Uncertainties identification of the blade-mounted lidar-based inflow wind speed measurements for robust feedback-feedforward control synthesis

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We thank the referee for the attention and enormous energy to read our work and write a detailed review which has helped us to improve the content of the paper. This document includes our response to reviewer comments. Furthermore, an additional document is attached, highlighting the changes that have been made.

Uncertainties calculation

Q 1: *The nominal model should be used, namely I. However, the authors use a first order low-pass filter, without further explications.*

Reply: The gain of the low-pass filter over the frequency of interest was 1. Using I or the low-pass filter leads to the same result. Due to also the second point "The transfer function is used for a measure of uncertainty, which is not correct" we have updated our modelling and have added some text to clarify this (see Section 2.6).

Q 2: *Please note, the uncertainty weight is not the multiplicative uncertainty Δ_ℓ. The transfer function is used for a measure of uncertainty, which is not correct.*

Reply:

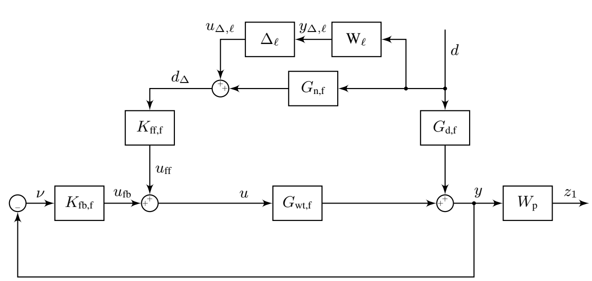
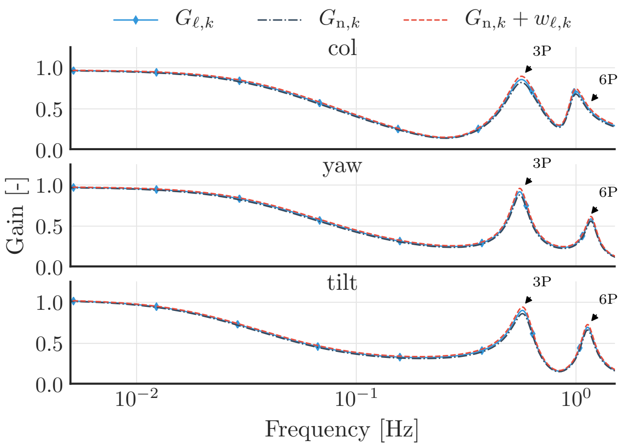
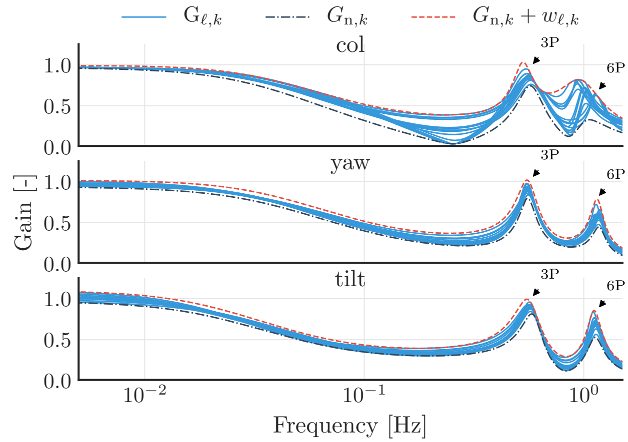


Figure A. Block diagram of the disturbance rejection control design with performance weight and uncertain input measurement. $K_{\mathrm{fb,f}}$, $K_{\mathrm{ff,f}}$ are the feedback and feedforward controllers, $G_{\mathrm{wt,f}}$ is the wind turbine model from the control input to output, $G_{\mathrm{d,f}}$ is the wind turbine model from the disturbance to the output, $G_{\mathrm{n,f}}$ is the nominal disturbance measurement model, Δ_ℓ is the uncertainty, W_ℓ is the measurement uncertainty weight, and W_{p} is the performance weight. The f in the index refers to the non-rotating (fixed) frame of reference.



(a) C_1: ideal modeling accuracy no-induction case.



(b) C_2: more realistic modeling accuracy, with uncertainties around the no-induction case

Figure B. The identified disturbance measurement transfer functions ($G_{\ell,k}(j\,\omega)$). The dashed-dotted line indicates the estimated nominal disturbance measurement models ($G_{\mathrm{n},k}(j\,\omega)$). The dashed line shows the sum of the estimated nominal disturbance measurement models and uncertainty weights ($G_{\mathrm{n},k}(j\,\omega) + w_{\ell,k}(j\,\omega)$), where $k \in \{$col, yaw, tilt$\}$.

This is a very good point and we have changed the modelling in accordance with Figure A, where we added a nominal disturbance measurement transfer function ($G_{\mathrm{n,f}}$) in parallel to the uncertainty weight (W_ℓ), leading to an additive disturbance measurement uncertainty modelling.

We repeated our investigation and first we established the transfer functions (G_ℓ) from the blade effective wind speeds (u_{beff}) to the corrected lidar based inflow wind speeds (u_{cor}), then we separately identified the nominal disturbance measurement model ($G_{\mathrm{n},k}(j\,\omega)$) and the uncertainty weight ($w_{\ell,k}(j\,\omega)$) as a 5th-order minimum phase filter for each of the inputs in such a way as to satisfy the following inequalities

$$|G_{\mathrm{n},k}(j\,\omega)| \quad < \quad |G_{\ell,k}(j\,\omega)|,\ \forall\omega, \tag{1}$$

and

$$|G_{\mathrm{n},k}(j\,\omega) + w_{\ell,k}(j\,\omega)| \quad > \quad |G_{\ell,k}(j\,\omega)|,\ \forall\omega, \tag{2}$$

with $k \in \{$col, yaw, tilt$\}$.

For example, this led to the results shown in Figure B. The figure highlights, that $G_{\mathrm{n},k}$ and $G_{\mathrm{n},k} + w_{\ell,k}$) are the lower and upper bounds of $G_{\ell,k}$. Although C_1 is useful to see how good our disturbance measurement (how far the magnitude of $G_{\ell,k}(j\,\omega)$ is from 1), it is to optimistic, any variation in the telescope parameters or inflow wind condition could result in $G_{\ell,k}(j\,\omega)$ being outside of the bounds. In contrast, C_2 covers a wide range of telescope parameter variation, and hence, if for some reason one or more lidar or telescope parameters cannot be selected as for the no-induction case, but close to these values, the established transfer functions from C_2 can be used for robust feedback–feedforward control development. In addition, C_2 also covers the cases where the mean blade pitch angle is increased or decreased because the wind turbine is at a different operating point.

The updated modelling is described in Section 2.6. and it also addresses the "Measure of uncertainty" comments in page 3.

Q 3: *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.*

Reply: We named these based on Skogestad and Postlethwaite (2005), where they call the "disturbance model" as the transfer function from the disturbance to the output and the "plant model" as the transfer function from the control signal to the output. But our naming could be confusing, hence, we change the caption of Figure 5 as:

Block diagram of the disturbance rejection control design with performance weight and uncertain input measurement. $K_{fb,f}$, $K_{ff,f}$ *are the feedback and feedforward controllers,* $G_{wt,f}$ *is the wind turbine model from the control input to output,* $G_{d,f}$ *is the wind turbine model from the disturbance to the output,* $G_{n,f}$ *is the nominal disturbance measurement model,* Δ_ℓ *is the uncertainty,* W_ℓ *is the measurement uncertainty weight, and* W_p *is the performance weight. The* f *in the index refers to the non-rotating (fixed) frame of reference.*

Preview time estimation

Q 4: *Section 2.6 describes the procedure how the preview time is estimated. Here, the phase angle between* $u_{\mathrm{cor},k}$ *and* $u_{\mathrm{bef},k}$ *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.*

Reply: Thank you for this comment, we had added the coherence as a weight, but we missed including it in the denominator. We wanted to give more emphasis to the phase shift where the coherence is high. We agreed that this was an unnecessary complexity. We have switched to the method you suggested (cross-correlation) which is a more straightforward way to determine the preview time.

The updated Section 2.7 is as follows:

Preview time plays an important role in the development of feedforward control. It must be larger than or equal to the time delay introduced by the feedforward controller and actuator dynamics. It is preferable to be equal, but a larger value is acceptable, as additional time delay can be easily introduced into the feedforward controller, as shown in Figure 4. To *determine the optimal preview time for a given focus distance, we evaluated the cross-coorelation between the blade effective ($u_{beff,k}$) and the corrected inflow ($u_{cor,k}$) wind speeds, with $k \in$ {col, yaw, tilt}, and we chose the index of the peak value as the available preview time.*

Furthermore, we updated the Discussion section, with the following:

By evaluating the cross-coorelation between the blade effective ($u_{beff,k}$) and the corrected inflow ($u_{cor,k}$) wind speeds for a discrete set of sampled values of the focus distances in Section 3.3.3, we found that the preview time is constant for all the selected focus distances. It is closely coupled to the time needed for blade $i-1$ to reach the position of blade i, i.e. 120° azimuth angle change. For example, by considering laminar inflow with wind shear, no matter what the focus distance is, the delay time between the corrected inflow wind speeds from blade 1 and the blade effective wind speed from blade 3, will always be the same, which is the time needed for blade $i-1$ to reach the position of blade i. If the focus distance has changed, the ϕ in the MBC transformation also has to be changed, furthermore, the control signal should be delayed accordingly. Note that control development must proceed with sufficient attention so as to ensure that the feedforward controller does not result in higher time delay than the available preview time. For example, a feedforward controller with a crossover frequency of 0.1 Hz may result in higher time delay compared to that with a crossover frequency of 0.2 Hz (Dunne and Pao (2016)). With this, we want to point out that the feedforward controller crossover frequency and the focus distance are coupled. Hence, defining the former typically leads to a minimal selectable focus distance.

Q 5: *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.*

Reply: In the revised manuscript, to analyse what the optimal telescope parameters should be for a given focus distance, we introduce a new objective function in Section 2.8, where the objective function is based on the coherence (γ_k^2) between the blade effective ($u_{\mathrm{beff},k}$) and the corrected inflow ($u_{\mathrm{cor},k}$) wind speeds, with $k \in$ {col, yaw, tilt}, leading to the following objective function

$$J_{\mathrm{lp}} \;=\; \sum_k J_{\mathrm{lp},k} \;=\; \sum_k \gamma_k^2(f) \;. \tag{3}$$

By evaluating J_{lp} for a discrete set of sampled lidar and telescope parameters, the maximum of the objective function would result in the optimal telescope parameters within the discrete set of sampled lidar and telescope parameters.

Q 6: *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.*

Reply: The telescope parameters computeation are based on a simple method described in Section 3.3.1. We have used LES to validate the approach and model the nominal transfer function and uncertainty weight.

Organization

Q 7: *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:*

Reply: This is a great point. We have organised the revised paper according to your recommendation. It does indeed make the paper more fluid to read.

Q 8: *Similarly, in Section 2.3, you could also explain that MBC is also applied to the blade-effective wind speed.*

Reply: We added the following to Section 2.4 (last paragraph):

We have already mentioned that the measured inflow wind speeds were transformed to the non-rotating frame of reference by applying the MBC transformation. In order to assess the performance efficiency of the blade-mounted lidar-based inflow wind speed measurement, the blade effective wind speeds were also transferred into the non-rotating frame using the MBC transformation as follows

$$\begin{bmatrix} u_{bef,col} \\ u_{bef,yaw} \\ u_{bef,tilt} \end{bmatrix} = T_{mbc}(\theta) \begin{bmatrix} u_{bef,1} \\ u_{bef,2} \\ u_{bef,3} \end{bmatrix} \tag{4}$$

where $T_{mbc}(\theta)$ is defined in Equation (9).

Q 9: *The paragraph about the control development (page 9), the remarks, the $G_{d,f}^{-1}$ 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.*

Reply: We agreed that including $G_{d,f}^{-1}$ is not crucial for the paper and we have removed it in the revised manuscript. However, we think the the performance weight is required to show the objective in the control development.

Q 10: *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.*

Reply: We added the following sentences to the end of Section 2.2:

The second-order polynomial function (f) is fitted on the data extracted from 10-minute large-eddy simulations with laminar inflow for mean wind speeds between $4\,\mathrm{m\,s^{-1}}$ and $25\,\mathrm{m\,s^{-1}}$. The $u(F,R)$ is the wind speed at an upstream distance from the blade of F, and at a blade radial position of R, and u_0 is taken from the same blade radial position of R, but at an upstream distance of three times the rotor diameter (3D).

Q 11: *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.*

Reply: Thank you for this suggestion. We have switched the sections in the revised manuscript.

Minor issues

Q 12: *Page 6, line 12: to estimate $u_{\text{h,est},i}$ 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.*

Reply: We updated this section by added the following to Section 2.2 first paragraph:

Without loss of generality, the weighting function of $W(F,\xi)$ from Equation (1) was neglected, and two assumptions were made: (1) the $v_{h,i}$ and $w_{h,i}$ components are zero and (2) the mean wind speed is parallel with the rotor axis, i.e., no tilt and no yaw misalignments are considered.

Q 13: *Several variables are introduced relatively late, e.g. k, $V_i(\xi)$.*

Reply: In the updated manuscript, we introduced earlier the variables in Section 2.1.

Q 14: *The variables are not consistently named: you use "blade-effective wind speed" for (1) the original $u_{\text{bef},i}$ with i for blade 1, 2, and 3, as well as (2) for the transformed $u_{\text{bef},k}$, for $k \in$ {col,yaw,tilt}.*

Reply:

As with many papers, there are many variables and we feel that it is sometimes more confusing to have different variable names for similar quantities. Thus we have used different indices i and k to distinguish between the different blade-effective wind speeds here.

Q 15: *Section 2.6: It is not clear that $u_{\text{cor},k}$, is delayed. The only delay introduced in Section 2.3 is for the pitch angles.*

Reply: By applying the recommended method, this part have been removed from the update manuscript.

Q 16: *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.*

Reply: Thank you for pointing this out, as we did indeed forget to specify the simulation time. We have corrected this in the revised manuscript and added the followings to Section 3.2:

Furthermore, the 10-minute simulation results in a turbulence intensity of 8.5 % and a wind shear corresponding to a power law description with an exponent of approximately 0.12.

Q 17: *Page 12, line 12: and not necessary.*

Reply: Thank you. We have corrected this typo.

References

Dunne, F. and Pao, L. Y.: Optimal blade pitch control with realistic preview wind measurements, Wind Energy, 19, 2153–2169, doi:10.1002/we.1973, 2016.

Skogestad, S. and Postlethwaite, I.: Multivariable feedback control: Analysis and design, 2nd edition, John Wiley & Sons, 2005.