Wind Energ. Sci. Discuss.,
https://doi.org/10.5194/wes-2020-50-RC2, 2020

WIND ENERGY SCIENCE DISCUSSIONS
Open Access

WESD

Interactive comment

Interactive comment on "Parameterization of Wind Evolution using Lidar" *by* Yiyin Chen et al.

Anonymous Referee #2

Received and published: 20 April 2020

This manuscript presents a statistical model of longitudinal coherence describing the evolution of turbulent structures in the wind as they travel downstream. The topic is very relevant for lidar-assisted control applications (and other wind preview-based control applications), where a good understanding of the correlation between the wind at the measurement point and the turbine is needed. There has been previous work in the literature focusing on developing wind evolution coherence models with parameters describing atmospheric conditions as inputs. However, the existing models don't necessarily fit observed data well for all atmospheric conditions. This manuscript includes many additional atmospheric parameters as predictors to estimate the coherence and also applies a machine learning approach to model wind evolution. The advantage of the machine learning approach is that the set of parameters used to predict wind evolution can be adapted to the measurements available at a given location. The manuscript describes novel and relevant research, and overall is well written.

Despite the significance of the research, there are several areas that I believe should be addressed. First of all, it would be useful to understand how the accuracy of the developed model compares to existing wind evolution models (e.g., Kristensen, 1979; Simley and Pao, 2015; possibly Davoust and von Terzi, 2016). The manuscript claims that the developed model is sufficiently accurate to model wind evolution, but if possible, it would be interesting to know how much it improves over these simpler models.

Second, the manuscript is very well organized and easy to follow! But the English usage could be improved throughout the manuscript. For example, there are several sentence fragments, the word "the" is used in many places where it is not needed, and some of the language seems too casual (e.g., pg. 10, ln. 255: "Think of making a regression model from some data.").

My biggest concern with the manuscript is that the analysis assumes that the spatial averaging effect of the lidar can be ignored (discussed on pgs. 7 and 8). The authors correctly show that the lidar weighting function does not affect the measured coherence as long as it is assumed that wind evolution can be ignored within the probe volume (Taylor's hypothesis is applied). But this over-simplifies the problem. For example, the authors are estimating the wind evolution between the two adjacent range gates, with range gate spacing as low as 27.5 m. But pulsed lidars typically have a Full Width at Half Maximum width of $\sim$30 m. Therefore, it seems problematic to assume Taylor's hypothesis within the 30 m probe volume, but assume wind evolution between the two range gates separated by a similar distance. From my own analysis of the impact of spatial averaging on the measured coherence, when the wind evolution model is applied within the probe volume as well as between range gates, the presence of the weighting function significantly impacts the measured coherence. This has the effect of increasing the low frequency coherence but causing the high frequency coherence to decay much faster. Therefore, it seems likely that ignoring spatial averaging altogether in this work leads to incorrectly fitting the coherence model.

The authors should include some analysis comparing the modeled coherence with and without wind evolution within the probe volume, using the results to either justify their approach or to show that Taylor's hypothesis cannot be ignored. A better approach would be to include the impact of spatial averaging and find the a and b parameters that best fit the measured coherence when the wind evolution model is combined with the spatial averaging model. In principle, this approach is similar to the method developed by Schlipf et al., 2015 (Meteorologische Zeitschrift), but much simpler since only a staring lidar mode is used.

Specific comments:

-Pg. 2, ln. 58: "adapted the Pielke and Panofsky's model by introducing a new parameter..." More accurately, the paper by Simley and Pao (2015) took the form of the coherence model for transverse and vertical separations suggested by the following paper, and adapted it to longitudinal coherence:

R. Thresher, W. Holley, C. Smith, N. Jafarey, and S.-R. Lin, "Modeling the response of wind turbines to atmospheric turbulence," Department of Mechanical Engineering, Oregon State University, RL0/2227-81/2, Corvallis, OR, Tech. Rep., Aug. 1981.

-Section 1: Introduction: Another very relevant paper should be discussed in the literature review section. The following paper discusses fitting lidar-measured coherence to the longitudinal coherence structure suggested in Simley and Pao (2015):

Analysis of wind coherence in the longitudinal direction using turbine mounted lidar S. Davoust and D. von Terzi 2016 J. Phys.: Conf. Ser. 753 072005

-Eq. 9: The last index "j" should be changed to "i".

-Pg. 8, ln. 188: This paragraph and Fig. 4 are hard to follow. I would suggest labeling the angles the text refers to in the figure, and also provide some equations to support what you are trying to explain.

-Pg. 8, ln. 196 - pg. 9, ln. 208: In this discussion, it is a little hard to tell if yaw misalignment is required by the coherence estimation method, or if it is optional. This

Printer-friendly version

Discussion paper

becomes obvious later, but I think here it would be good to explain that the final model allows different combinations of predictors (including yaw misalignment) depending on availability.

-Section 2.5: Can you compare the predictors you are using to the predictors used in previous longitudinal coherence models in the literature (e.g., Kristensen, 1979; Simley and Pao, 2015)? It would be insightful to understand which new parameters are included in this study.

-Pg. 9, ln. 213: It would be good to define turbulence intensity here.

-Pg. 10, ln. 233: "thus how likely or to what extent the local terrain changes" makes it sound like the terrain variations are the primary reason the coherence would depend on "d". But even if the terrain stays the same, I would still think there could be a dependence on "d".

-Pg. 10, ln. 234: "For prediction, it is not possible to obtain Delta t_maxcorr." Why can't it be determined? It can be calculated just like all the other predictors, right? Also, on pg. 6, ln. 130, you say that the d/U approximation is not used in this study and Delta t_maxcorr is used. Which of these statements is right?

-Pg. 10, ln. 251: (Chen, 2019). Can you describe how this current manuscript compares to the earlier work? Better yet would be to discuss this in the introduction.

-Pg. 11: Section: "Hyperparameters of GPR": In general, this section would be more clear if the specific variables discussed were connected to the wind evolution application.

-Pg. 11, ln. 271: "where x is the input vector of different parameters" Can you provide an example of what these input parameters are in your application?

-Pg. 12, ln. 278: "where X is the aggregation of all input vectors." Can you explain in more detail? What are the dimensions of X? # of parameters x # of observations?

-Eq. 19: I did not see these basis functions or the coefficients beta discussed anymore in the manuscript. Can you describe how you chose the basis functions and how the coefficients were estimated? And how do these values affect the final estimate in Eq. 22?

-Pg. 12, ln. 286: Please describe in more detail why you are using a kernel function (I assume because then you don't need to actually define the functions "phi(x)").

-Pg. 12, ln. 288-290: I don't think these sentences are needed, since in the next paragraph, you thoroughly introduce the ARD-SE kernel.

-Pg. 12, ln. 291: Why is the ARD-SE kernel chosen? And please provide a reference about this kernel.

-Eq. 21: Please define "D".

-Pg. 12, ln. 296: "A relatively large length scale indicates a relatively small variation along corresponding dimensions in the function" From Eq. 21, it seems more accurate to say that a large length scale relative to the amount of variability in the predictor indicates a smaller variation along the corresponding dimensions. For example, it seems the size of the length scale is only meaningful by itself if all of the predictors have been normalized to the same std. dev. Is this correct?

-Eq. 22: Can you state the difference between X and X_* here? Also, this equation seems to just be saying that the conditional distribution is normally distributed, so I don't think the right hand side of the equation adds anything. Perhaps it would be less confusing to just explain that the function values are estimated given input parameters X_* by conditioning f on the training parameters and observations, X and y, as well as X_*: f_*| X, y, X_*. Finally, how are the estimates formed from the resulting distribution? Is the mean value used?

-Table 2: The lidar weighting function width (e.g., Full Width at Half Maximum) would be a relevant parameter to list in this table.

-Pg. 15, ln. 367: "The threshold for both are 6 m/s and 3 m/s". How are the thresholds used? For example, are these the thresholds in terms of deviation from the mean value of the three-data point window? Also, how is the standard deviation defined here? How many data points are used to calculate the std. dev.?

-Pg. 18, ln. 436: "all the PDFs supported by MATLAB" Is there a particular MATLAB toolbox you are referring to here? Also please provide a reference for MATLAB.

-Pg. 20, ln. 465: "all the fitted curves of the coherence are grouped together proves it is reasonable to model the wind evolution based on dimensionless frequency". Do you mean that they are grouped together at high frequencies (f_dless > 0.1)? Additionally, "proves" seems like a strong statement here. Maybe "suggests"?

-Fig. 9: The caption should refer to the different subplots that are labeled (a-d).

-Pg. 22, ln. 486: "all the potential predictors are included... to determine the characteristic length scale". Please describe how the training to find the length scales is performed.

-Pg. 22, ln. 489: "Figure 11 illustrates a comparison among the log(sigma_m)." As mentioned earlier, it doesn't seem fair to compare the sigma_m magnitudes unless all of the predictor variables have been normalized to have the same std. dev. (or some other normalization). Is this done?

-Table 4: Please explain "standard deviation of observed responses" in more detail. It's not clear what the "observed responses" are.

-Fig. 13: I'm not sure how to interpret this figure. Are there errors in the plots or the legend? The legend lists separate solid lines and dotted lines, but I don't see both in the plots. Do they perfectly overlap? Additionally, the legend says that blue dotted is a fitted case and solid red is the predicted case. But these two lines are very far apart, which does not support the claim that the fitted and predicted curves are very close. Additionally, what is the significance of the particular period being shown here. Is this

Printer-friendly version

Discussion paper

one of the periods with the best match between the fitted and predicted coherence? Or is it representative of a typical case?

-Pg. 26, ln. 556: "Rˆ2 at least over 0.65." What is the significance of 0.65 as an indication that the prediction accuracy is "satisfactory"?

-Pg. 26, ln. 573: "no obvious relevance between the error and values of any of the predictors is indicated in both figures." I don't quite agree. I think there are some interesting trends, like in Fig. 14 (b), the RMS prediction error decreases as sigma_l increases. Trends can also be observed in Fig. 15 (b) and (f).

-Pg. 29, ln. 612: "capable of achieving a parameterization model with sufficient accuracy for the prediction of wind evolution." This seems like a very strong statement to make. Please provide some more context for this statement. How is "sufficient accuracy" determined?

-Pg. 29, ln. 616: "methods to improve the estimation of the coherence and the wind statistics are desired." What are some of the shortcomings of your current approaches that you think could be improved?

-Fig. A1-A5: I think if there are appendix figures, they should be in a labeled appendix section.

-Pg. 30, ln. 623: Is the current computational time acceptable for real-time applications?

-Section 6: If possible, it would be nice to hear your thoughts on whether the chosen coherence formula structure (Eq. 5) could be improved. In other words, the paper mostly focuses on how to estimate the a and b parameters, assuming Eq. 5 is the right model. But is this model good enough? For example, Simley and Pao (2015) show that this kind of model did not fit stable atmospheric conditions very well.

Interactive comment on Wind Energ. Sci. Discuss., https://doi.org/10.5194/wes-2020-50, 2020.