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WIND ENERGY SCIENCE DISCUSSIONS

Interactive comment on "Improving Lidar-Derived Turbulence Estimates for Wind Energy" *by* Jennifer F. Newman and Andrew Clifton

Anonymous Referee #1

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The manuscript states that it presents a new method of estimating turbulence for lidar and as such represents a new contribution. Overall, it is difficult to determine how important this technique is because as far as it has been possible to determine the corrected turbulence is not evaluated/presented except after being filtered through power prediction. These improvements do seem to be very small – can you comment on whether this is significant? And if so which part of the model streams are necessary?

Much of the introductory material is qualitative and could be much improved and likely made briefer by using quantitative metrics. So for example from the abstract it is not possible to evaluate how good the new method is or how much effort is needed to implement it.

This conversational style continues through the Introduction which could be much re-

duced or improved. For example, 'turbulence' is not defined – so when comparing turbulence from cup anemometers or sonic anemometers versus lidar what is really being compared?

In the Background section there needs to be some reorganization. If there is going to be a general introduction to lidar technology this is where different lidar systems should be compared – with a focus on how 'turbulence' is derived by different systems.

The section on errors in lidar data is probably too broad and needs to be quantitative. Which of these errors will dominate wind speed and turbulence estimation? If not maybe it could simply be a table with references. The WC scanning circle (p4, l5-10) is surely dependent on the instrument and not a general quantity.

Similarly, the section on correcting lidar turbulence gives a qualitative overview. If this cannot be quantitative, which would be best approach, then a table giving the necessary inputs, output and advantages and disadvantages would be helpful.

In section 7 it is not clear why the step with the power law is needed – this surely introduces much larger errors than can be corrected for later?

Is it correct then that you are defining turbulence as the standard deviation of u/u for lidar, cups and sonics? (p7, l26). If so probably best to state this upfront. You say the process is similar for all three but it can't be – no coordinate rotation for cups?

It is my understanding of what you have written that you apply corrections for 1) instrument noise in the form of a spike filter 2) volume averaging and 3) variance contamination and then use machine learning to train (TI error?) on predictor variables including shear parameter, mean wind speed etc.

The datasets are$\sim$6 months from ARM site cf 60 m tower data, 2 months from BAO vs 300 m tower and 7 months from a wind farm.

You had sonic data and used the shear parameter to classify stability, why ? (Table 2)?

Table 3/4 is unclear what it is showing and what has been done? What is the MARS model ? is this different from Terra?

Please work on these Table and Figure captions. It is really difficult to understand what they pertain to. Figure 1: Not sure why this is here or why it is needed. Figure 3/4. Quality needs to be improved. Figure 5. Not sure why these histograms are needed. Which instruments are these from? Figure 6. Not sure why these graphs are needed. What is the corrected turbulence here? Figure 7. Not really sure why the regression lines are needed. Are these the TI corrected data?

I was surprised when I got to the end of the Tables and Figures that no results are presented for the turbulence-estimates given that is the main topic of the paper?

Section 5 is descriptive of the general behavior of turbulence at the sites – is it needed? There did not seem to be anything here that was unexpected so it was not clear why it is present. When I look at Figure 7 I think you are showing that there is better agreement between the sonic TI and the (uncorrected?) lidar TI when the wind shear is lower?

The results section is very unclear. A better approach would be to indicate how the results were obtained rather than stating 'optimal model combinations are shown in Table 3' – this table refers to MAE in kW before and after (training with Terra?) and indicates a reduction in MAE – so not turbulence? Where are the turbulence results? It makes me wonder if I am missing the point of the paper. How important is this? If the overall power is 2MW – then is a reduction in MAE from 2.16 to 1.77 kW important? Actually now I read this I realise I don't follow this at all. Is this like a reduction in error from 0.14% to 0.11% - is it worth this effort? Where does the error in reduction come from? Is this on the average power over one year or ? Unfortunately I was unable to determine the results that lead to the statement 'L-TERRA improves TI estimates' – how would I see that?

Overall the paper had an interesting premise but it needs a major overall to clarify and show the results pertaining to turbulence and to remove unnecessary material. The

error reduction approach has to be systematically compared to other methods and to show the effort level required to obtain it to be of utility. As it stands it would not be possible for anyone to reproduce this work.

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