Interactive comment on "Control-oriented Linear Dynamic Wind Farm Flow and Operation Model" *by* Jonas Kazda and Nicolaos Antonio Cutululis

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Received and published: 9 June 2018

Author's response
Dear anonymous reviewer,
We would like to thank the reviewer for the comments and their time spent with the review. The extent of the comments invites to an open discussion and we appreciate that. All constructive comments are addressed and highlighted in the attached, revised paper and discussed overall in the following.

This paper addresses an important topic in wind farm control: the need for an accurate

and linear model of the turbine and flow dynamics inside a wind farm. However, there are a number of important short-comings in this work. The need for a wind farm model has not been motivated sufficiently in the introduction. Claims are made about the need for a dynamic wind farm model, while good results have been shown with steady-state wind farm models in the literature. Furthermore, there are publications in the literature that address the APC problem while neglecting the wake interactions between turbines, showing positive results.

Author's response
Thank you for your comment. The reviewer first comments that there is a need for a dynamic model of wind farm flow and turbine operation and thereafter states that this is not the case and static models would be sufficient. We are confused by the contradicting message. Nevertheless, we strongly believe that dynamic models are needed.

The literature overview given in the introduction is very sparse, and does not suffice for a journal publication. The scientific gap has not been presented convincingly.

Author's response
We would be grateful if the reviewer could be constructive and concrete with regards to the literature overview. The length of the introduction and the discussion on the scientific gap is extended in the revised paper.

A linear model is claimed to be presented. However, actually, a nonlinear model is presented which is linearized periodically, if the conditions change sufficiently. However,

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how often the model needs to be updated has not been investigated under the relevant conditions, such as changing wind directions, wind speeds, and turbine operational settings. Furthermore, the actual expressions of the model are not presented, which makes the general model formulation abstract and hard to follow.

Author's response
We thank the reviewer for the comment. However, we have to disagree. The presented model is linear and not a linearized version of a nonlinear wind farm flow and operation model. In order to provide more information on the presented, linear, dynamic model, the revised paper includes more details on the linear wake deficit model. The model update frequency is discussed with respect to changing wind speeds and turbine operational settings in section 3.1.4. The accuracy of the presented model in different wind directions is investigated in section 3.1.5. To improve the reader's understanding of the test conditions, the following description is added to section 3.1.4 in this regards. *"The study is performed in the same wind conditions and wind farm operation as presented in section 3.1.2. The robustness of the linear model is thus tested in varying wind conditions and changing turbine operational settings."*

It is mentioned several times that the computational cost of this model is significantly lower than comparable models from the literature. However, the computational cost is never quantified inside this paper. Even more so, the upper limit on the computational cost that can be afforded has not been addressed, leaving the reader completely in the open about the computational costs involved.

Author's response
The paper addresses the computational effectiveness of the presented model, which is regarded as the number of model states required to estimate wind farm flow dynamics.

The discussion is extended in the revised paper.

"It is further estimated that the Dynamic Flow Predictor requires only 0.5% of the states of a comparable, dynamic 2D CFD model. The state space system of the Dynamic Flow Predictor used for the two turbine case study has 5 states. A similar two turbine set-up simulated using a 2D CFD model in (Doekemeijer et al., 2016) uses 1034 states. Both, the Dynamic Flow Predictor and mentioned 2D CFD model use sparse system matrices. Hence, the smaller number of states used in the Dynamic Flow Predictor can result in larger computational effectiveness."

The use of an amount of states that is three orders of magnitude smaller than a comparable model is a clear indicator for lower computational costs. An explicit specification of the computational costs of the Dynamic Flow Predictor is not useful in this work, since it would be dependent on IT characteristics such as hardware and programming language.

Many decisions are taken without proper argumentation, and the underlying assumptions are often ignored. For example, the choice of the wake superposition model, the sampling time of the model, the choice of model states and outputs, and the use of a Kalman filter, which uses a periodically linearized model.

Author's response
The linear wake superposition model is chosen as it is observed to give the best agreement with the nonlinear wake superposition approach used in SimWindFarm. Other linear wake superposition approaches can be chosen when using the model in other environments.

The following description on the choice of the model sampling time is added to the paper.

"The sampling time used in the present work is 30s. The relevant, in this case, fast

turbine dynamics are generator power dynamics and blade pitch control dynamics, which are typically in the order of seconds. It is thus assumed that the dynamics of turbine power production can be modelled using algebraic variables" Using wind speed as output of the flow model is motivated by the need to predict wind speed dynamics at wind turbines in a wind farm, as stated in the introduction of the paper.

However, the choice for the (Extended) Kalman filter has not been discussed, nor have the underlying assumptions been presented. It is not clear whether the underlying assumptions of the KF are valid for the problem at hand. Furthermore, it is not discussed how the covariance matrices in the KF are chosen.

Author's response
The use of a Kalman filter is already motivated in section 2.2 of the manuscript. The employed Kalman filter is an ordinary Kalman filter and not an Extended Kalman filter, since the underlying model is linear.
The authors are convinced that the variety of results showing the benefit of the use of the developed Kalman filter, justify the use of the Kalman filter.
As regards the calculation of the covariance matrices of the Kalman filter, the following descriptions are added to the paper.
"The covariance [of the process error] is estimated using physics-based estimates of the model errors."
"The covariance [of the measurement noise] is estimated using physics-based and empirical estimates of the measurement errors. The cross-correlation between R1 and R2 is modelled to be zero."

The SimWindFarm simulation model has been used to "validate" the control-oriented

model. However, the control-oriented model has been chosen such that the wake models are identical in the two models, introducing bias into the validation process.

Author's response
We would kindly ask the reviewer to consider that the models are not the same. SimWindFarm uses models for wake deficit, wake meandering, nonlinear wake superpositioning, flow propagation and wind turbine dynamics. The only similarity with the Dynamic Flow Predictor is the wake deficit model. The adaption of the wake deficit model to represent the conditions of SimWindFarm is justifyable, since a similar model adaptation process could be used for the use of the Dynamic Flow Predictor on a real wind farm.

Furthermore, the validity of SimWindFarm is unclear, and it is uncertain why the authors did not choose to use a high-fidelity dataset from a validated LES model. Irrelevant of the results presented in this paper, it is uncertain whether the proposed algorithms work in a realistic scenario, since the fidelity of the simulation model is very limited.

Author's response
We would be grateful, if the reviewer could be more specific on the relevance of phenomena that are better estimated in an LES model. To the author's knowledge, all phenomena relevant for the validation of the Dynamic Flow Predictor are considered in SimWindFarm, given the spatial and temporal resolution of the Dynamic Flow Predictor. With regards to the validity of SimWindFarm, we would kindly ask the reviewer to consider that SimWindFarm is composed of validated models published by recognized institutions, as referenced in the methodology section of the paper.

Furthermore, the results section is very difficult to understand, and it is unclear what the measurements are that are fed in to the Kalman filter. Furthermore, several variables such as the power available have not been introduced mathematically. It is unclear how to reproduce the results presented in this work.

Author's response

The aspect of reproducibility of the results is, of course, very important in research practice. However, it is not clear why the reviewer feels that this is not possible. To address this uncertainty, we have added more details in the paper, albeit it is not possible to include all the details in an article.

A definition of the available power is added to the section 3.2.3. As regards the measurements used as input to the Kalman filter the following explanation is added to the paper.

"The measurements y_{meas} are wind speeds related to selected wind speed states of the flow model, such as the current rotor effective wind speed at wind turbines."

The paper is not very easy to read. Specifically, the results section is confusing, and several figures do not add information compared to the text. The reader should consider removing these figures, and replacing them with more informative graphics, such as flow field plots or tables with simulation settings. The graphics are often pixelated, and the captions and axes labels often have different fonts than the rest of the text. The symbol notation is confusing at parts, too. See the attached pdf for more detailed comments. Due to the number and the severity of the remarks, I suggest this paper is declined.

Author's response It is unfortunate that the reviewer finds the paper not very easy to read. We have made several changes in the manuscript in the direction indicated by the reviewer, more specifically:

- Figure 1 and 2 are combined into a new Figure 1 that includes more information on the Dynamic Flow Predictor.

- Figures 3 and 10, indicated as non-informative by the reviewer have been replaced with an improved version. It is our feeling that removing them completely might lead to confusion as to the layouts used in the simulations.

- A table with the simulation settings was added.

- More details are added to the definition of the total system matrices.

- The resolution of all figures is increased by either conversion to vector graphics or high resolution images.

Furthermore, we would like to address comments from the supplemented document of the reviewer. However, we cannot consider comments such as "Is this acceptable?"

I would be interesting in seeing the time series of the signals for SimWindFarm and for your model.

Author's response
The time series are already shown in Figure 5 of the paper.

Are you assuming a zero-order hold in between your discrete-time predictions? What is the collected average RMSE for e.g., a 1 second sampling rate?

Author's response
We do not assume a zero-order hold between the discrete-time predictions. The discrete-time estimate of wind speed at time step n is defined as the average wind speed over the time interval $[nT_s, (n+1)T_s]$, where T_s is the sampling time. We would have been grateful if the reviewer had stated the reason for suggesting to estimate wind speed at a 1 second sampling rate. In wind farm control the sampling time is typically in the order of 10s and larger.

Are you estimating and measuring the same things? What is the time horizon of the estimates?

Author's response
All statistical comparisons between the Dynamic Flow Predictor and SimWindFarm are always performed using the same sampling approach in both models. As such, for example, the comparison of wind speed is conducted as follows. The wind speed state in the Dynamic Flow Predictor is defined as the mean wind speed over the time interval $[nT_s, (n+1)T_s]$. Therefore, the mean wind speed over the same time interval is used for the comparison from SimWindFarm. The normalized root-mean square deviation in wind speed u_{RMS} is thus calculated as

$$u_{RMS} = \sqrt{\frac{1}{N}\sum_{n=0}^{N}\left(\frac{u[n] - u_{SWF}[n]}{u_{SWF}[n]}\right)^2} \tag{1}$$

where N is the number of samples of the comparison and u the wind speed esti-

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mated by the Dynamic Flow Predictor. The discrete wind speed u_{SWF} obtained from SimWindFarm is defined as

$$u_{SWF}[n] = \frac{1}{T_s} \int_{nT_s}^{(n+1)T_s} u(t)dt \qquad (2)$$

Also, why does the error grow so large for turbines 5 and 6, in a relatively short time?

Author's response
The observed jump from accurate predictions to less accuracy in predictions is due to the change from prediction based on internal model delay states to persistence-based prediction, as explained in section 3.2.2.

Also, Doekemeijer et al. 2018 do a forecasting with their 2D CFD model for power captured by the turbines for a 400 second horizon (Figure 10 in https://www.wind-energ-sci-discuss.net/wes-2018-33/). How does your work compare to theirs?

Author's response
We thank the reviewer for the comment. The error of the referenced 2D CFD model ranges between 5% to 15% in that submitted paper. The error of the Dynamic Flow Predictor ranges from 1.5% to 14%. The errors of the two models are thus comparable. While comparable in accuracy, the Dynamic Flow Predictor only uses 0.5% of the states of the 2D CFD model.

Please also note the supplement to this comment:
https://www.wind-energ-sci-discuss.net/wes-2018-29/wes-2018-29-AC1-supplement.pdf

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Interactive comment on Wind Energ. Sci. Discuss., https://doi.org/10.5194/wes-2018-29, 2018.