Wind Energ. Sci. Discuss.,
doi:10.5194/wes-2016-38-AC1, 2017

WIND ENERGY SCIENCE DISCUSSIONS

Interactive comment on "Stochastic Wake Modeling Based on POD Analysis" *by* D. Bastine et al.

D. Bastine et al.

david.bastine@uni-oldenburg.de

Received and published: 13 February 2017

We thank the referee for the detailed revision of the manuscript. We are happy that the referee appreciates our model idea and will gladly consider the referee's comments to improve the quality of the article. In the following, we will answer the comments of the referee. The original referee's comments are written in italic letters.

1 Answers to General Comments

The main weakness of the paper is its length – there is a lot of information packed into it and although the content is very interesting, it gets a little tedious after a while due

Printer-friendly version

Discussion paper

to the repetitiveness of the figures and the organization of the results. A suggestion: can you give another review of the text and figures to ensure that there is no way to compact it a bit? Some figures are there but only one sentence is given about them, are they really necessary?

We see this point of the referee and agree to shorten the paper with a stronger focus on the main results. Some of the figures, which are not crucial, will be removed. Here we will take also the advise of the second referee into account.

We tried to provide in our paper all results of the considered cases. In a revised version some of our results can can be taken out of the main part of the paper and put in an appendix, if possible and accepted.

Finally, by the end of the paper it is still not clear what a good model performance is i.e. what are the authors aiming for with this model?

This remark of the referee shows us that we have to improve our conclusions. Actually our result is that there is is not one perfect model, but depending on the chosen aspect different simplified models are proposed.

In the end, our goal is to build stochastic models, which reproduce important aspects of the load dynamics acting on a wind turbine in the wake, such as e.g. DELs. Furthermore, these models should be as simple and efficient as possible. Due to the flexible nature of our model, different models can be taken as "good" and different levels of complexity might be needed dependent on the aspect or load of interest. Our main approach is, to use computationally expensive simulations like the highly precise LES models to deduce such appropriate stochastic models. Due to the efficiency of the stochastic models, long-term studies are made possible. We will try to make this point more clear in the revised version of the manuscript, particularly in Sect. 3.5 and in the conclusions.

As an improved beginning of our conclusions we suggest:

"In this work we presented a conceptional approach to derive stochastic reduced order wake models from costly CFD calculations such as LES simulations. Such expensive computations can be done on big computer clusters only for several hours of operation of a turbine. We show that a strong reduction of complexity is possible and important aspects of the wake flow and its impact on a wind turbine can be grasped by relatively simple models. The corresponding wake flows can be generated in a very fast stochastic manner enabling, for example, long-term studies of e.g. load assessments.

In contrast to a general model reduction of a wake flow, we show that the necessary complexity of the wake models strongly depends on the quantities the models should be able to reproduce. This is not only true for the necessary number of modes, as also discussed in Bastine et al. (2015b); Saranyasoontorn (2005, 2006), but also for the complexity of the stochastic models describing the temporal dynamics through the weighting coefficients $a_j(t)$. The value of our approach has to be judged by the application as for each new aspect or even each new turbine and possibly new simplified models have to be worked out. In our opinion such a model reduction can be done by a straight forward procedure......"

2 Answers to specific comments

P5 L5: What are the criteria to define satisfactory performance? If you get to them later, just say here that you will get to them later. Same issue P7 L29.

We propose to add “as discussed in Sect. 3.5” to P5L5. In P7L29, we removed the corresponding sentence, since it basically just repeats the same statement in P5L5.

P6 L2: "temporally local": can you elaborate a bit more on what this means and how it affects your analysis? ie, at each snapshot the wake-defining vd value is different? Can you say how much this value changes over time? Since it depends on the max vd, which is a very unsteady quantity, this could criterion could also oscillate a lot depending on your TI? At this point I am confused as to why "the stochastic modeling approach, presented in the following, does not principally rely on the chosen preprocessing procedure" but I assume it will be clear as I continue reading the manuscript.

We agree that the maximum velocity deficit is a very unsteady quantity, as shown in Fig. 1. However, what we mean by "the stochastic modeling approach, presented in the following, does not principally rely on the chosen preprocessing procedure" is explained in the following.

The goal of the preprocessing is to extract the wake structure. In other words, coherent structures which are only weakly influenced by the wake should be excluded from the analysis e.g. the light blue structures on the right of Fig. 2a. This can be achieved by many different approaches and thresholds. Our "temporally local" threshold is only one possible way. It actually turns out that using a steady threshold of 0.4 times the maximum deficit value of all snapshots leads to similar extractions. These similar results are also caused by the additional dilation procedure, which makes the extraction relatively robust. In some sense, we just extract a relatively small part of the wake structure but keep the outer regions of the wake through dilation.

As explained in P6L5-9, we also used fixed confined spatial region for our analysis instead of a threshold, which also leads to similar results. In that case, the POD modes are slightly different and we sometimes need a bit more modes to achieve the same performance for the truncated POD as in the case with threshold extraction. However, our general procedure to obtain a stochastic reduced order model stays the same.

Additionally, we will subsitute the words "temporally local" by "for every time-step", which is one of our suggested changes in the corresponding paragraphs:

"Before the POD is applied to the data, the velocity field is preprocessed similarly as in *Bastine et al. (2015)* to focus the analysis on the wake structure. Coherent structures which are only weakly influenced by the wake should be excluded from the analysis e.g. the light blue structures on the right of Fig. 2a. The preprocessing is illustrated in Fig. 2. First, we subtract the mean field far upstream of the turbine (Fig. 1a) from the wake flow (Fig. 2a). The velocity deficit obtained after changing the sign of the field is shown in Fig. 2b. Second, we extract the deficit by using a relative threshold for every time step. This means that we set all values smaller than 40% of the current deficit maximum to zero. This extraction is followed by a dilation procedure *Serra (1982)* to keep the neighboring regions which are lower than the threshold. The kernel used for the dilation is a disk with radius 20 m. The resulting extracted deficit is shown in Fig.2c.

It should be noted that the stochastic modeling approach, presented in the following, does not principally rely on the chosen preprocessing procedure. Changing paramaters, such as the threshold value only lead to minor quantative differences. Even the analysis is performed confined to a fixed circular region around the wake center, instead of using a threshold, qualitatively similar results have been obtained. The threshold procedure is chosen to be consistent with our former work presented in *Bastine et al. (2015)* where it lead to better results concerning the selection of POD modes."

P12 L15: "The POD modes also reveal this non-symmetric behavior of the wake." Would this also be the case if you were looking at a downstream distance > 3.5 D?

Due to the presence of the ABL, the wake is not axisymmetric for higher distances either, as can e.g. be seen by the local TKE for a downstream distance of approx. 5 D (Fig. 3 a).

However, the wake seems to become a bit more symmetric in some sense. For example, additionally to the mode 1 describing horizontal motion we now also obtain a mode 4 representing vertical motion ((Fig. 3b and (Fig. 3c). However, both have completely different eigenvalues (226 and 82) and thus also the large-scale motion is still not axisymmetric.

L18: "As discussed in Bastine et al. (2015b), mode 1 is related to the horizontal large-scale motion of the wake." What is meant by horizontal here, its downstream advection or its cross-stream meandering? Because your cross-stream component is zero here, right? OK you briefly discuss this in P15 L7-10 but have a think about whether the controller also may be driving this.

Here, we mean the cross-stream meandering in the horizontal $y-$direction. Note, that the LES simulations including the actuator disk with rotation are run with non-zero v,w component. Thus the "meandering" of the wake can also be caused by v and w components. v and w are set to zero only for the approximated descriptions through truncated POD or stochastic models. Obviously the controller of the actuator disk with rotation used in the LES influences the dynamics of the wake and thus also its movement. How this happens exactly is unclear. However, this is a realistic process which we do not want to exclude in our simulations.

P14 L19: why are a lot fewer modes needed to reproduce the torque? could this be just a consequence of how your variable speed controller works, ie it is driven by your hub height wind speeds and does not respond to small scale fluctuations?

The first question is also discussed in P15L7-10 which we suggest to change to

"The generally simpler performance for the torque might be related to the large moment of inertia of the rotor causing the higher frequencies and the occurring spectral peaks, which are poorly captured, to play a less important role. This can be seen by the relatively low frequency peaks in Fig. 9a and the relatively smooth time series in Fig. 8a. The more relevant low-frequency dynamics can be captured by low order POD modes corresponding to relatively large coherent structures. Hence, the time scales relevant to a specific load strongly influence the number of modes necessary for a satisfying description of this load."

As we recently found, the torque signal put out by fast is not to be understood as the instantaneous torque obtained from summing up the tangential forces over all blade elements. It actually is calculated as a function of the rotational speed and give the same output as the generator torque. It is thus a reaction to the tangential forces. Consequently, the large moment of inertia of the rotor leads to relatively slow dynamics and a smooth signal. We will therefore refer to this torque as the generator torque instead of rotor torque in the revised version of the manuscript.

Obviously, the controller also influences the dynamics. It is actually driven by the rotational speed and is based on a relatively coarse lookup table. It therefore has relatively long response time. However, we are not convinced that this results in less high frequency dynamics of the loads such as the torque.

P23 L25-26: and what does this mean in terms of fatigue loading / premature failure? Would you be able to add a sentence commenting on this, if not quanti- then at least qualitatively?

In the revised version of the manuscript we will change P23L25-28 to:

"It should be noted that the stochastic wake models, presented in this section, still have the same shortcomings as the truncated POD, namely the missing energy for small-scale dynamics when including only a few modes. Consequently, the fatigue of a wind turbine component will be severely underestimated if the estimation is solely based on these low order stochastic models. Thus, in the next section we investigate an approach to include small-scale structures without the inclusion of a large number of modes."

P26 Figure 20: why is torque so off in your stochastic models, while it is the best for the truncated POD? Also why standard errors for the spectral model only are given? In one of my comments above I wondered about the variation in the uncorrelated model, can you include that?

This topic has been addressed P22L21 and discussed in P23L13-17. However, we will slightly rephrase P23L13-17 to:

"It is difficult to identify the reason for the possibly weaker performance for the torque when including more modes. One possible explanation might be that the parametrization of the PSD or the fitting procedure might be less good for higher mode numbers ($j > 15$). However, preliminary experiments with spectral surrogates of the a_j , which match the PSD exactly, indicate similar trends. Another reason could be that the assumption of independence of the a_j becomes problematic when including many modes."

The issue of the standard errors has been addressed in P22L22-25. We have slightly rephrased it to

"The errors shown are estimated as the standard deviation for an ensemble of 10 realizations of the *spectral model*, calculated for $N = 1, 4, 5, 6, 10, 30$. It should be noted that the statistical estimates for the truncated PODs also have errors. Due to the highly turbulent nature of the flow, every LES simulation would lead to slightly different values. However, these errors are much more difficult to estimate. Running an ensemble of LES, for example, is computationally too expensive and other statistical methods lead to relatively unreliable estimates. As a first guess, we can assume that these errors are of the same order of magnitude as for the *spectral model*."

P31 L16: your work didn't really reveal this but just confirmed it. It is pretty well known that small-scale fluctuations are extremely relevant for fatigue loading.

We agree and will change the "revealed" to "confirmed".

Conclusion: "models which are as complex as necessary", but what is necessary for fatigue life estimation? After your discussion this question of how much we care about these high frequencies really remains unanswered?

The answer to this question depends on the load we aim to reproduce, as also discussed in the answer to your second general comment. It turns out that a few POD modes, e.g. $6-10$ for the rotor thrust, plus the inclusion of additional homogeneous turbulent field are necessary to reproduce realistic RFCs and a DEL in the right order of magnitude ($\pm 5\%$). We will slightly rephrase the corresponding paragraph starting at P31L10 to

"Hence, our approach allows to build models which are as complex as necessary but as simple as possible with respect to a specific application. Having chosen a quantity, which should be reproduced, the major challenge is to find such an optimal reduction through the selection of modes and the choice of stochastic models for the $a_j(t)$. The LES and aeroelastic simulations used in this work have proven to be a useful tool for this purpose. For example, in order to reproduce the DEL of the original LES for the rotor thrust within a few percent, approximately 6 POD modes plus a homogeneous turbulent field were necessary. Further investigations using for example high-speed PIV measurements in laboratory wake flows would be very interesting. Due to their growing potential also lidar scanners could offer complementing measurements in the near future, e.g. following ideas presented by *Beck et al. (2015)* and *Barthelmie et al. (2016)*."

Additionally, we would like note that our main goal is to present a procedure to deduce reduced order stochastic wake models. The question what is necessary for a reliable fatigue life estimation is beyond the scope of this work since it is not even clear if DELs based on RFCs are a reliable tool for this purpose.

Conclusion: Do you have any ambition to validate these spectra or TKE values with measurements? If so, mention it here perhaps where you have the LiDAR comment.

Even though the main purpose of this work was a proof of concept concerning the presented stochastic modelling approach it would obviously still be interesting to validate the results the LES through lidar and load measurements. Unfortunately, there are currently no concrete plans to do so.

3 Technical Corrections

In the corresponding section of the referee comments, the referee makes technical comments and suggestions concerning e.g. our use of the English language. We agree with most the referee's comments and will modify the manuscript accordingly when a revised version of the manuscript is requested by the editor.

Interactive comment on Wind Energ. Sci. Discuss., doi:10.5194/wes-2016-38, 2016.

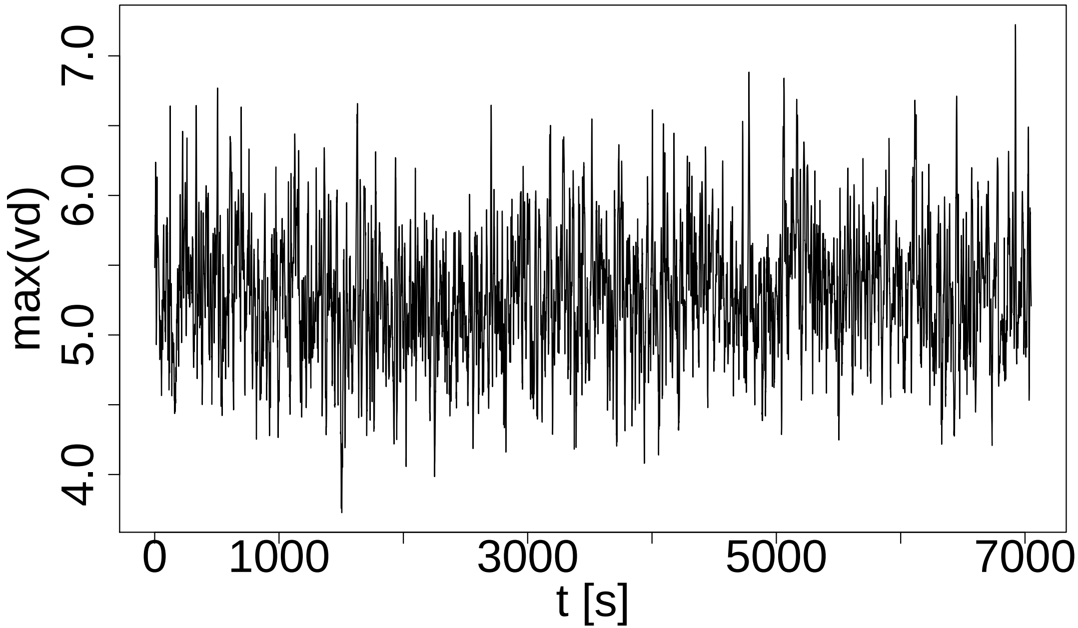

Fig. 1. Maximum velocity deficit versus time.

Interactive comment

Printer-friendly version

Discussion paper

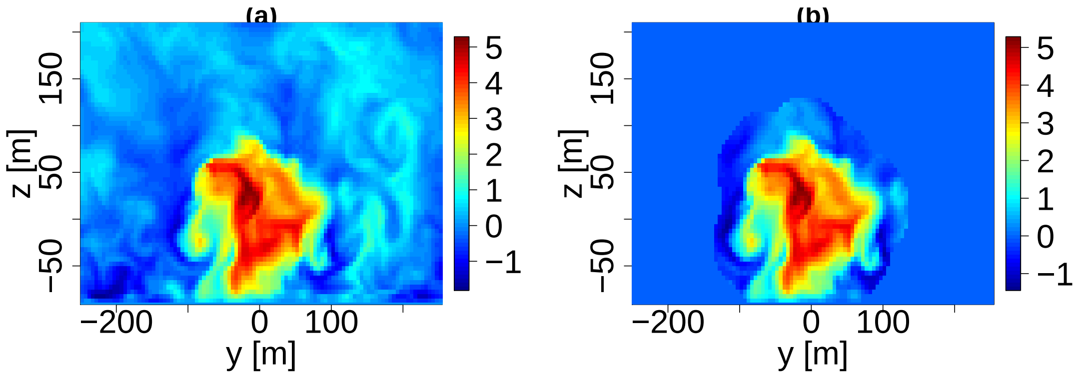

Fig. 2. Velocity deficit before (a) and after (b) extraction for t=1000 s.

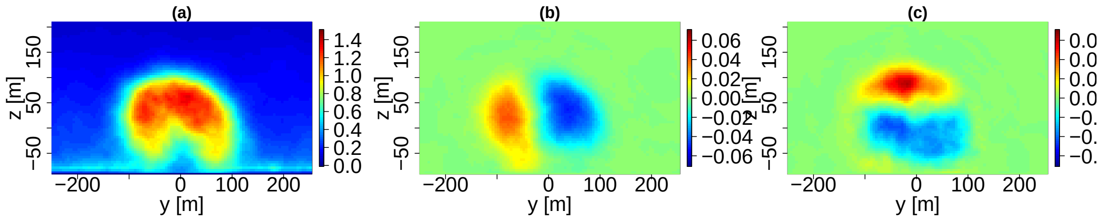

Fig. 3. Results for the yz-plane 5D away from the turbine (a) local TKE (b) POD mode 1. (c) POD mode 4.

Printer-friendly version

Discussion paper