Wind Energ. Sci. Discuss.,
https://doi.org/10.5194/wes-2020-50-RC3, 2020

WESD

Interactive comment

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

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Received and published: 20 April 2020

review of "Parameterization of Wind Evolution using Lidar"

DRAFT WES 2020-50, SUBMITTED BY Y.CHEN, D.SCHLIPF, AND P.W.CHENG

Reviewed by M.Kelly, 13 Apr. 2020

General comments

This work examines evolution of advected turbulence, in terms of spectral coherence, with the motivation of wind turbine control assisted by inflow measured by lidar. The topic is quite relevant to wind energy, and fits well with the journal (WES).

There is some interesting content and potentially useful results, with the use of GPR

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Discussion paper

and Bayesian inference being quite nice.

Unfortunately the paper appears to be somewhat ‘unfinished’; perhaps it is also due to the lack of English fluency or preparation time. For example, the abstract has simply copy-pasted a few sentences from the paper, and repeats in a cumbersome way: *‘This paper aims to achieve parameterization model for the wind evolution model to predict the wind evolution model parameters’*. The paper needs to be proofread by somebody with English fluency, at any rate.

The abstract does not clearly provide an idea of the work done, and while the text has more detail, it is not clear throughout; I am not sure that readers could repeat what has been done.

More importantly, there are inconsistencies that have not been considered, and should be addressed/rectified; perhaps most significant are the form itself chosen for coherence (see derivations below in *Specific comments*), and the use of Taylor’s hypothesis for some (but not other) parts of the model/parameterization. There are a number of details and also explanations which are missing, but which could hopefully be included, to make the work publishable. The results/performance are a bit overstated (in English), but this is not needed, as the numberical results presented tell the story less subjectively—and are good to share with the wind energy community, provided that they are given with sufficient detail, replicability, and consideration.

Specific comments

Line 15 and elsewhere later in the paper: while the authors define ‘wind evolution’ as squared coherence, they imprecisely define such (e.g. *“Coherence is a dimensionless statistic in the frequency domain”*). Specifically, this should be written as temporal coherence (time shift, frequency domain), in contrast with spatial (wavenumber spectra) coherence. Further, coherence does not describe the “correlation between two signals”, but rather the correlation between spectral components of two signals.

Do lines 19–20 not imply that use of Taylor's hypothesis means ignoring wind evolution? This may be relevant, for consistency later (line 235).

Line 35: The statement "*dependence of coherence on separation and atmospheric stability was not adequately researched*" lacks reference and/or explanation. It was not adequate, according to whom, or how?

Line 36–38: You write "*The longitudinal coherence differs from the lateral and vertical coherence because the former measures the correlation with respect to time lag while the latter with respect to spatial separation.*" This is not correct: longitudinal, lateral, and vertical coherences all depend on f, based on integration over time lags; they are depending on spatial separations in the respective directions. For the longitudinal coherence to give spectral correlations 'with respect to time lag' Δt, then the longitudinal separation is related to Δt in some way, though you have stated before this point that you are not using Taylor's hypothesis.

Line 112/equation 2: how is this a function of frequency (f)? I.e., include the f-dependence on LHS, and also within τ on RHS.

There appears to be incompatibility between Eqs. 3–4 and Eq. 5; in particular (5) is missing σ and U. Further, in Section 4.2 and Fig. 10, you analyze the behavior of a with $\Delta t_{\mathrm{maxcorr}}$ (stating $a \propto \Delta t_{\mathrm{maxcorr}}^{-0.49}$), but do not consider that the equations already imply a Δt dependence.
That is, eqns. 2 and 5 give

$$C\Delta t/\tau = \sqrt{(af\Delta t)^2 + b^2};$$

however (3)–(4) with (2) give

$$(CI_u f\Delta t)^2 = (af\Delta t)^2 + b^2$$

where the turbulence intensity is defined $I_u \equiv \sigma/U$. Thus one sees that

$$a = \sqrt{(CI_u)^2 - \left(b/f_{\text{dless}}\right)^2} = CI_u\sqrt{1 - \left(\frac{b}{C\Delta t/\tau}\right)^2}.$$

In the limit of high dimensionless frequency or large turbulence intensity, i.e. without the offset b, then $a \to CI_u$ like Pielke & Panofsky (1970). But in the limit of small turbulence intensity or low dimensionless frequency (small $f\Delta t$), i.e. a large offset, then we see a become imaginary, implying a (nonphysical) coherence oscillating with Δt or Δx.
The form (5) is the same as that of (4) in Simley & Pao (2015), with d_ℓ in the latter replaced here by $U\Delta t$, and b here replacing their abd_ℓ; this should be noted, and the text is not quite clear nor correct following your Eqn. 5. You do note the reason for keeping Δt (instead of using $\Delta t = d_\ell/U$), but why did you drop the spatial separation dependence (d_ℓ) from the 'b' part of the Simley & Pao (2015) expression? From your logic for the 'a' term, then instead of just b you would have bd_ℓ (but not $bU\Delta t$).

In lines 145-9: your text is a bit imprecise here—the sonic anemometer has a measuring volume as well (not a point), it is just much smaller than the lidar's. Also, among the reasons why longitudinal coherence from lidar deviates from that calculated via sonic anemometers, one key possibility is missing: the validity of Taylor's hypothesis.

Line 153: what do you mean by "complete coherence curve"?

Lines 157–164: please include references, as this is not original.

Line 184: neglegt of the spatial averaging effect and $w(x)$ in $\gamma^2_{i,j}$ also demands use of Taylor's hypothesis. This should be noted (along with its potential inconsistency).

Lines 193–5: the sentence *"To retrieve the longitudinal coherence in this case, the above discussed spatial averaging effect must be coupled to a specific turbulence model (Schlipf, 2015; Mann et al., 2009), and thus the wind evolution model is included in the final model implicitly"* does not quite make sense. Could you clarify?

Line 217: by "*its definition*", do you mean the definition analogous to (13), where the time-lag s is replaced by spatial separation r?

Lines 217–19: If you say $L_{int} = T_{int}U$, then aren't you just using U as a potential predictor somehow? Also, isn't this inconsistent with the previous section, where you state that Taylor's hypothesis is to be avoided? Or, is Taylor's hypothesis avoided only for certain aspects? Please clarify.

Line 223: The statement "*atmospheric stability represents a global effect of the boundary layer on the wind field*" is not quite correct.
From what you measure, or via M-O similarity, it is a 'global' effect from the surface, and potentially only through part of the ABL (sometimes not even above the surface layer in stable conditions).

Line 234: "*So as the travel time* Δt*.*" is not a sentence. What are you trying to convey here?

Line 235: So you are meaning that distance d is used instead as a predictor.

Line 250: by "*performs the best*", perhaps you should use 'performs well'—unless you explain what 'best' means (i.e. what other models).

Line 257: The phrase "*underlying functions of the data*" is not clear. Do you mean behavior conditioned on other variables, or relation to other variables?

Table 1/ line 255+ : Is it even possible to use the fourth or even third moment, given the large sampling uncertainty involved for highter-order moments? Please see and reference e.g. Lenschow, Mann & Kristensen (1994) and Ch.2 of Wyngaard's textbook (2010), to understand and defend use of μ_4—let alone μ_3.

Line 284: To be clear and consistent, can you not specify that β is a weight, and the 'basis function' $h(\mathbf{x})$ maps the means into the new space?
What is meant by '*Basis function is one of the hyperparameters*'? I.e., how is a function a parameter, or is $h(\mathbf{x})$ already assumed to have some form, possibly related to the $\phi(\mathbf{x})$

forms?

lines 289–296: σ_m is not a 'length' in the physical sense; it has units of whatever x_m has. Thus it is a characteristic magnitude for the predictor having index m.

lines 299–302: To be explicit, the RHS of (22) does not contain a 'conditioning bar'. I.e. to help the reader and match the text, show in the math that the joint Gaussian prior is conditioned on X_* (is the eqn. correct?). Most readers will not have read Duvenaud's PhD thesis, so it is useful to help them understand.

Lines 307–311: why k =5? IF it is due to needing a large enough sample for verification, then this should be stated.

Line 335: was the lidar on the nacelle, at what height?

Line 350: please be more clear and specific, and also include references.

Line 362–366: Why are two different filtering types used?
State why -24dB for CNR; include reference.

Line 375: Using "C_N^2" to symbolically write 'N choose 2' unique pairs, is not standard practice. You can write $\binom{N}{2}$ or equivalently $N(N-1)/2$.

Line 479–480: need citation for Levenberg-Marquardt algorithm.

Fig.9 caption: mention which plots belong to which campaign.

Fig.11 / § 5.1 : why not plot σ_m^{-2}? This is what is actually used in the ARD-SE kernel shown in eqn.21, and its behavior more clearly demonstrates relevance.

Line 489–90: you state "*predictors are selected according to different preset limits of the log(σ_m) considering different cases of application or data availability*", but what are these preset limits?

Line 494 / Table 4: how/why did you choose the initial σ_m =10?

Line 509: I am not sure that R^2 of 0.65 is "*satisfactory*"; perhaps this could be just written in terms of R^2 and RMSE without the subjective claim. Also, "*all situations*" is not quite consistent with just the recommended cases (i.e. it implies all cases).

Lines 519–520: This is a good point, and it would be useful to repeat this earlier, when introducing the potential predictor variables because some of them appear redundant.

Lines 524–527: It appears that you are conflating two things here, one of which you are missing—**applicability of Taylor's hypothesis** will also affect L compared to T via U, whereas this is not the case for the usage of U to 'convert' σ_u to I_T.

Lines 532–534: perhaps μ_3 or μ_4 could help prediction; but to be responsible, one needs to mention that [1] the uncertainty in these quantities are very large (Lenschow et al. 1994 reference), and [2] lidar may not be able to consistently measure these. Further, these higher-order moments are likely more affected by your filtering.

Lines 537–539: this is likely due to the implicit co-dependence I derived above, i.e. a is a function of CI_T and b/f_{dless}. Your finding confirms also the need/utility to consider the behvior of the parameters involved.

Lines 542–554: what about cross-comparison using the sonic? Were the wind directions such that the sonic (at 270m upstream) could be compared to the lidar (e.g. at 163.5m upstrean)?

Line 555-6: isn't this '*satisfactory*' $R^2 \geq 0.65$ only true for certain cases and variables?

Figure 14 / Line 560 and afterward: these plots do not responsibly/transparently show prediction error, as they don't give an idea of the magnitude of a.
You should plot percentage error or similar; given that a can be small depending on b and I_T (as derived above), the plotted differences in a might be relatively significant.

Lines 574–5: if the whiskers are large because of sample size, then why not (also) account for this via $\sqrt{n}$?

Line 576: The claim "*it is proven that the Gaussian process regression is capable* *of achieving an accurate parameterization model*" is an overstatement. It is DEMONSTRATED (not proven) that the GPR was able to predict two coherence model parameters with an $R^2 \geq 0.65$ in chosen cases (not simply 'accurate').

Technical corrections

There are many English usage/grammatical corrections and suggestions, which are included in the attached annotated PDF-file. I thus only include a sample of them here in this list. The sentence structure and writing is unclear or ambiguous in numerous places; the paper really should be reviewed and edited by somebody with adequate fluency.

I list some of the specific corrections below, but since there are >300, after the first few I include the line numbers, which refer to the annotated attached PDF. After page 16 I did not correct much English; this is left to the authors for the next draft.

- Abstract/line 1: One (generally) shouldn't copy sentences from the introduction into the abstract (further, this first sentence is a definition); 'turbulence' should be 'turbulent'; pluralize 'structure'; delete 'of the eddies'; replace 'while the eddies' with e.g. 'as they'; replace 'by the main flow over' with 'through'.
- L.2–4: Remove 'the'; change 'because' to ':'; remove 'only'...see annotated PDF for more details.
- L.5–7: These 2 sentences are quite unwieldy (cumbersome) and also somewhat tautological—particularly for an abstract, also with repeated phrases that need to be reduced/condensed. Please correct the English usage here.
- L.12–13: First sentence can be corrected from "*Wind evolution refers to the physical phenomenon that the turbulence structure of the eddies changes over time while the eddies are advected by the main flow over space.*" to something

like ‘Wind evolution’ refers to the physical phenomenon of turbulence structures (eddies) changing over time, while the eddies are advected through space by the mean flow..

- L.13-15: change “The mathematical” to ‘A common statistical’; delete ‘usually’; ‘hereinafter for brefity, also’ should be ‘hereafter’; delete ‘two time series data sets of the’ and instead add ‘measured at two different locations,’ after ‘velocity’; change ‘with certain time shift’ to ‘calculated over varying time shifts’.
- L.17–19
- L.22
- L.25–27
- L.29
- L.32: run-on sentence; use parentheses as noted
- L.33–34
- L.36–38
- L.40, L.42–44
- L.47–48
- L.52 use of definite articles and parentheses
- L.55–59, L.61
- L.63 delete ‘model’
- L.65, 67

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- L.73–74
- L.79–81: delete a number of redundant words, add punctuation as noted
- L.122–123
- L.136
- L.151 Capitalize ‘Oppenheim’, here and elsewhere.
- L.156
- L.169
- L.177: have already introduced U as mean wind speed (delete here).
- Page 8: L.186–7; 189; 196–9; 201–2
- Page 9: L.204; 210; 212; 220–2;
- L.224 and elsewhere: not ‘Monin-Obukhov length’, just use ‘Obukhov length’
- Page 10: L.225–8; 233; 250–7
- Page 11: L.258–9; 261; 264; 269; 272
- Page 12: L.275–8; 288–9; 292; 295–6; 302; 304–5
- Page 13: L.308
- Page 14: L.335; 339; 346–7; 349–353; 363–5
- Page 15: L.367
- Page 20: L.470; 479

- Page 24, Table 5: Taylor is italicized under case 6, but should be Roman font.
- Page 25: L.537; 550

Please also note the supplement to this comment:
https://www.wind-energ-sci-discuss.net/wes-2020-50/wes-2020-50-RC3-supplement.pdf

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