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Interactive comment

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

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"General comments"

Chen et al. develop a method to predict the coherence of horizontal wind velocity fluctuations for mostly longitudinal separations. Their predictions are based on first to fourth order wind speed statistics that can be calculated from either nacelle-mounted lidar or mast-based in-situ anemometry. They use data from two measurement campaigns to test their approach and find good results that are especially relevant for lidar-assisted wind turbine control. The work lies therefore well in the scope of WES and is of broad international interest. The paper builds up on an existing wind evolution model and presents a novel approach to parameterise its two coefficients by means of machine learning. The manuscript explains the study thoroughly and reproducibly, presents all relevant results and discusses them critically.

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Section 2.6 "Gaussian Process Regression" lies outside my field of expertise and I can therefore not evaluate if the chosen model is suitable for the task of parametrization the wind evolution model. The manuscript is overall understandable but would benefit greatly from being proofread by a native English speaker or similarly qualified person before publication. I recommend reconsideration for publication after major revisions.

"Specific comments"

l.2: I assume you mean "the mean flow" (also l.13 and all other occurrences).

l. 50: The introduction would benefit from references to research that support Taylor's frozen turbulence for very large turbulent structures but limit its applicability for long separation distances or small scale turbulence such as Willis and Deardorff (1976), Schlipf et al. (2010), and Kelberlau and Mann (2019).

l. 59: "the vertical intercept" It would be better to describe the second parameter without referring to the coherence-frequency plot that is not yet introduced here.

l. 61: "Mann spectral velocity tensor" Mann (1994) should be cited here.

l. 68: "If any data... is also available..." Please mention which data is available or would be of interest.

l. 94: Please introduce this travel time as a function of the mean wind speed here.

l. 97: Please explain why "it is not possible to predict every point of the coherence curve". For my understanding, the coherence curve is visible on a plot like in Fig. 1. Do you refer to not having not enough data to smoothen the curve or not having data for all separation distances?

l. 100: Do you mean "...according to measured wind velocity time series by a parameterisation model"?

Fig. 1 and Fig. 2: In general, it is good to visualize the workflow like done here. But both figures show overlapping information and I recommend to merge them into one figure. The numbering used in Fig. 2 with an explanation in the text and caption(!) is more informative than the keywords currently used in Fig. 1. A figure and its caption should be self explanatory whenever possible. Please try to improve the text l. 98-106 for better understanding.

l. 120: Please describe which frequency you are referring to. Probably the frequency of the horizontal wind velocity fluctuations. Maybe also introduce the wavenumber k here that is used as a measure of eddy size in many other publications.

Eq.(5): It is not clear where (5) comes from. If you do not want to include the complete deduction, I suggest to give a reference that shows it and uses the same form of the equation. In Simley and Pao (2015), a and b are defined a bit differently, I think.

l. 147: You should include the weighting here: e.g. "...but the weighted average of the wind speeds within the measurement volume"

l. 148: This is a bit ambiguous because spatial averaging does also refer to combining data from different measurement volumes in different lidar beam directions. Better write: "so-called line-of-sight averaging effect of lidar". (also l. 156)

l. 151: Please refer to more fundamental work (Nyquist-Shannon sampling theorem).

l. 152: You should mention the sampling rate of the lidars here (not only in the table) and compare it with the frequency of the eddies that you want to detect.

l. 158: The line-of-sight weighting of a pulsed lidar is usually approximated by a triangular function as in e.g. (Sathe and Mann (2012)) which is a sinc^2 function in the frequency domain.

l. 184: It should be considered that w(x) is approximately 0 for fluctuations that occur with a wavelength of twice the length of the illuminated section of the lidar beam (or length of the range gate). In this case the measurement signal would be determined by noise only. I suggest to estimate a range of critical frequencies based on the length of the range gates. This range of critical frequencies should be considered in the further analysis, if it is relevant for the results. Your derivation assumes furthermore that the weighting function is identical for all range gates. This is only true if the laser beam is well collimated. Is this the case for the lidar devices used in this study?

l. 199: You could mention that a lidar with additional beams would help here and could also be used to avoid yaw-misalignment.

l. 208: What is the expected order of magnitude for the misalignment angle? How much "decorrelation" do you expect from a turbulence model (e.g. Mann (1994)) due to the resulting lateral separation? Can you quantify the order of magnitude of the resulting error approximately?

l. 213: Please always write which variable you are referring to when you mention standard deviation sigma.

l. 236: It would be good to introduce the variable alpha already in 2.4 (l. 199 and Fig. 4)if you refer to it here.

l. 263-266: This sentence is very long and difficult to understand.

l. 332-343: Please reassess which information should be given here: I miss: the measurement height of the lidar, length of each range gate, measurement distances... Some of these values are given in Table 2 but should also appear here. The information about the coordinate system will not be used again later in the text and do not need to be given at all then.

l. 342 and 350: Main wind direction refers usually to the direction from where the wind blows most frequently. Better write mean wind direction.

l. 389: I suggest a similar filtering against the line-of-sight averaging. See comment for l.184. Probably it is not worth it to re-run the computations. But check in the coherence plots, if the frequency range is relevant and if you see a random increase in coherence in it.

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l. 397: Why do you not also filter the lowest percentile?

Fig. 6: Subfigure (e) would benefit from a zoom into just some few minutes of data with thin plot lines to show all velocities clearly and not on top of each other.

l. 445: That makes the difference caused by the reduced sampling rate in Fig. 7 (b) even more interesting. Earlier you write that "As long as the sampling rate of lidar is sufficiently high to acquire a complete coherence curve, it will not have a noticeable effect on the study of the coherence." but here you find that the influence on parameter b is big (logarithmic y-axis). What could be the reason? f_s=1/3Hz means, at least at high wind speeds, that it is difficult to measure exactly after the eddy travel time passed. That means you might miss the best moment to take your measurement. Could this maybe have to do with the results?

l. 462: This is a very interesting finding! Better write that the coherence is dependent on the separation distance but independent of the measurement distance, i.e., the position in the induction zone.

l. 471 ff.: You point out that "The decay parameter a shows a decreasing trend with increasing measuring separation." After reading the explanation given in lines 475–480 several times I still do not fully understand why longer travel time (or separation distance) leads to a less decay. And if I could follow the explanation, it would not explain why the value increases again for very long separations with ParkCast data. Maybe you can explain this better?

l. 489: You should explain the results shown in Fig. 11 at least briefly to justify your predictor selection shown in Table 5. Please mention the log(σ_m) thresholds.

Fig. 12: The scatter plots are not as informative as they could be. Please decrease the marker size and maybe add transparency to make it possible to see the density of the data points. Also a regression line would help to quantify the relation between x and y.

Table 5: Please provide a better caption. What does for example bold font mean?

l. 564: The prediction accuracy is very good for this one example case but from Fig. 12 we know that the scatter throughout the whole dataset is quite high. Is this particular example (12.12.2013, 12:00–12:30) representative in any way? Maybe it is more interesting to show a plot for a case where the deviation between modelled and fitted curves equals the RMSE?

l. 599: The theory about atmospheric stability in section 2.5 can be removed then.

l. 625: The computation time was not mentioned before. Rather don't mention it only in the conclusions.

Fig. A2: Please add a grid and either add x-ticks or if that is not possible out of confidentiality concerns, remove [Hz] and [m/s^2] from the labels.

"Technical corrections"

l. 16: "taking values", "1..."

l. 45: "lidar is a remote sensing technology"

l. 67: Remove "Some"

l. 81: Remove "again"

Fig. 1: The text in the plot is too small. Maybe simply enlarge the whole plot a bit.

Fig. 2: The caption is a stub.

l. 109: "...with only a few simple parameters." or better: "... with as few parameters as possible."

l. 114: "a linear function or a more complicated term."

l. 127: Please check all occurrences of "the both" and use either "the two" or only "both" instead.

l. 131: "dimensionless frequency" should be written in roman script.

l. 141: "projected onto"

l. 150: Please check all occurrences of "starring" and change them to "staring".

l. 151: Please capitalize "Oppenheim".

l. 155: "...laser beam pointing into a fixed direction."

l. 200: "Since..." the sentence is not correct. Please rewrite.

l. 234: "So as the travel time Dt" is not a sentence. Please rewrite.

Table 1: Unusual table style. Consider three columns including a header for each column instead of lines separating the rows.

l. 351: "sampling rateS because of THEIR"

Table 2: The Matlab like interval syntax is maybe not the best way to present the measurement distances. Better write, e.g., 30,90,...,990, Dec is abbreviated, June is not.

l. 375: What does C stand for? Probably a formatting error here?

l. 379: and 381: No new paragraphs here.

l. 399: "referred to throughout"

l. 419: Remove both occurrences of "as"

l. 459: The idea for colours and markers is good but most of it is not visible in the plot. Try slightly thinner lines and different marker sizes for different colours (e.g. blue circle tiny, red circle small, yellow circle medium...).

Figure 9: The caption does not explain the difference between a) and c) and between b) and d).

l. 579: "And the more noisy..."

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l. 586: "nacelle-based"

l. 611: "error is" or "errors are"

References:

Willis and Deardorff (1976): 10.1002/qj.49710243411

Schlipf et al. (2010): 10.18419/opus-3915

Kelberlau and Mann (2019): 10.5194/amt-12-1871-2019

Mann (1994): 10.1017/S0022112094001886

Sathe and Mann (2012): 10.1029/2011JD016786

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