royal Meteorological Society and Journal of Geophysical Research.

- Table 1: A lens diameter should not have the dimension of µrad.