Responses to Reviewer 2's comments

(author responses are given in **RED** text)

General comments:

Very interesting and well written paper on how the precision of VAD scans can best be quantified using in-situ data. Following (maybe) my comments from the pre-review, the authors now make a clearer distinction between precision and uncertainty, where the latter contains contributions from both random ("precision", zero mean) and non-random ("bias", non-zero mean) sources. This paper, as the authors now recognise, is very much about precision and only mentions bias in passing. In their example, where the VAD scan is short (not the main duty of the lidar), this might well be fair enough since the random errors will be large and may (or may not) dominate. For a dedicated VAD scanner where the reconstructions are based on 10 minute mean radial wind speeds, the random error will be extremely small. In any case for an application such as a resource assessment in wind energy, it is bias (from the radial speed itself, from the elevation angle, from the range) that matters since the final result will inevitably be aggregated from many hundreds or thousands of samples. Put simply, random errors average to zero, biases don't! I would welcome some reflections on these issues in the paper (e.g. in the introduction or in a discussion) on where the precision quantification techniques are relevant (these are probably there already) and where they are less relevant.

I am still a little disturbed by the term 'radial wind speed uncertainty' meaning a spread of deviations from the speed expected by the VAD model but have trouble finding something better: 'radial wind speed non-conformity' perhaps? In any case I would be grateful if the authors could be even clearer when they introduce the term in explaining exactly what is meant.

In the introduction (original and revised) we felt it was important to stress that, in the context of VAD, any deviation in the radial velocity from a perfect sinusoid (when view as a function of the azimuth) is interpreted as error.

The paper is a little long and sometimes I got a bit lost, especially in section 3. Maybe some more sub-section headings would be useful. Also consider shortening the paper. Could the work on using the precision assessment for looking at different scanning strategies (2d, 3d, 4beam, 8beam etc.) be moved to a separate paper (it gets a bit lost here anyway)?

After some careful thought, we have to agree with the reviewer. We originally included the stuff on the effects of scan geometry and dimensionality in this paper because we felt it would be of interest. However, it doesn't fit too well. So, we have opted remove this material in the revised manuscript.

Specific comments:

P2, line 23: Maybe make it clearer that by 'perfectly homogeneous flow' you also mean a flow without any turbulence. Could for example just add "of turbulence and" making ".. in the absence of turbulence and measurement error.."

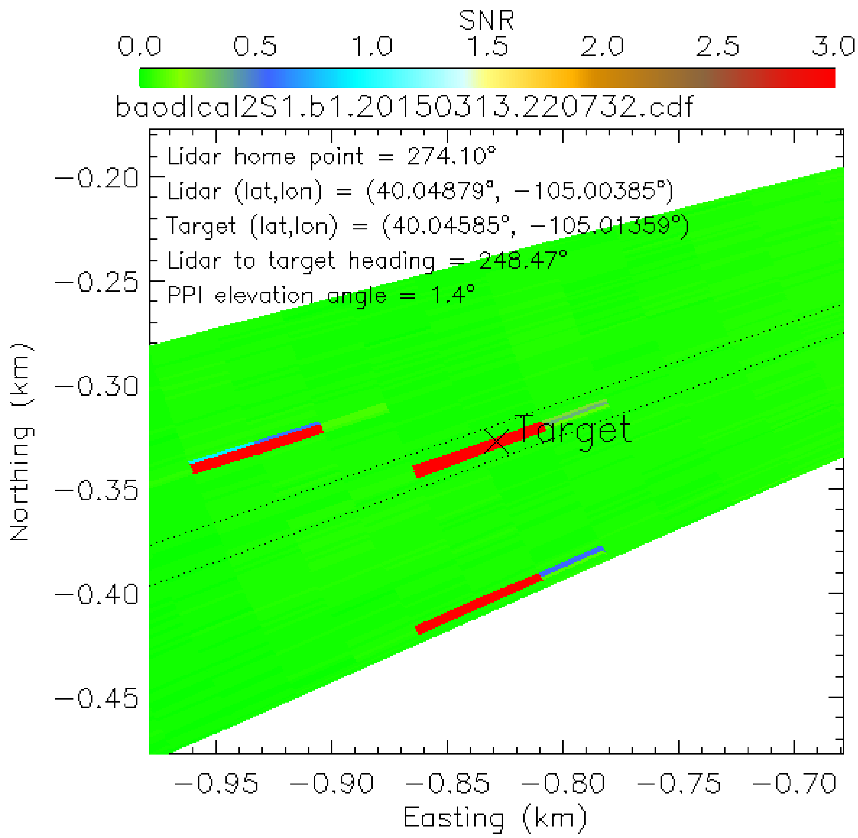
We have made the change.

P4, line 27: I think you mean “resolution” not “precision” (enough of those already…) as in “angular resolution of the scanner”

Actually, it should be “precision” (i.e. random uncertainty in the pointing direction) and not resolution.

End of section 2.1: You briefly mention pointing uncertainty but you don’t estimate it (i.e. elevation angle uncertainty, azimuth uncertainty) and you don’t even mention range uncertainty. If you are not sensing at the right height (you aren’t exactly) you are not sensing the right speed. Another significant uncertainty comes from the size and shape of the probe (you touch on this later). It would be a really useful addition to the paper to make an estimate of how much (non-random) uncertainty all these things (and the los speed uncertainty and anything else) combine to (e.g. using GUM). It is not zero – it never can be. It is probably quite significant.

As the reviewer points out, the issue of pointing accuracy is addressed in the last paragraph of section 2.1, where we discuss the daily target scans that were performed to monitor any azimuthal drift in the scanner. We did not observe any drift over the short deployment period (if we did we would have corrected for it). The daily target scans were also useful in establishing the so-called range errors. The plot below shows the SNR from a typical daily target scan. The “x” indicates the predicted location of the target (stadium light post) based on the known GPS coordinates of the target and the lidar. The red pixels show the high SNR returns from the light posts. As you can see, the “x” falls nearly in the middle (in the range dimension) of the pixel with the hard target return. Based on this we can safely assume the range error to be negligible.



In response to the reviewer's comment we have added the following text to p4 lines 24-26 of the revised manuscript:

"The observed location of the hard target return in the scan data, together with the known GPS coordinates of the lidar and the target enabled us to determine of the lidar's orientation with respect to true north, and to estimate any error in the reported range. In this case, no significant range errors were observed."

P7, line 1: please explain why sigma_n should be 1.

Sigma_n is the "instrumental precision" as we now refer to it in the revised manuscript. We do not set sigma_n=1, rather we set sigma_ri=1. In this case, all the measurements are equally weighted. This is common practice when the uncertainties are not known (see Numerical Recipies).

P7, equation 7: please define psi

Psi is given by equation (1).

P8. After line 2: Here it could be good to have a sub-section heading “Obtaining the radial velocity measurement precision”. Just an example.

We have added a new subsection header on p7 line 17.

P10, line 3: Why scalar averaging? Will it make much difference over such a short time anyway?

Scalar averaging of the wind speed is the common practice in the wind energy community.

P10-11 – the section on interpolating the sonics using the lidar weighting function: This section strikes me as really over-complicated and unnecessary since as you conclude, the lidar senses pretty much at the sonic heights anyway (would have been a silly experimental design if it didn’t). What would be more interesting here are some reflections on what you are comparing with what. What role does the uncertainty (precision + bias) of the sonic play (and how big are these)?

We should point out that the application of the range-weighting-function (RWF) serves a dual purpose. First, it provides a way of interpolating the sonic data to the height coordinates of the CDL. Second, it accounts for the spatial averaging effect that is inherent in the CDL measurements.
The reviewer raises a good point though. In the original manuscript, we simply introduce the RWF without explaining the need to reconcile point measurements (from the sonic) with spatial averages from the CDL. Thus, we have added the following text to p10 lines 7-13 in the revised manuscript:

“The sonic anemometer’a probe volume is considerably smaller than that of the CDL, where for all practical purposes, we may regard the sonic anemometer as a point measurement. By contrast, the CDL measurements represent a convolution (in the range dimension) of the instantaneous (i.e. point) radial velocity with the laser pulse range weighting function (RWF) and the range gate length (Frehlich and Cornman 2002). The size of the lidar’s probe volume is defined by the width of the Gaussian laser pulse and the transverse extent of the beam, which is roughly 10cm.
In an effort to account for the spatial averaging that is inherent in the CDL measurements we applied an estimate of the CDL’s RWF to the sonic anemometer data. This was also used as a means of interpolating the sonic anemometer measurements to the height coordinates of the CDL. This interpolation takes the following form:”

P12, line 13: “Thus it is appropriate to equate precision with uncertainty in this case.” – Completely disagree with this statement. You have one observation (assuming the non-random effects to be persistent throughout the campaign) and are comparing against something that is itself uncertain. One zero error does not mean that the uncertainty is zero. This comes again in the conclusion (p15, line 4).

This paragraph has been completely revised. In the revised paragraph we no longer make the above statement. We are assuming the sonic data to be “truth.” In the summary (original and revised) we are careful to state that the CDL winds showed “… negligible bias when compared to the BAO tower sonic anemometers.” We believe that we have properly qualified our statement by saying “…when compared to the BAO tower sonic anemometers.”