Dear Mr. Alan Brewer,

We thank you very much for the time and effort invested in the careful revision of our manuscript. The comments you provided are very helpful to improving our study. You find a first, short response to your major comments below. We hope that this will outline our strategy to improve the manuscript based on your input.

Many thanks for your work and kind regards,

Philipp Gasch and Co-authors

An LES-based airborne Doppler lidar simulator for investigation of wind profiling in inhomogeneous flow conditions This is an excellent tool for evaluating airborne Doppler lidar measurement design and will be an invaluable part of experimental design. The authors have done an excellent job of addressing the key considerations of airborne Doppler lidar sampling strategy.

General comments: It was not clear to me if the paper was designed to illustrate the capabilities of the simulator using a single set of LES and lidar configurations or if the authors are attempting to explore the operational trade space of an airborne system with sufficient detail so that the community could use this paper as a design tool rather than the simulator itself. If the goal of the paper is to illustrate the application of the tool, then I think the authors need not explore as much of phase space in every degree of freedom. A single variable plot accompanied by discussion of the underlying mechanism for the relationship for each of the major impacts would suffice. Concentrate on those plots/relationships that show significant structure and focus there.

It is true that we tried to achieve both, introducing the simulator as well as exploring the trade space of an airborne system, at the same time. We now realize that this is too complex for one study. Therefore, we will reduce the content of this study to the introduction of the simulator. As you suggested, we will highlight the ability of the LES-based simulator tool to investigate wind profiling error due to flow inhomogeneity with a basic example. The simulator is then available for use by the community and specific setups, as suggested by you. We will explore the trade space of an airborne system and the effect of measurement system inaccuracies in detail in a future study.

For example Fig 10 would benefit from another approach that highlights the differences between the two plots (highlighting the effect/impact of noise) and changing structure within a plot (slow increase in RMSE/REL). The actual values of the static variables in the plots are only meaningful for one particular set of conditions (both LES and lidar characteristics) and may not be of interest to a wide range of folks. How certain variables change wrt the chosen independent variable is of interest as it will generally do so for all configurations. These relationships should be highlighted and the underlying mechanism explained if possible. If the goal of the paper is to probe the trade space of all systems/conditions, then I think some effort should go into finding fewer “normalized” variables that are independent of specific choices for system values or LES characteristics. For example – my understanding is that the underlying bias sensitivity of the horizontal wind fit to coherent structures in the vertical wind field comes down to how many “pairs” of these features you average/accumulate over prior to doing a fit. As you increase the number of pairs of up/down motions in the volume – the residual of the fit may have a higher RMS, but the bias will decrease. This condition will depend on the dominant spatial scale of the turbulence, the size of the sample volume, the density of the sample points. . . A potential “normalized” variable might be the number of independent turbulent scale lengths per sample volume. For a given turbulence profile in the LES and a given scan pattern and beam PRF, one could imagine a profile of this “normalized” variable and corresponding uncertainty in the fit. . . If the authors are able to combine variables and break the analysis into the underlying mechanisms, it may serve to widen the impact of the paper and reduce the number of variables that have to be studied. I’m happy to discuss directly with the authors if need be – I’ll ask the editor to share my contact information. For our application, we have a fixed scan geometry (wedge scanner) so the only system variables we have to adjust are scan rate, beam PRF & pulse width. The adjustable fit parameters are height resolution and number of sweeps to integrate over. The hope would be to use a vertical transect to characterize the strength and dominant spatial scale of the turbulence as a function of height then combine that with the system/scan parameters to come up with a normalized variable “number of turbulent scale lengths per sweep” as a function of height. The hope would be that we could use that profile and your results to determine the uncertainty in the horizontal wind fit as a function of number of sweeps integrated. . . Another set of variables that could be combined – SNR, beam PRF, and LOS vel uncertainty (using the CRLB discussed below). Once the results are expressed in these terms, they are no longer only applicable to the system defined in the study. At 50 pages, this is a long manuscript and the length may limit its impact and applicability. Much of the wind profiling theory section, evaluation of errors section (definitions) could be moved to appendices.

With the limitation of this study to the basic simulator and method description we cannot include more general findings, using normalized variables as suggested, in this study. Nevertheless, your remarks make total sense and we are happy to discuss these aspects directly with you in the near future. As you proposed, based on the simulator, we have already developed a method to estimate the uncertainty of the retrieved wind profiles in-situ without the need for additional data except for some basic boundary layer parameters. Unfortunately, including this method in the study would be beyond the scope of this manuscript as it is too long already, but we are happy to share it with you immediately.

Some concerns/questions on the approach taken in the study (some of these may be redundant with first section)

1. Vertically averaging/combining results when you have a height dependent footprint combined with a height dependent turbulence profile / integral scale. Are each of the 9600 profile points from different distances below the AC? If so – how does the dominant spatial scale of the turbulence compare to the spatial scale of the sample volume/arc? (ie “number of independent samples” within a scan arc).

The 9600 profile points are from different altitudes below the aircraft. As you say, they were determined using height dependent footprints as well as turbulence profiles in order to achieve a spread among these important input variables.

With our proposed new uncertainty estimation method the average ratio of the dominant turbulence scale compared to the scale of the sample volume is taken into account by calculating an effective sample size. Therefrom, the uncertainty is determined. Unfortunately, this is too complicated to include in this study, but as stated before we are happy to share it with you immediately.

2. Sensitivity to static errors in pointing/orientation – does this just fall within the RMS of the assumed uncertainty in pointing or does a static offset impact the fit differently?

As you state below, our error emulation does not capture all aspects of errors encountered in real world systems. The effect of a static error in pointing/orientation depends on its origin, whether it is a static offset in the scanner pointing or the INS orientation. In general, it should lead to increased error levels and potentially a bias depending on the flight and wind direction. Due to the complexity of the situation and our incomplete emulation we will remove this part from the manuscript and investigate more in-depth in the discussed further study.

3. Dependence on one set of operational parameters (one lidar design) coupled with non-physical simplifications (constant uncertainty in vel as a function of height) is problematic. Need to find a set of independent variables that the user can calculate for their system / scan design.

As stated above, this will be the subject of a future study. Nevertheless, we are willing to share the method developed with you immediately.

4. Break analysis into single dimensions where possible and describe the underlying process if you have been able to glean that from the analysis so the user can project the result into their operational space.

The system trade space analysis will be removed from the manuscript and subjected to further study.

5. Describe mechanism behind bias mentioned in paper in more detail.

The mentioned possible bias at low wind speeds will remain in the revised manuscript, as this is a new and important finding. We will explain in more depth.

We will respond to the specific comments below in our final response together with the revised manuscript. Thank you once again for all the effort invested.

Specific Comments Pg 1 Line 10: “laser system noise” - detection uncertainty (see Cramer Rao Lower bound- CRLB) “beam pointing inaccuracy due to system vibrations” – We have found dominant “pointing” errors come from Inertial Navigation Unit (INU) orientation uncertainties. Most concerning are bias errors rather than RMS from vibration. Pg 1 Line 11: “system setups” – define first use Pg 1 Line 16: “short horizontal averaging distances” Along track vs cross track? Pg 2 Line 9: “the vertical wind through nadir ” - the vertical wind with nadir Pg 2 Line 20: “while neglecting the non-standard beam geometry” poorly worded / not clear Pg 2 Line 22: anelastic -> an elastic Pg 4 Line 13: “collinearity in model geometry” you mention this on Pg 2 in terms of the matrix inversion, but given that this is a primary research question, you should describe more fully. This will allow the reader to better interpret the phrase at the end of this sentence. Pg 4 Line 30: “5 x 5 x 1.8 km” I am concerned whether this will allow for sufficient independence in the multiple sampling configuration described at Pg 6 Line 10 (see comment below). By using one small set of data and varying the sampling heading angle of the plane through the domain, all paths share a common volume in the center and are not completely independent. This will impact each height differently due to sampling footprint as a function of distance below plane. I assume that the dominant turbulent spatial scales at each height are also different – so this further complicates the interpretation.

Pg 5 Line 5: “The boundary layer height is approximately. . .” At first consideration, it would seem that an important quantity would be the relationship between the integral scale and the scan volume/sample arc length - how many "integral scale" lengths are averaged over in one scan/retrieval. Do you vary this relationship and, if so, is it done by changing the input wind field or only the scan configuration? After my initial reading, it seems you are averaging your results in height, so you may be diluting the effect by averaging over multiple conditions in height. As mentioned in the prior comment, you have both height dependent sample volumes and turbulent length scales.

Pg 6 Line 11: “As the different flight tracks lead to different air mass volumes being sampled, the different flight directions are independent and can be used to increase the sample size of the statistical analysis” This is the phrase I was basing the Pg4 line 30 comment about independence.

Pg 6 Line 27: “For the frozen-in-time wind field, the LES coordinate” Are you considering atmospheric features that might be correlated to the ground - ie land usage or topographic effects? Making measurements in complex terrain might lead to making measurements in where coherent features in the vertical wind field might be present .

Pg 7 Line 29: “direction noise” Several issues to consider here: 1) While it may end up being a similar effect in the end our experience is that the estimation of orientation angles p,r,y and their angular rates are more prone to error then reading an encoder on the scanner. 2) The uncertainty in pointing angle also feeds into the LOS plat- form motion correction algorithm which then feeds back into the wind profile. 3) The static/low frequency component of the pointing offset is more problematic than a zero mean Gaussian noise source on the pointing. Depending on the inertial navigation unit (INU), errors in the drift correction of the sensor can lead to low frequency/static errors. 4) Latency in the communication with INU sensor and fast scanning/beam rates can lead to static offsets in orientation/angular rates estimation.

While you can use the ground to “calibrate” the static angle offsets – time varying, low frequency effects can still be present in the data. Upward looking scans, cloud cover, operation over water could all lead to periods where ground strikes are not available. Being able to quantify the sensitivity of these errors propagated into the wind profiles would be great.

Pg 8 Line 15: “detector noise, speckle effects and turbulence within the measure- ment volume” In areas where there are adequate aerosol, the dominant mechanism for uncertainty in the Doppler measurement comes from uncertainty in estimating the spectral peak (take a look at Frehlich, Coherent Doppler lidar measurements of winds in the weak signal regime). CRLB depends on SNR, pulse width, and averaging time – (beam rate, vertical resolution, distance from AC) these are quantities that are part of the trade space when designing a scan/sample strategy.

Within a beam you will get a range of SNR and hence uncertainty in the LOS Vel measurement (as a function of range). Setting up your runs with a single velocity error for all ranges is not representative of a true measurement. This effect is only exacerbated when you add a variable aerosol field as a function height as well.

Pg 8 Line 27 “System components” (and used elsewhere) this is an ambiguous term. In this case it would seem that you are referring to pointing vectors – but elsewhere you seem to have different meaning.

Pg 9 Line 6 “aircraft center of gravity...” The moment arm should be between the location of the INU and the center of the final turning mirror in the scanner.

Pg 10 Line 2 “restore” do you mean isolate?

Pg 10 Line 4 “system noise” No matter how well you apply the alignment calibration, there will always be a non-zero static offset. You should consider a sensitivity analysis of the wind profiles to static errors in the orientation. (rather than always assuming the errors are zero mean.)

Pg 10 Line 34 “horizontal wind profile” It can remove the effect of a static profile, but not the natural variability. If you are trying to measure w’ˆ2 – the variability in the horizontal wind will still manifest in the w’ˆ2 profile.