Comments to Reviewer 2.

We thank the reviewer for the detailed comments. Of course we are saddened by the negative recommendation and would like to take this opportunity to refute the main criticisms. Should the editor in any case consider our paper for publication we will happily deal with the remaining issues kindly indicated by the reviewer.

The reviewer believes that our modelling assumptions are too restrictive, stating *'There are numerous assumptions that must be made to solve the equations analytically.'* This statement is not strictly correct. We are not **solving** equations analytically, we are simply applying the GUM methodology which basically requires us to calculate and combine a number of (rather complicated) partial derivatives. We could have included a logarithmic vertical shear model rather than the power-law, we could have included horizontal shear parameters and we could have included correlations between the uncertainties. We choose not to do so because we believe that none of these would significantly change the result, only make the system of resulting equations even more complicated. There is nothing preventing us (or others, the code is open source and freely available) from extending the model as and where necessary, for example by adding horizontal shear gradients and including correlations between uncertainties if we identify this requirement.

The reviewer also states that our assumptions are '*incredibly limiting*'. We do not believe this to be the case. There will be many relevant applications, for example flat terrain and offshore wind resource assessment, where the model is readily applicable. Here we will attempt to justify our reasoning:

1) Horizontal homogeneity – We have not included horizontal gradients of wind speed (or direction) since for the applications familiar to us (in wind energy) where accuracy of wind speed is an issue, the horizontal gradients may be of the order of 1%/km whereas the vertical gradients are of the order 0.1%/m, a factor 100 greater. It is the vertical shear that drives the wind speed error (through elevation angle and inclined beam range errors). Including realistic horizontal gradients would not significantly change our horizontal speed uncertainty estimates. This does not mean that we can not use the model for applications where horizontal gradients are present (measuring such gradients is a major and important use case). It simply reflects that the horizontal gradients themselves make no significant contribution to the uncertainty estimates since they are far too small. An example to illustrate this: A range error of 10m on a beam inclined at 1°, measuring at 100m in a vertical shear with power-law exponent 0.1 gives a speed error of about 0.17% (sin1x10/100x0.1*100). The error due to the horizontal shear (1%/km) would be 10/1000*1% = 0.01%.

2) Power-law vertical wind speed profile – This is a deliberate choice. What is required is a simple and approximate parametrization of how wind speed changes with height, including that the gradient depends on sensing height above the surface and having a parameter representing the strength of the shear. The power-law fulfills the role admirably with the advantage that the strength of the shear, the alpha parameter, is well known and typical values are readily available. This choice of model keeps the mathematics as simple as possible. A logarithmic

model could also have been used, the mathematics would have been more complicated and we would have had to choose semi-arbitrary values for two or more parameters. Our estimate of the local wind speed gradient would not have been more accurate and therefore neither would our derived wind speed uncertainty estimates. A more sophisticated shear model is of course essential to describe wind speed variations over a significant height range but that is not our ambition here. We require simply a reasonable ('ball park') estimate of the vertical wind speed gradient so that we can in turn estimate the effect of elevation angle and range errors on the reconstructed wind speed.

3) Uncertainty is uncorrelated – Again in the spirit of keeping the model from exploding mathematically, we have assumed no correlation between uncertainty components and therefore use the simpler form of the GUM equation. Where correlations are identified, they can be added as necessary. There could be some arguments that the los speed uncertainties are correlated to some degree. Please see our comments later in this note.

4) Lidars use shallow elevation angles - Most applications of dual-Doppler lidars require only moderate elevation angles since otherwise vertical wind speed components can begin to corrupt the reconstructions. Typical uses for dual-Doppler lidar where uncertainty is important include determining wind resources. Almost invariably this will be measuring at ranges of some kilometers at heights of around 100m. These will be quite shallow angles.

5) Vertical velocities are minimal – Linked very closely to the previous assumption. In typical applications we need to use low elevation angles. In this case, vertical velocities are actually unimportant since their contribution will be very small. At slightly larger elevation angles, the model will still be acceptable provided that vertical velocities are insignificant. In typical flat terrain and offshore wind resource estimation, this will also be a fair assumption. It might begin to fail in measurements over complex terrain.

In a revision of our paper, we should clearly include these justifications as well as more critically examining the limits of application of our model.

The reviewer suggests a Monte Carlo simulation as a more appropriate approach. Whilst we have used a Monte Carlo simulation to check the veracity of this model, we do believe that our approach is highly relevant for campaign planning, where the analytical expressions allow a complete uncertainty map to be generated extremely quickly. A Monte Carlo simulation would require tens or hundreds of thousands of iterations for each and every grid point. This would be extremely expensive numerically and surely rather slow. Indeed, the model is currently being used to plan a major experiment to investigate global blockage at offshore wind farms. Here the uncertainty is fundamental to distinguishing between competing hypotheses and therefore determining the success of the project. We know of no other model that combines the necessary aspects of the uncertainty determination.

The reviewer also takes exception to our assumption that the LOS velocity is a constant, stating that this is actually *'a function of range and aerosol backscatter',* also stating that *'this also violates the assumption that the uncertainty contributors are uncorrelated.'* If the reviewer is thinking of the random error on the radial wind speed then we agree that this would typically be a function of range and aerosol load but disagree that this would imply correlation between uncertainty contributors. Uncertainties can be equal but completely uncorrelated. Correlation requires a mechanism whereby an unknown error on one source necessarily implies a correlated unknown error on the other source.

However, we consider the main contribution to the los uncertainty to be the type B (unknown bias) resulting from the calibration process. This will typically be a standard uncertainty of between 1.5-2% , coming essentially from the reference uncertainty of the cup anemometer used in the calibration process. Since this is a property of the calibration process, it is reasonable to assume that it is constant with lidar range. Obviously approaching the limits of the lidar range, the lidar is unable to return a reliable speed and the uncertainty will become very high. This effect is not included in our uncertainty model but would naturally be an important parameter in any campaign design.

In our interpretation of the los speed uncertainty, the reviewer's claim that the los uncertainties are correlated may actually be true. If it is the same lidar type or calibrated (and explicitly corrected) at the same facility, then there is a possibility that both lines of sight may have the same unknown error to some degree. This limitation should be explained in the paper and possibly modified in future versions of the model.

Our proposal for a significant improvement to the paper would be to add a section justifying and explaining our model assumptions and choices very much as we have outlined above. We hope that with this major improvement the reviewer will reconsider the recommendation against publication. If this approach is acceptable, we will of course consider and treat the reviewers other comments for which we are very grateful.