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Interactive comment on "Uncertainties identification of the blade-mounted lidar-based inflow wind speed measurements for robust feedback-feedforward control synthesis" *by* Róbert Ungurán et al.

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The authors present an uncertainties identification study for blade-mounted lidar wind speed measurements. In general, this is an interesting topic since using lidar for control is a promising technology. Based on the abstract and the conclusions, there are 3 main contributions:

1. Identification and modeling of uncertainties as frequency-dependent uncertain weights that can be employed in feedback–feedforward individual pitch and trailing edge flap control development and analysis.

2. Presentation of a method that can estimate the preview time online
3. Introduction of a simple method to calculate the telescope and lidar parameters.

The paper simulates three blade-mounted lidar systems. Most of the paper focuses on the analysis between two set of signals, where $k \in$ {col,yaw,tilt}:

1. $u_{\mathrm{cor},k}$: processed signals from the blade-mounted lidar systems.
2. $u_{\mathrm{beff},k}$: blade-effective wind speeds transformed into fixed frame.

The paper is mostly well written and the amount of work done is very impressive. Performing several large-eddy simulations created a nice environment for the intended contributions.

However, there are some issues in the analysis:

Uncertainties calculation

This is the main weakness of the work in my opinion. The issue and can be separated in two sub-problems:

1. The modeling and identification of the transfer function is not consistent.
2. The transfer function is used for a measure of uncertainty, which is not correct.

Inconsistency

Based on Figure 5 and Equation (11), the transfer function between d and d_Δ is $I + \Delta_\ell W_\ell$. Δ_ℓ seems to be a multiplicative uncertainty used in robust control to transform a system into a M-Δ structure. Usually this structure is then used to design controllers which are still stable for a multiplicative uncertainty $\Delta_\ell \leq 1$. For this purpose, the uncertainty weight needs to be identified using Equation (17) as a worst case. Here, the nominal model should be used, namely I. However, the authors use a first order low-pass filter, without further explications. Anyway, the whole identification of the uncertainty weight can be considered as a fit to $1 - G_\ell$. Please note, the uncertainty weight is not the multiplicative uncertainty Δ_ℓ.

Further, in the caption of Figure 5, $G_{\mathrm{d,f}}$ is named "disturbance model" and $G_{\mathrm{wt,f}}$ is named "wind turbine model". However, both should be part of the wind turbine: $G_{\mathrm{d,f}}$ is the part of the wind turbine which models how the disturbance affects the outputs. $G_{\mathrm{wt,f}}$ is the part of the wind turbine which models how the control inputs affect the outputs.

Measure of uncertainty

The author write on page 16: "Thus, 0% of uncertainty indicates that the identified transfer function (G_ℓ) from the blade effective wind speeds (u_{beff}) to the corrected lidar-based inflow wind speeds (u_{corr}) can have a gain of 1 in that frequency. Moreover, 10% of uncertainty means that the identified transfer function (G_ℓ) can have a gain of either 0.9 or 1.1 in that frequency."

In my opinion, the use of uncertainty is misleading here. If the uncertainty weight is 0 at a certain frequency, this means that the gain of the identified transfer function is equal to the nominal transfer function. If the uncertainty weight is 10% at a certain frequency, this means that the gain of the identified transfer function is within 10% of the gain of to the nominal transfer function. However, it does not give you any information about an uncertainty in the sense how well a lidar measures or not or how well the signal can be used for feedforward control.

Small example: Let's considered two signals, s_1 and s_2, where s_2 is generated by

passing signal s_1 through a linear low-pass filter. Applying the approach above will lead to a uncertainty weight of 0 at 0 Hz and will approach 1 for high frequencies. If now s_2 is the disturbance d acting on the plant and s_1 the signal d_Δ used for feedforward control, one could simply use the same linear low-pass filter to get perfect disturbance rejection.

In short: I fear the proposed method is not useful to describe the uncertainty for lidar measurements.

Please check the uncertainty modeling in your reference Dunne and Pao 2016 (using additional noise input) or the measurement error introduced in E. Simley and L. Pao, "Reducing LIDAR wind speed measurement error with optimal filtering," 2013 American Control Conference, Washington, DC, 2013, pp. 621-627..

Preview time estimation

Section 2.6 describes the procedure how the preview time is estimated. Here, the phase angle between $u_{\mathrm{cor},k}$ and $u_{\mathrm{beff},k}$ is used. It is not well explained, but still understandable that minimizing the absolute phase angle provides signals which are well aligned in time. Further, the weighting with the spectra S_k is a quite empirical approach, but might be considered to be an acceptable approach to estimate the preview time. However, dividing with the coherence seems strange to me. Since the coherence can become zero, this does not seem right. In my opinion, it also does not help much that later you explain that only frequencies up to 0.06 Hz are used, where the coherence is larger than zero. The use of the coherence in J is not explained. It also is not included in the integral in the denominator, so also can not be considered an empirical weight. It seems to be an additional, not well explained and maybe not necessary complexity. It is not clear why not usual methods to determine the preview time are used, such as the peak of the cross-correlation. In Held, D. P. and Mann, J.: Lidar Estimation of Rotor-

Effective Wind Speed – An Experimental Comparison, Wind Energ. Sci. Discuss. in review, 2019. the information theoretical delay estimator presented in Moddemeijer(1988) is proposed, which also seems to be more useful. Further, it is not clear how this method can "estimate the preview time online“ as claimed in second of the three main contributions of the paper.

Further, J is used in Figure 17 and Section 3.3.5 to optimize the telescope orientation. Lidar scan configuration has been done in several studies before based on different cost functions. Minimizing J with a fixed preview time might lead to somehow optimal telescope orientation angles for the selected preview time in terms of timing. However, it is not clear, how the optimization leads to useful signals with high measurement quality if e.g. the mean wind speed is chancing etc. Further, the method (running LES simulations and using J) does not seem to be a "simple method to calculate the telescope and lidar parameters“ as claimed in the third of the three main contributions of the paper.

Organization

The paper's organization can be improved by following points:

- Section 2.1 and 2.2. In these two sections, the lidar-simulation, the estimation of the blade-effective wind speed and the definition of the blade-effective wind speed are somehow mixed together. This was quite confusing to me. It is very important to understand, how the two sets of signals mentioned above have been obtained, since the whole study focuses on the analysis between them. It would be better to have three subsections:
  - New Section 2.1: lidar-simulation: all the text of Section 2.1 until page 6, line 9.

  - New Section 2.2: wind speed estimation: all the rest of Section 2.1 and text of Section 2.2 on page 7.
  - New Section 2.3: blade-effective wind speed: text of Section 2.2 on page 6.
- Similarly, in Section 2.3, you could also explain that MBC is also applied to the blade-effective wind speed.
- The paragraph about the control development (page 9), the remarks, the $G_{\mathrm{d,f}}^{-1}$ and the performance weight is not important for the rest of the paper and should be removed. Again, it seems to be an additional, not well explained and unnecessary complexity.
- Equation (6) and (7): Since the whole paper focus on the two sets of signals, Function f should be either explained in detail or simply avoided. Again, it seems to be an additional, not well explained and maybe not necessary complexity.
- Section 3.1 explains the simulation setup using PALM, which then seems to be used in Section 3.3. In Section 3.2 however, generic wind speed measurements are used. It would be better in my opinion to switch them.

Minor issues

- Page 6, line 12: to estimate $u_{\mathrm{h,est},i}$ from Equation (1) to (3), you also need to neglect the weighing function. This is missing in the assumptions leading to Equation (4). Further, the expression "the measured LOS can be corrected“ might be misleading, since the LOS are correct, you use Equation (4) to estimate or reconstruct the longitudinal wind speed.
- Several variables are introduced relatively late, e.g. k, $V_i(\xi)$.

- The variables are not consistently named: you use ”blade-effective wind speed“ for (1) the original $u_{\mathrm{beff},i}$ with i for blade 1, 2, and 3, as well as (2) for the transformed $u_{\mathrm{beff},k}$, for $k \in$ {col,yaw,tilt}.
- Section 2.6: It is not clear that $u_{\mathrm{cor},k}$ is delayed. The only delay introduced in Section 2.3 is for the pitch angles.
- The simulation time is not stated in Section 3.1, but might be interesting for all the frequency estimates. Sorry, if I missed that information somewhere else.
- Page 12, line 12: and not necessary.

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