Wind Energ. Sci. Discuss.,
https://doi.org/10.5194/wes-2018-71-RC1, 2019

eawe european academy of wind energy
Open Access WIND ENERGY SCIENCE DISCUSSIONS

Interactive comment on "Minute-Scale Wind Speed Forecasting Using Scanning Lidar Inflow Measurements" *by* Elliot Simon et al.

Anonymous Referee #1

Received and published: 16 January 2019

Review of WES-2018-71.

This manuscript has promise to be a real contribution to understanding the potential for Doppler lidar to be used for sub 1-hourly inflow prediction to wind farms, but as it stands does not fulfill its potential and some of the main conclusions are difficult to evaluate from the proceeding text. I am very confident that if the authors follow the suggestions given in the review below the resulting product will be much improved and accepted for publication.

The main reasons the manuscript is hard to read and follow the main points is that it reads like a report not a scholarly article. It is much too long (contains lots of only tangential information e.g. on forecasting that is not directly relevant to the analysis), the figures are generally of poor quality and are too numerous (25 figures and 6 tables)

and/or do not make the points the authors infer from them (or do so only marginally), and a fewer better quality, more synthetic figures would be much better. The figure captions are insufficiently specific to allow the reader to readily interpret the figures. Many of the references are incomplete (and rather too many are from non-refereed literature). Many sentences are unneeded ' X and Y are shown in Z' (e.g. final sentence on page 14 just cite the reference where you name the approach).

Specific comments by section/figure and table: Abstract: Imbalances in what? Did you actually test normalcy? How? Introduction Abbreviate so it highlights only the key information Motivating questions: These are very broad and the 'answers' are not really derived in the manuscript e.g. you don't really demonstrate 'how a horizontal wind field is correlated in time and space'? at least in terms of the generalizable beyond what was known prior to your analysis? But you do provide information for use of lidar at relatively high heights (200 m a.gl.) over flat (fairly uniform terrain) in short term prediction of inflow to a wind turbine. Your work is relevant and interesting but these questions are rather too broad and do not reflect what is written in the conclusions. Section 3; this is very long and could be shorter without loss of information. The size of the terrain/vegetation height map is much smaller than would be covered by any reasonable advection velocity for upto 1 hour (or even a few minutes) – maybe it could/should be expanded at least to the coastline) Section 4; This is the heart of the research but again the wind field retrieval description is very long and could be shorter. 4.2 – this machine learning procedure is not well known and is not well described. The broad outline of the approach is presented in general terms but the needed details are omitted. This section needs to be made much clearer (including materials from 4.2.2 into 4.2.1) so others could duplicate the analysis. Also instead of having web sites referenced try to find primary references. (you can't say is the parameter isn't mentioned then the default values are used since how would someone know what the default values are). Section 4.3 From a statistical point of view you should not optimize the model based on the same statistic as you use to evaluate it. Also I suspect you did this but of course temporal autocorrelation means your sample size for statistical

testing will be much smaller than the actual sample size (so maybe you can/should quote an effective sample size). This may reflect the lack of detail but I can't really see how model parameters are set or tested (e.g. was cross-fold validation used?) Section 5 5.5 – I am unclear what the model weights mean (and what the iterations mean- e.g. in Figure 24). Section 5.7 not very clear – what is being presented here? (e.g. I guess rows here mean selecting removing some range gates?)

Comments on each figure: Figure 1; maps should have scales or lat/long and/or UTM Figure 2; not needed Figure 3; much better if you overlay the scan pattern (i.e. merge with Figure 4) (note figure 11 is figure 3 repeated which is not appropriate) Figure 4 see above Figure 5-7; I appreciate there was a pointing error but these figures don't really make any point other than that – so they are not really presenting new information (beyond the reference cited), so maybe make one synthesis figure. Figure 8; Ok Figure 9 Could encode direction (and thus no need for Figure 10) Figure 11; see above Figure 12; this is a very simple concept no figure needed. Figure 13 & 14 ; integrate to make one more effective figure Figure 15 & 16 are very poor quality and emphasizes time variations NOT degree of agreement. Figure 17 presents the same data as Figure 15 and 16 – make one good effective figure – probably as a scatterplot (which would match better with the statistics quoted for a y=mx +c fit). Figure 18 & 19 – appear to represent the same data but were not very legible. Make one better figure. Figure 20; the caption is not clear – what does this show? Figure 21; this is very interesting (but this is not the meteorological definition of a gust – maybe it would be better to use a different term) Figure 22; This is potentially interesting although the authors might want to consider if RMSE is the best statistic since it is not resilient and given the comments above. Either way the terms should be defined in the caption Figure 23 – this could be included in the remake of figure 15 & 16 (unless I am wrong the mast data are the same??) Figure 24; I can't follow what is shown based on the caption provided. Figure 25 is quite hard to read and could probably be improved. – I suspect all figure captions should note the sample size. Comments on the Tables Table 1 and 2 could be integrated without loss of information Table 4: integrate this information into a figure

or the text.

Interactive comment on Wind Energ. Sci. Discuss., https://doi.org/10.5194/wes-2018-71, 2018.