Review of “Uncertainty model for dual-Doppler retrievals of wind speed and wind direction” by Nikola Vasiljevi et al.

General comments

This paper presents an analytical model for the uncertainty in horizontal wind speed and direction estimates obtained using a dual-Doppler sampling technique. The model attempts to account for ranging and pointing errors using a very simplistic atmospheric model, i.e. one in which the height variation is modeled using a power-law and the flow is assumed to be horizontally homogeneous. As a result, the radial velocity uncertainty contains no azimuthal dependence, which is not realistic. The author’s don’t provide a compelling justification for this assumption. I believe the problem calls for a more stochastic approach in which 3D variations are considered, as the authors suggest in the “discussion” section of this paper. Another limitation is that the error in the observed radial velocity is treated as a constant, when in fact it varies strongly with range (and depends on aerosol loading) in real lidar systems. At longer ranges I would imagine that this is probably the dominate source of random error in most dual-Doppler lidar set ups. The only practical utility I see is that this model may be useful is as a planning tool for dual-Doppler deployments (provided the range-dependence of the radial velocity uncertainty is accounted for). In my view, this paper is incomplete, in that there is more that should be done to make this a useful contribution. I recommend against publication in its current form.

Specific comments

Abstract: The abstract is short on results and it does not adequately convey important assumptions, i.e. that the elevation angle must be small, and the wind field is assumed to be horizontally homogeneous.

page 1 line 5: change “…available though as …” to “…available as …”

page 4 lines 5-10: Here the author critiques the inconsistent use of terminology (i.e. precision, accuracy, trueness??) but does not offer to clarify the meanings of these various terms.

Equation 4: Change “V_{rad1}” to “$V_{radial1}$”

Page 5, line 2: The author states “Due to the above-mentioned amplification of uncertainties caused by the Dual-Doppler setup…” But the author has not explained the “amplification” parameter yet at this point in the paper.

Page 5, line 7: Here and many other places in the paper the author refers to the radial velocity as the “radial wind speed.” Wind speed is defined as the magnitude of the velocity vector. So “radial wind speed” doesn’t make any sense. Please change all occurances of “radial wind speed” to “radial velocity.”

Page 5, lines 7 and 8: The author poses two questions about accuracy, without ever defining “accuracy” (or for that matter without defining uncertainty, error, precision, or trueness???).

Page 5, line 12: The author introduces the beam direction parameter “D”, but it is never used anywhere else in the paper. Please delete.

Equation 5: This equation gives the true radial velocity as a function of the measured radial velocity, This cant be right, because it implies you can somehow correct for random errors. This equation would make more sense if the right-hand-side was the radial velocity uncertainty. Please clarify.

Page 6, lines 4: Here the author discusses the atmospheric flow model. Winds are assumed to be horizontally homogeneous and the wind speed profile is modeled using a power-law relationship. As I point out above, this is an oversimplification. As a result, the uncertainty model would not be useful for assigning uncertainty estimates to actual measurements performed in the field (what value of alpha to use?). The only utility I can see is as a field campaign planning tool that provides crude estimate of expected errors for a given dual-Doppler set up (provided the range-dependence of u_{est} is included). For the results of this study to be of practical use a more realistic model of the atmospheric flow is needed (e.g. Rod Frehlich's approach using the Von Karman spectrum). This would probably make any sort of analytical solution impossible.

Page 7, line 14: The standard meteorological definition of "wind direction" is the direction from which the wind blows. You could simply point this out, rather than introduce a non-standard term like "wind from direction".

Equation 17: u_{est} is range dependent.

Page 9, line 2: When referring to uVradial , uest, u_, u' and uR the author states "These uncertainties are derived using dedicated calibration procedures which will be the topic of the follow-up publication." This is partly why I feel this study is not complete. These error estimates are central to this paper, and need to be discussed here, particularly u_{est}, which appears to be treated as a constant in this study, but should be range dependent.

Page 13, line 5: I don't believe "digitalized" (or digitize) is appropriate here. It would be more accurate to say that the model was programmed in Python.

Table 2 and 3: The top row (parameters) is confusing because its not clear how these relate to the numbers below. In the caption I would suggest that you explain that these numbers represent averages for theta=0 and theta=270.

Figure 8. The units for the wind direction uncertainty are shown as m/s. Should they be in degrees?. Please clarify.

Page 22, lines 3-5.The author states "Such a correlation would tend to slightly increase the combined uncertainty for the u component (where the radial speeds add) and reduce it for the v component (where the radial speeds subtract)." This is not generally true. The author seems to be referring to a specific dual-Doppler configuration.

Page 22, lines 7-9: The author states "The LOS uncertainty comprises both a random (noise) and a systematic part. Usually the latter will dominate and is largely determined by the uncertainty of the reference speed used in the calibration (often a cup anemometer)." The second sentence isn't generally true (that bias is larger than precision), unless the author is referring specifically to the behavior of their uncertainty model. Please clarify.

Page 22, lines 12-13: The author states “With a Monte Carlo simulation the measurement process is mimicked by calculating the measurement output for a number of randomly chosen input parameters but with known statistics corresponding to their expectation value and uncertainty.” I concur with this approach.