Response to reviewer's (Peter Taylor) comments on the manuscript "A six-beam method to measure turbulence statistics using ground-based wind lidars"

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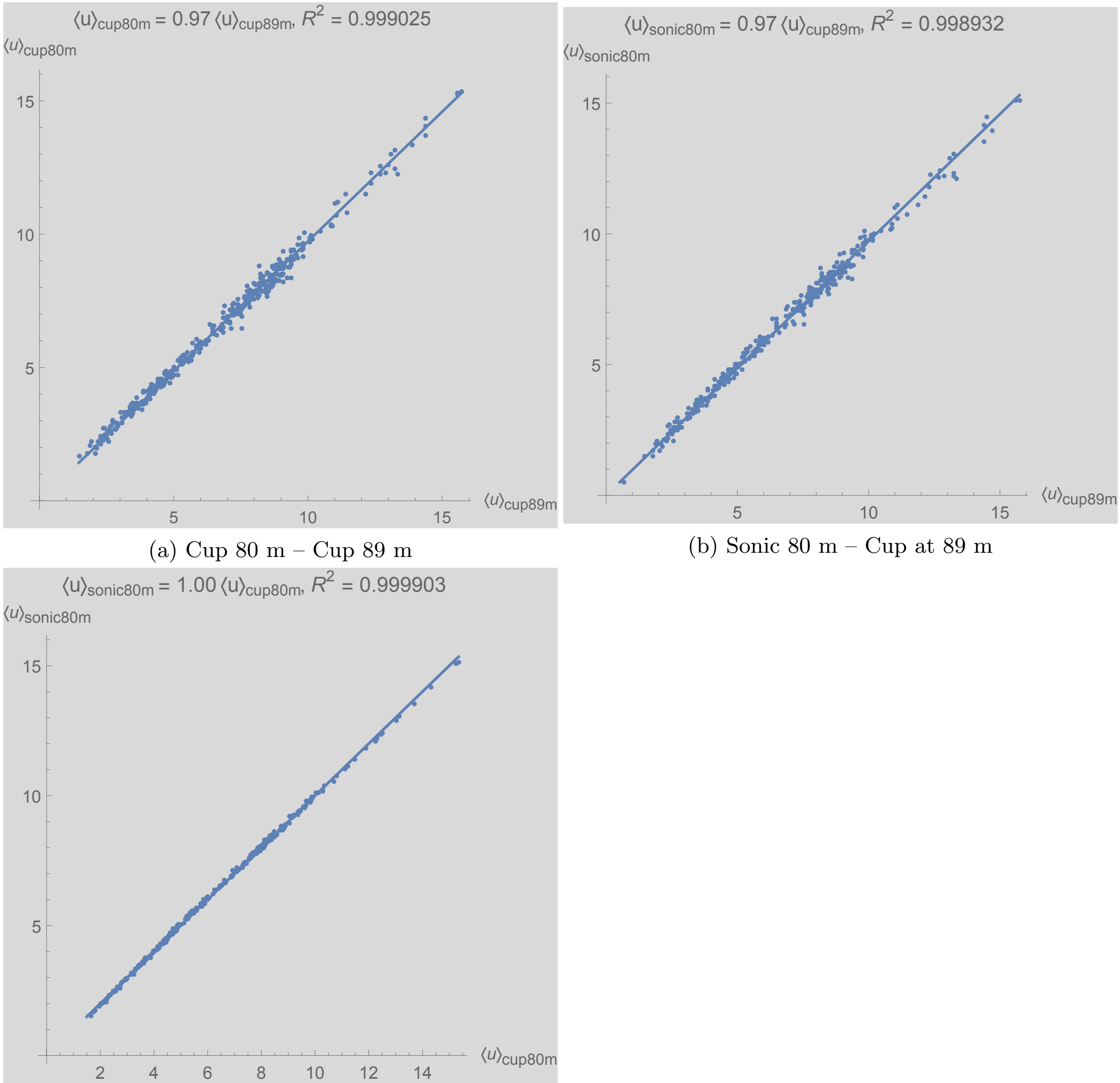
First of all we would like to thank the reviewer for taking the time to review our manuscript. Following questions are raised, for which the corresponding responses are given:

Question Despite the wind energy community enthusiasm for cup anemometers this raises the question of why there were no comparisons with sonic anemometer determinations of the complete stress tensor. Maybe that is in progress but clearly measurements and comparisons of all 6 stress tensor components would be desirable. There are sonic anemometer data available near the site which were used for Obukhov length determinations. Admittedly these were from an 80m level sonic on a mast that was about 1000m away but some rough comparisons might have been appropriate. I presume that they may have been made but not reported.

Author Response We indeed had to make a choice between the sonic anemometer at 80 m that is placed on a mast approximately 883 m south-east from the position of the WindScanner, and a combination of a cup anemometer and a wind vane at 89 m placed on a 89-m mast approximately 41 m in the west direction. Owing to the wind turbines in the east, we were restricted in the wind directions only from the west, where there is a built-up of an internal boundary layer due to sudden roughness changes. We thus had to first make sure that the assumption of horizontal homogeneity, on which the six-beam method is based, holds true for large distances. To this extent, we first made inter-comparisons of the 30-min mean wind speeds between the reference instruments, i.e. the sonic and cup at 80 m height on a 116-m mast, and the cup at 89 m on the 89-m mast.

Figures 1a and 1b show that there is approximately 3% bias in the wind speed measurement when reference instruments that are separated more than 850 m in horizontal extent are inter-compared. Figure 1c shows that there is hardly any bias when two instruments on the same mast are inter-compared. These measurements indicated possibility of horizontal inhomogeneity between the WindScanner position and the 116-m mast. To further strengthen our arguments, we also inter-compared $\langle u'^2 \rangle$ between different reference instruments.

Figures 2a and 2b show that there is a significant bias and scatter between the cup anemometer at 89 m on the 89-m mast, and the sonic and cup anemometers at 80 m on a 116-m mast, which are separated by approximately 868 m distance. The bias and scatter are much reduced for the sonic and cup on the same 116-m mast at 80m height (Fig. 2c). From the above analysis, we were convinced about the existence of horizontal



(a) Cup 80 m – Cup 89 m

(b) Sonic 80 m – Cup at 89 m

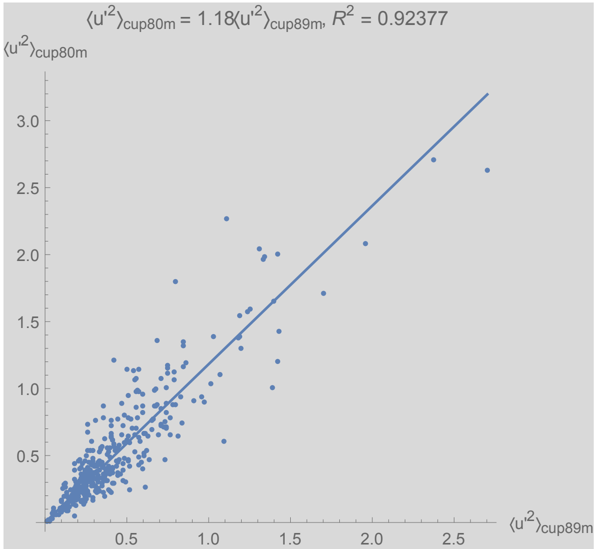
(c) Sonic 80 m – Cup 80 m

Figure 1: Comparison of the 30-min mean wind speeds between different instruments

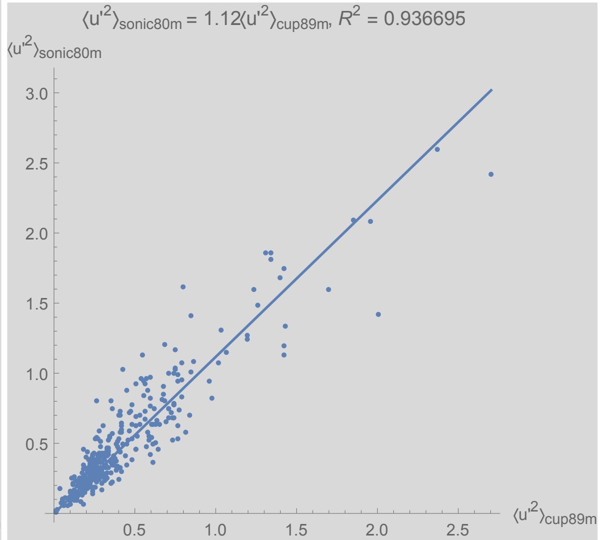
inhomogeneity between the WindScanner and the 116-m mast where the sonics are available. Hence we chose the cup and vane at 89 m for inter-comparisons with the WindScanner turbulence measurements.

Question One slightly unsatisfactory aspect of the paper is Section 2.2. After struggling through the matrix algebra of Section 2.1 we suddenly get the results in Table 1 with rather a minimal explanation and only a partial indication of how the results were achieved. It certainly seems reasonable that with five beams at the same zenith angle and one with a zero zenith angle then uniform azimuthal spacing is optimal, but it is not obvious that the 5 and 1 arrangement is better than a 3 and 3 division between 2 polar angles or some other arrangement. The 45° zenith angle also seems reasonable but this was imposed as a limit and it would be interesting to know how different this is from a larger angle or the 15° or 30° used in some commercial lidar profilers.

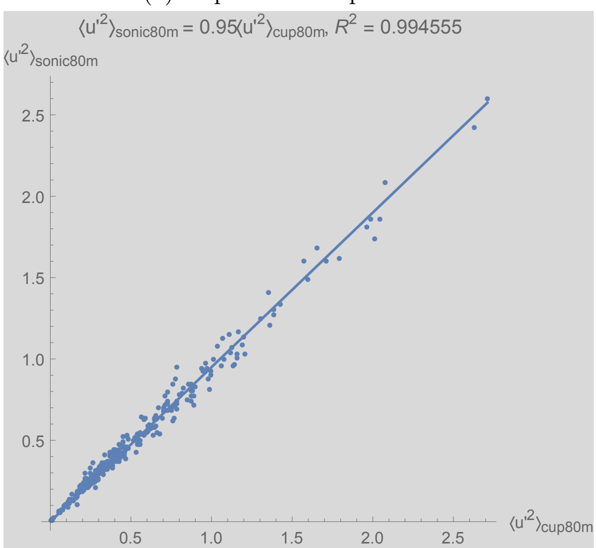
Author Response We agree with the reviewer that it is certainly not obvious that the 5



(a) Cup 80 m – Cup at 89 m



(b) Sonic 80 m – Cup at 89 m



(c) Sonic 80 m – Cup at 80 m

Figure 2: Comparison of $\langle u'^2 \rangle$ between different reference instruments

and 1 arrangement is better than any other arrangement. Therefore in section 2.2, we have first stated in lines 8–9 that the optimization problem contains 12 unknown variables, i.e. a combination of six unknown zenith angles and six unknown azimuth angles. Subsequently we performed optimization using three different algorithms as explained in lines 21. Only after that we obtain table 1, where all the three algorithms produced the same optimum result. We also accept that the found optimum may only be a local optimum, since direct search methods of optimization are used, as stated in lines 15–17.

Unfortunately we cannot visualize the optimization problem, since there are 12 unknown variables, which otherwise would have given a clearer picture of the obtained result.

Question The large data set (401 thirty minute blocks) is impressive and the results very interesting. Mean winds show excellent agreement ($\langle Ulidar \rangle = 0.999 \langle Ucup \rangle$ with $r^2 > 0.999$) in comparison with the cup and vane measurements but the variances show more scatter and less agreement. Turbulence levels with the 6 beam variance calculations are higher than those obtained with the VAD method, as one would expect, but for unstable and neutral stratification they have lower variance than values from the cup anemometer and vane method (ratios 0.85 - 0.9). More discussion of this aspect would be useful, including spectra of the radial velocities and some discussion of the reliability of variances computed with the cup and vane approach.

Author Response A brief statement is made with regards to the smaller measurement of the variances by the lidars as compared to the cups in section 4, lines 19–21. One could include plots of radial velocity spectra to illustrate the problem further. However, it has already been done before (albeit with sonics) (fig. 6 in Mann et al. [2009] and fig. 4 in Sjöholm et al. [2009]). In the next version of the manuscript, we will incorporate a statement concerning this, referring to the respective articles after line 21 in section 4.

As regards reliability of the computed variances, it is true that cups and vanes might have difficulties resolving small-scale turbulence (of the order of 2 m or less), but at the heights where we are measuring, the turbulence length scale is quite large, so there is not much to loose. Also, one could partially see in Fig. 8 that there is no high frequency noise that could potentially also contaminate variance measurements.

Question As a final point it might be worth noting that there are 5-beam commercial units available with two zenith angles (0° and 30°) and if one were to rotate the coordinate frame into the mean wind direction and assume, for example, that $\langle v'w' \rangle = 0$ then it is possible to compute the remaining stress tensor components from the 5 beam variances.

Author Response In the boundary layer, we often see that $\langle v'w' \rangle \neq 0$, because of change of wind direction with height. This can also happen close to the surface [Berg et al., 2013]. This means that it is not safe to use the assumption that $\langle v'w' \rangle = 0$ (or perhaps $\langle u'v' \rangle = 0$), since it will increase the bias in turbulence measurements.

References

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