Dear Referee #2,

We thank you for the initial review of our manuscript and your detailed remarks. Unfortunately, it seems that there was a problem which prevented a full review of our study, particularly the results section. Therefore, we would like to address your major concerns here in an immediate reply. Hopefully our response can enable a further of review of the manuscript.

You find our remarks on your main concerns below. We will address the rest of your questions adequately in the full review. Please do not hesitate to voice any further findings.

Many thanks for your work so far and kind regards,

Philipp Gasch and Co-authors

Major comments:

(1) The writing style, English and grammar needs work and many comments are listed below. I stopped after several pages because it was taking too much time. Significant re-writing and organizational changes are needed in the manuscript. I also think the paper is much too long. It appears that this journal does not have a page limit, but it would help readers to shorten the discussion in several places and remove sections that are not needed (some examples given below). The paper reads a bit like a dissertation with too much background and drawn out detail. A published paper should be more concise without sacrificing understanding of the problem. Please shorten the results section, it looks like too much information is presented and it might not be worthy to publish all of it.

We are sorry to hear that the style, English and grammar did not suit your tastes. We are surprised about the comments about the English, as one of our co-authors is a native speaker and carefully proofread the manuscript. We are therefore very sorry for the inconsistencies that have slipped our attention.

Regarding the shortening: The manuscript was designed for publication and not as a dissertation. Due to the many aspects considered we felt that some background information might be helpful for some readers. We will sincerely consider a further shortening in the review process and have noted your remark on the 'nadir' section.

(2) Throughout the manuscript the word "homogeneous" and "inhomogeneities" are used and this represents a critical aspect of the study and results. For example, "...mismatch between assumed homogeneous wind field models and the wind field inhomogeneities during the measurement process". These are ambiguous terms and I don't understand how they are being used in this context. Since they represent critical points of the paper, it is hard for me to assess the method and results. The authors need to lay out in detail what they are referring to here and clarify this throughout the manuscript. Are you talking about wind variability below the scale of the instrument footprint, grid spacing of the wind retrievals, something else?

In this study we are concerned about errors in wind profiling that arise from the fact that the analysis method assumes that the flow is homogeneous (i.e. constant mean wind profiles), whereas an actual boundary layer experiences flow inhomogeneities driven by turbulence, such as discussed in Shapiro and Fedorovich (2007), Kiemle et al. (2011) and Lundquist et al. (2015).

By the term “wind flow inhomogeneities” we refer to deviations of the wind speed from the mean state due to boundary layer turbulence. In order to consider only turbulent conditions, we limit the extent of our analysis domain to 0-800 m vertically (inside the boundary layer) in Sec. 4. The structure of the turbulence present in the LES is further detailed in our answer to remark (3). The range of the wind variability is presented by the spectra of the turbulence (fig. 5.8 in Stawiarski, 2014). The dominant range of the turbulence spectra is larger than the instrument footprint (10 cm beam diameter, 300 ns Gaussian pulse width, 72 m range gate length), enabling an accurate representation of the turbulence in the simulated measurements. The dominant range of the turbulence spectra is smaller than the horizontal grid spacing of the retrieval (1.3x1.9 km) but larger than the vertical spacing (60 m). This spatial mismatch clearly influences our results in Sec. 4 and is discussed there.

The term homogeneous flow (a synonym is homogeneous wind field conditions) means a uniform flow and the absence of any deviations from the mean flow state, both spatially and temporally (Stull, 2000). Homogeneous flow is assumed in the most simple form of the Velocity Azimuth Display (VAD) and Volume Velocity Processing (VVP) retrievals (Koscielny et al. 1984, Boccippio, 1994, Banakh et. al, 1995, Leon and Vali, 1996). Homogeneous flow is rarely present in the real atmosphere and certainly not inside the boundary layer. The violation of the homogeneity assumption in the VAD/VVP retrieval due to boundary layer turbulence causes an error in the retrieved wind profile. This error is the focus of our study.

We will certainly try to make our focus and the meaning of the term ‘inhomogeneity/inhomogeneous flow’ as well as ‘homogeneous flow’ more clear in the revised version of the manuscript.

(3) The LES domain size of 5 km X 5 km X 1.8 km is extremely small and I have doubts that this domain will represent a realistic environment to test the lidar wind sampling. The authors state that a single flight through the LES does not yield sufficient statistics. However, making 25 different aircraft trajectories through a very small box, probably does not generate any real independent statistics since the retrieved winds are sampling almost the same flow (the decorrelation spatial scale is probably larger than the box itself). It appears the grid spacing of the wind retrievals might be 1.3 – 1.9 km for along/across track. Given this spacing, I don’t think the authors can generate independent flight tracks and statistics through a 5 km X 5 km domain. The authors should try their simulations with a flow in a larger domain (with coarser resolution) in order to study a more realistic environment and allow for independent statistics.

While we appreciate the reviewer’s request for a larger domain, applying coarser resolution would actually undermine the attempt to represent fully developed turbulence. We are confident of the decorrelation spatial scale, as the integral length scale (Tennekes and Lumley, 1972, Lenschow and Stankov, 1986) of the turbulence present in the LES is $L_i < 500$ m, as stated in section 2.1. Further documentation of the decorrelation scale of the turbulence is provided in the attached fig. 6.3 and fig. 6.6 from Stawiarski (2014) below.

As we constrain the analysis to heights < 800 m (above which gravity waves are present, increasing the integral length scale), the assumption of statistical independence is valid for our analysis volumes of 1.3x1.9 km, which are much larger than the integral length scale (keeping in mind that turbulence is the driver of profiling error which we are interested in).

At the given spatial and temporal integral length scales, we argue that the 16 flight directions, repeated at 1-minute spacing and with a random initial profiling offset, do sample independent air masses. The spatial distance of the wind profiles is larger than the integral length scale. The temporal spacing is on the order of the integral time scale, but aided by decorrelation through advection between subsequent wind profiles.

Unfortunately, we think a coarser model would be problematic to investigate the errors due to turbulent structures in the boundary layer. In our study the grid resolution is 10 m (corresponding to a resolution > 50 m). Thereby, Doppler lidar measurements at a measurement frequency of 1 Hz, with a range gate length of 72 m and a flight speed of 65 m s^{-1} can be realistically represented, as the resolution corresponds to the sampling rate and spatial extent of the range gates. In a coarser model, the high sampling rate would not produce independent measurements. Further, the structure of the turbulence, causing the wind profiling error we are interested in, would be less accurately represented.

Please note that our approach follows established methods. These LES wind fields have been used for Doppler lidar studies in a similar way before (Stawiarski, 2014, Stawiarski et al., 2015) and that other authors choose similar approaches for ground-based studies (Scipion et al., 2009, Scipion, 2011).

We will make sure to discuss the independence of the acquired data in a more clear way in the revised version of the manuscript.

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Figures from Stawiarski (2014)

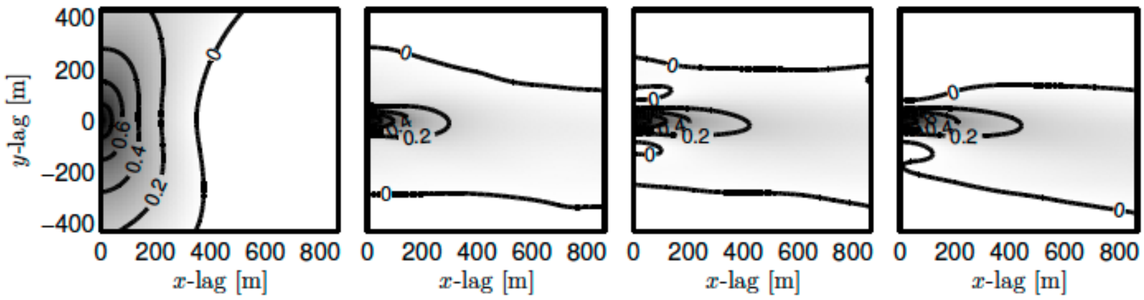

Figure 6.3.: Development of the spatial autocorrelation in the LES u wind fields with the background wind, $u_G = \{0, 5, 10, 15\}$ m/s from left to right.

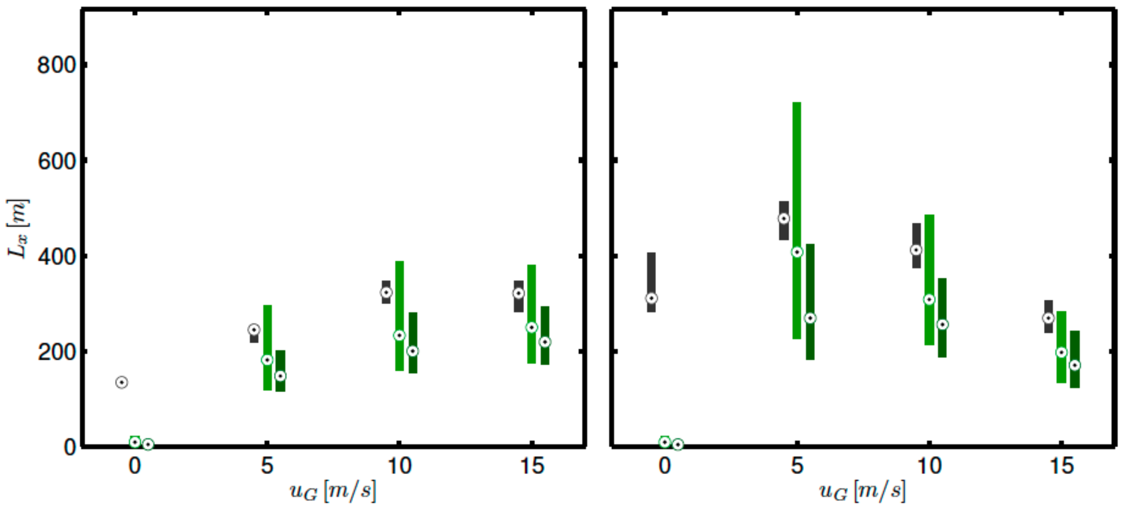

Figure 6.6.: *Integral length scales*: Scales L_x of the u (left) and v (right) wind fields in mean wind direction for the time series, computed with the full mean and variance of the data set (light green) and the mean and variance of the respective series (dark green). The black bars show the comparative LES results. The bars cover the range between the 25th and 75th percentile, the circles mark the median.