Comments to Reviewer 1.

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's main criticism is that we have adopted 'a very simplistic atmospheric model', in particular 1) that we have used a power-law shear model and 2) that we have not included horizontal gradient.

We strongly believe that the choice of power-law shear is in no way a weakness or limitation. 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. In a revision of this paper, we would of course include this justification and can readily see that it is missing in the current version.

The second main criticism concerns our assumption of horizontal homogeneity, i.e. not including any horizontal wind speed gradients. Again we regret the omission of a justification but we do not believe that this is a significant weakness of our model. The justification is simply that (in the typical wind energy applications that we have in mind at least) horizontal gradients are typically no greater than 1%/km whereas vertical shear is of the order 0.1%/m (e.g. measuring at 100m with an alpha=0.1), a factor 100 greater. Wind speed errors due to measuring at the wrong height (e.g. due to (inclined beam) range or elevation angle errors) are typically one or two orders of magnitude greater than measuring at the wrong horizontal position. Including typical horizontal gradients (e.g coastal gradients or gradients due to wind farm blockage) would only very exceptionally affect the uncertainty estimates. A clear exception would be measuring across wind turbine wakes. In such applications, a detailed uncertainty analysis is much less relevant than for example in determining a coastal wind resource estimate.

A third limitation seen by the reviewer is that *'the error in the observed radial speed is treated as a constant, when in fact it varies strongly with range (and depends on aerosol loading) in real lidar systems'*. We interpret this statement as saying that the statistical uncertainty (scatter) of the radial speed varies strongly with range and aerosol loading. This we partly agree with, although for signal levels well above threshold, the statistical uncertainty is small in comparison to the type B uncertainty (unknown bias) associated with the radial speed. This type B uncertainty is the uncertainty that we can assign to the radial speed following a calibration. Typically this will essentially be the uncertainty of the reference cup anemometer in a field calibration. As stated previously, this will be much larger (typically 1-2%) than the random error and much more relevant for uncertainty estimates since no amount of averaging will reduce it (we are normally concerned with averages of between 30s and 10 mins). We do assume that this type B uncertainty remains constant with range since it is more a property of the calibration process than the lidar itself. Clearly, the ability of the lidar to return a valid signal decreases with range depending on the aerosol load. We do not treat this aspect in our model but it should clearly be included as an additional parameter in a campaign design. It would in any case be quite straightforward to use values of the los uncertainty that vary with range should this be relevant.

The reviewer does recognize the possible value of the model as a campaign planning tool but we take polite exception to the expression '*the only practical value*' since this is indeed the intended value and an extremely valuable one! For example, 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.

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.