WIND ENERGY SCIENCE DISCUSSIONS

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Interactive comment

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

D. Bastine et al.

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We thank the referee for the detailed revision of the manuscript which will surely help to improve the quality of our paper. However, we believe that some of the referee's concerns might be caused by central misunderstandings, which we hope to clarify in the following. Original comments of the referee are written in italic letters. A revised version of our manuscript will be handed in when requested by the editor.

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Discussion paper

1 Answers to major comments and related detail comments

The manuscript is very lengthy. Sometimes discussions are redundant, quite trite,or providing superfluous bridges between different sections. In contrast, key points of the work, such as description of the models,fitting procedures,are not described in detail.As reported in my detail several figures and paragraphs can be completely removed.

We see the point of the referee that the manuscript is long but believe that relevant information is contained in our detailed results and explanations. However, the manuscript might be easier to follow when shortened with a stronger focus on the main results of the article. Further aspects we presented for completeness of our argumentations, may be put in an appendix, if this possibility is given. Thus, we will revise the manuscript accordingly taking into account the referee's comments. Furthermore, more details about the models used will be added, as also discussed in the answers to the "detail comments" of the referee.

Comments concerning the convergence of the POD
1. From Figs. 5 and 6, I guess POD analysis has not achieved a statistical convergence and several inaccurate conclusions might have been drawn. See my detail comments 16 and 17.
16. Fig. 5a: Can you show the convergence of the POD eigenvalues and POD modes of interest for different numbers of snapshots and different sampling time?
17. Fig. 6. POD modes typically capture flow dynamics as couple of two POD modes with about same energy content (POD eigenvalues), spectral content, but they are orthogonal. Your first POD mode is clearly isolated and decoupled from the other modes. Therefore, it should not be associated with flow dynamics. In contrast, this might be a sign of not-achieved convergence of the POD analysis. If you try to reduce

the number of snapshots, then energy of this mode should increase. Can you please verify my speculation?

The eigenvalues and POD modes in our work have actually converged relatively well for most values and modes of interest. For example, the first eigenvalue has reached approximate convergence already after around 1000 s of data (see Fig. 1 left of this reply), when using a snapshot every $\Delta t = 0.6$ s. Moreover, the corresponding first POD mode stays almost the same after around 1000 s as well (Fig. 2 of this reply). Similar results can be found for other eigenvalues and modes (Fig. 1, Fig. 2, Fig. 3 of this reply). For high mode numbers $n > 15$, the convergence of the modes sometimes gets less good, which is an additional reason for using only a few POD modes and try to capture small-scale structures in a different way, as discussed in other comments.

As requested, we also investigated the behavior of estimated eigenvalues when varying the time between the snapshots used for estimation. It turns out that when averaging over $T = 1000$ s and adding additional snapshots through reducing Δt below 5 s, the eigenvalues do not change strongly anymore (Fig. 4 of this reply). This indicates that a lot of redundant information is contained in such "temporally near" snapshots. However, reducing Δt also leads to an increase of the used number of snapshots, which could also cause the observed convergent behavior. For a complete study, we would need to study convergence in the whole plane defined by Δt and N but this is beyond the scope of this work.

It should be noted again, that our work is to be understood as a proof of concept and we are sure that the relevant modes have converged well enough for this purpose. An even better convergence, which is not easy to achieve, might even improve our results. In the revised version of the manuscript, we will thus only shortly comment on the convergence of modes and eigenvalues. For example by adding

"Most of the POD modes and eigenvalues have converged relatively well when

averaging over $T > 2000$ s. However, particularly the convergence of the modes is less good for mode numbers $j > 15$. Since our work aims for a proof of concept and not for an exact estimation of POD modes and values a detailed convergence study is not presented here."

to Sect. 4.1. If requested , we could also present a convergence study in the appendix of the article.

Regarding Comment 17., we agree that the appearance of approximately degenerated (paired) eigenvalues with corresponding couples of modes does occur relatively often but by far not always. Degenerated eigenvalues often occur when symmetries are present in the flow. The corresponding eigenspace is then invariant under a certain symmetry transformation. The most important symmetry in our flow, namely the axial symmetry corresponding to rotations around the stream-wise axis, is broken due to the ABL and thus we do not expect degenerated eigenvalues.

2. Performance of the uncorrelated model are extremely poor. Therefore, I recommend to remove this model from the manuscript.

We agree that the results for the uncorrelated model are mostly poor except for the local turbulent kinetic energy. However, we included it in our results to present a systematic increase of complexity for the stochastic models of the weighting coefficients. In this way it can be understood how complex such models need to be, in order to capture different aspects of flow or loads on a turbine. For example, the rainflow counts of truncated POD and OU-based model are very similar. This raises the question whether an even simpler model than the OU-based model might already lead to the same results. The uncorrelated model shows that this is not the case and that at least the integral time scale of the weighting coefficients needs to be captured. These arguments will be pointed out more clearly in the revised version of the manuscript, but

at the same time we can reduce the discussion and the shown results of this simple case to a minimum.

Comments concerning the performance of the models
3. Nonetheless, From Figs. 14, 15, 17 show that predictions obtained with the spectral and OU-based model are very poor as well. Even if mean and standard deviation of the original signal are predicted with a good accuracy, these predictions are completely out of phase. This makes me thinking that applications of these models to real flows with a varying atmospheric stability or operative conditions of the wind turbines will lead to very poor predictions.
23. Fig. 14. In my opinion, these models do not provide a satisfactory prediction. Are you sure it is worth to document these results?
24. Fig. 17. "For the OU-based- and 10 spectral model, the time series resemble the loads of the truncated POD but drawing further conclusions from a single short time window is difficult", In my opinion, the model predictions are completely out of phase. Why we should learn about these models?

The referee's comments show that there have been some central misunderstandings. Our model Ansatz leads to a **stochastic** wake model. Thus, it only aims for matching the results of the original simulation or of truncated PODs in a statistical sense. A deterministic prediction of loads is not what our model aims for. Therefore, our predictions are obviously "out of phase". Moreover, we do not expect that a deterministic prediction of loads is possible at all. The highly turbulent nature of wake flows makes most of their behavior unpredictable due to their very sensitive dependence on small perturbations.

To avoid the aforementioned misunderstanding, we will further stress the stochastic character of our model in the revised version of the manuscript. For example, we will rephrase P10L13-14 to:

"To draw conclusions on the performance of the stochastic wake models, we compare their calculated loads with the loads for truncated PODs and the original LES. Due to the stochastic character of the models, these comparisons can only be made statistically. The load time series themselves as shown in Fig 14 (of the manuscript) can only give a visual impression on the dynamical behavior in principle."

Additionally, we consider to completely remove the figures showing the load time series. Even though they can give a first impression whether the statistical behavior looks similar, they do not offer any "real" statistical insight. Since both referees would like us to shorten the manuscript, this might be on possible way to do so.

Based on this discussion above, we do not agree that our models show poor results.

Obviously, we do not provide a complete model ready to be applied in the wind energy industry. So far, several works have discussed the POD and resulting modes as a tool for obtaining reduced order wake models (P2L19-P3L5). We simply see our work as a suggested procedure for modeling the temporal evolution of a POD-based decomposition through stochastic models for the weighting coefficients. We show that this is a promising approach since several aspects of corresponding truncated PODs and resulting loads, such as the behavior on large temporal scales, can be captured. To obtain variance and damage equivalent loads similar to the original simulation we additionally have to include small-scale turbulence. This is a further step also discussed in our article and in the answers below.

Comments concerning small-scale turbulence
4. In my opinion, the method proposed to include predictions of small-scale turbulence is quite rudimental and without any theoretical background. I am concerned that these models might fail for real atmospheric flows. Actually, we have already quite robust models, such as these cited in my detail comment 5, to reproduce synthetic turbulence or, if needed, CFD tools.

5. P3L16: "it is principally possible to capture the small-scale properties of the flow by adding a homogeneous turbulent field to the wake structure modeled by the POD-based approach". In my opinion this is theoretically incorrect and, thus, it lacks of generality for the model. The model can be satisfactory from a statistical standpoint because approaching smaller and smaller scales turbulence becomes more isotropic. However, turbulence theory clearly indicates that there are specific relations between correlations and energy content at different scales, which vary for different characteristics of the specific turbulent flow. A good example to produce a synthetic turbulent signal is the Mann?s model (J. Mann, The spatial structure of neutral atmospheric surface-layer turbulence, JFM, 273, 141-168, 1994), or the modified version for stably stratified flows proposed in A. Segalini et al., A spectral model for stably stratified turbulence, JFM, 781, 330-352, 2015.

Yes, we agree that our approach is quite simple and rudimental. It is an empirical approach and not explicitly derived from physical theory. However, in our opinion our results show that this approach leads to promising results showing that statistical inhomogeneities and large-scale dynamics can be described by a few POD modes, while a statistically homogeneous field can capture the small-scale structures. Interestingly, in a multiple wake in the laboratory, *Hamilton et al. (2016)* also find indications that discarding higher order modes "is equivalent to excluding energy homogeneously from the wake".

It should also be noted that such an empirical approach is not unusual. An established model like the DWM *Madsen et al. (2010)* also uses an empirical approach to include wake added turbulence. In their scenario a homogeneous "Mann field" with spatially dependent multiplication factor is added in the meandering frame of reference, which is also clearly an empirical approach.

In our work, we only aim for showing that the separate treatment of inhomogeneous large-scale behavior and small-scale turbulence, e.g. described by a homogeneous spectral model, is a promising approach. We chose to use a spectral surrogate of the

small- scale wake turbulence from our LES since this allows a direct comparison with the original LES simulation. Otherwise, we would also compare the ability of other models to reproduce LES turbulence which is not our goal here and beyond the scope of this work.

The sentence of the referee

However, turbulence theory clearly indicates that there are specific relations between correlations and energy content at different scales, which vary for different characteristics of the specific turbulent flow.

is obviously true. Our surrogate partially captures these correlations for the u-component since it keeps the spatial PSD of the LES, as described in Sec. 3.4. We do **not** argue that this is the best way to obtain small-scale wake turbulence models. Finding the best model for this purpose is a next step and beyond the scope of this work and should be based on existing approaches, as also proposed by the referee. A homogeneous Mann field could also be used, as already mentioned e.g. in P29L5. However, the Mann field in its original version is designed for neutral ABL turbulence and not for wake turbulence. Finding the specific properties of wake turbulence and capturing them with models like the "Mann approach" is also still a matter of current research.

Based on this discussion, we will try to make these arguments more clear in a revised version of the manuscript. For example, we will add

"The added homogeneous field is estimated directly from the LES data in the wake center, as described in the next paragraph. In this way the resulting wake model can be compared most conveniently to the original LES simulation In principle, other models such as *Mann (1998)* could be used to model the homogeneous field but in this case we would also investigate the ability of such approaches to reproduce LES turbulence which beyond the scope of this work."

to Sect. 6.1.

2 Further Detail Comments and Answers

1. P1L5: "...load static characteristics"; if I am not mistaken, the proposed model can only predict load fluctuations, is that right? In that case please revise your abstract.

You just misread. In P1L5 is written statistic characteristics, which should answer your comment. We changed "statistic" to "statistical" to avoid this possible confusion.

2. P1L2 and throughout the paper: "which" typically goes after a comma.

We cannot find the word "which" in P1L2. Furthermore, it is oversimplified to state that "which" typically goes after a comma. In a lot of cases a comma is not allowed before "which". For example, there must be no comma before a defining relative clause. However, we checked all our "which" sentences and found some phrases where a comma is missing. These commas will be added in the revised version of the manuscript.

3. "... differential equations can be obtained by projecting..." I guess you mean performing Galerkin projection.

Yes that is what we mean. For completeness, we will add
" "..., which is called a Galerkin projection." "
to the revised version of the manuscript.

4. P3L3: there is a typo, Kalman. 5.

We corrected this.

6. Fig. 1: the mean velocity field looks skewed in the vertical direction. Some comments are reported later in the paper. Please provide your justifications here.

We do not understand what you mean exactly. In the vertical direction we do not see any skewness which is not simply related to the mean field of the ABL. Which justifications do you mean?

7. P4L21: "Snapshots of this plane are shown in Fig. 3 revealing a variety of shapes of the wake structure". This information is trite. I suggest removing text and related figure.

We do not think that this information is trite for people not dealing with wake data from LES simulations on a regular basis. We will rephrase the corresponding sentence to

"Snapshots of this plane are shown in Fig. 3. These snapshots nicely illustrate different shapes of the wake structure, which are likely to play an important role for the loads acting on a wind turbine in the wake. As mentioned in the introduction, this is one of the major motivations for investigating a POD-based modeling approach, which can roughly describe different shapes of the wake."

8. P5:4: Revise Data in data.
Done.

9. Fig. 4: You filter out data with deficit lower than 40 maximum deficit is about 4, thus any value lower than 1.6 should be removed. How is it possible you still have negative values?

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Thanks you for this comment. There is sort of a typo in P6L3. It should read "This extraction is followed by a dilation procedure to keep the neighboring regions which are lower than the threshold". Maybe this already answers your question. Dilation is a standard method from image processing (see e.g. *Serra (1982)*). For completeness, we revised this part in the manuscript to:

"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."

It turned out that without dilation we miss some outer regions of the wake structure leading to slightly too small wake structures in the truncated PODs. However, the dilation only leads to a quantitative improvement of some of our results. Qualitatively, the results with and without dilation are very similar.

10. P7L24-30: Please rephrase this paragraph. It is quite cumbersome.

We rephrased this paragraph to:

"Since turbulent flows such as wind turbine wakes show only statistically reproducible results, our Ansatz is to describe these N weighting coefficients as a stochastic system. In this article, we additionally assume that the weighting coefficients are statistically independent yielding a description of $(a_j(t))_{j=1}^N$ by N one-dimensional stochastic processes. It should be noted, that even though the assumption of independence is inspired by Eq. (6), it obviously leads to a significant approximation since the nonlinear coupling of different scales in the fluid dynamical equations is neglected. It therefore has to be justified by a satisfactory performance of the deduced model."

11. Sect. 3.3: The stochastic methods are described too quickly and it is difficult to get the main differences among them. I suggest dividing this section is sub-sections for each model.

We tried to keep this part short, since these are very simple standard stochastic models whose properties can be found everywhere in the stochastic literature. It is a good suggestion, however, to make the main differences between the models more clear. Thus, we will revise this section according to the referee's comment.

12. P11L9: explicit to which models belong to u or $\tilde{u}$.
We are not sure what you mean but we hope to clarify this by changing the corresponding sentence to
"... described by a modal decomposition, such as the truncated PODs $u^{(N)}(y, z, t)$ from Eq. (4) or corresponding stochastic wake models $\tilde{u}^{(N)}(y, z, t)$ from Eq. (7)."

13. P11L9: Remove "This discussion will enable us to gain a deeper understanding of the results presented in the next sections 4-6." That's obvious, and as it should be indeed. Please remove this sentence.

The sentence will be removed.

14. P11L12: " flow structures in the rotor plane change in time due to the hydrodynamics of the flow field". What do you mean for hydrodynamics of the flow field?

We rephrased this to simply "dynamics of the flow field", which should make things clearer. With "hydrodynamics", we just meant the dynamics of the flow field, which follow the laws of fluid dynamics.

15. P11L19-28: I suggest to remove it. It is a quite obvious discussion.

Thank you for this comment. We will consider removing this part in the revised and shortened version of the manuscript. It might be even possible to remove the section and move its decisive parts to the discussions of the results.

18. Fig. 6: Showing the POD modes does not provide any essential information. I would save space by removing this figure. We still believe that for a POD-based model some of the POD modes used should be shown. However, we will remove three modes to save space and can also remove them all if requested by the editor.

19. P18L18: Explain more in detail this fitting procedure.

The explanation of the fitting procedure will be changed to

"For the *spectral model*, the PSDs of the a_j have to be estimated. They show a qualitatively similar behavior for all j starting with a flat region for low frequencies followed by an approximate power law behavior (Fig. 12a). This form motivates the parametrization of the PSDs given by Eq. (14). The parameters S_0, α and $f_{\frac{1}{2}}$ are estimated using least squares in a logarithmic framework. This means they are obtained by minimizing $\sum_{i=1}^{N}(\log(S_i) - \log(S(f_i; S_0, \alpha, f_{\frac{1}{2}})))^2$ with respect to $S_0, f_{\frac{1}{2}}, \alpha$, where S_i and f_i are the PSD and frequency values obtained through the statistical estimation from the LES data. While this procedure yields satisfying estimates of α and $f_{\frac{1}{2}}$, S_0 is systematically underestimated due to the nonlinear weighting by the logarithmic function. We circumvent this problem by choosing S_0 to be the value which yields the estimated variance of the $a_j(t)$: $\text{VAR}[\tilde{a}_j(t; S_0, \alpha, f_{\frac{1}{2}})] = \langle a_j(t)^2 \rangle_t$, where $\alpha, f_{\frac{1}{2}}$ are taken from the logarithmic fit. An example fit is shown in Fig. 12b. It should be noted that alternative fitting procedures are also possible and lead to similar results as long as the PSD is matched well in all the frequency ranges and not only for low or

high frequencies."

Fitting in a logarithmic framework is a very common procedure to find power law exponents or damping coefficients in exponential functions. However, the logarithmic function introduces a nonlinear weighting to the least squares fit. Therefore, large values of the S_i have less influence than for a fit without $\log$. One resulting problem is for example that when fitting to a constant function or constant region of a function, the fit will lead to an underestimation in this region, since positive deviations are weighted weaker than negative ones. That is why we estimated S_0 separately.

20. P18L19: "S_0 is systematically underestimated due to the logarithmic function". Why a fitting with a log function always underestimates?

See end of former answer.

21. P18L18-P19L4: You present 2 figures (6 panels) is 6 lines. If these plots are not crucial, then just remove them.

It is true that we did not say that much about these figures and added them mainly for the sake of completeness. We will therefore consider removing some of them in the revised version of the manuscript.

22. Since here and in the following you will show that the uncorrelated model is highly inaccurate (see Fig. 14, 15c, 17 etc.). Then, why do you present this model? In my opinion, a scientific paper should present the main information for the community in a concise way.

This question has been answered in the first section when answering the major comment (2.).

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25. P27L10-12: "We use a three-dimensional spectral surrogate of this region, as introduced in Sect. 3.4, to build a homogeneous turbulent field with similar structures. This surrogate is shown in Fig. 21c." This small-scale turbulence is already included in your POD modes. Why don't you try to recover this information from your POD results?

This is an important point. As already discussed in P26L1-9, this has multiple reasons. The most obvious one is that many modes lead to many parameters for the stochastic processes. Our idea and hope is that a homogeneous spectral model to capture the small-scale structures can be parametrized in a more simple manner. Models such as the "Mann Model" for free neutral ABL turbulence give hope that such simple models for small-scale wake turbulence can be obtained as well.

26. P27L16: "Outside the structure, we use the atmospheric boundary layer flow from the LES which is uninfluenced by the turbine" Do you add the mean flow or the instantaneous turbulent flow? In the second case, in my opinion this procedure is theoretically incorrect. You can find a large number of papers describing interaction between wakes and boundary layer flows.

We add the instantaneous turbulent flow. We agree that this procedure is a strong approximation and that close to the wake structure the ambient turbulence will not be statistically identical to the "wake-free" case. However, it is still possible and supported by our results that such a strong approximation can lead to a useful wake description, i.e. that this interaction region is not relevant for all aspects of loads acting on a turbine in the wake. Hence, we will add a sentence concerning the neglection of interactions to the revised of the manuscript.

27. Fig. 24: Is this a satisfactory prediction? This question has been answered in the former section when answering the Major comment (3.).

C16
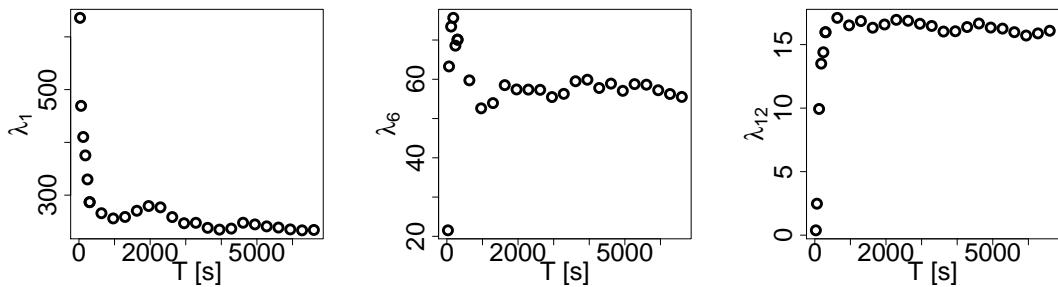
Fig. 1. Convergence of the eigenvalues: Eigenvalues estimated using temporal averaging over different times T. Here, the time between two snapshots used is 0.6 s.
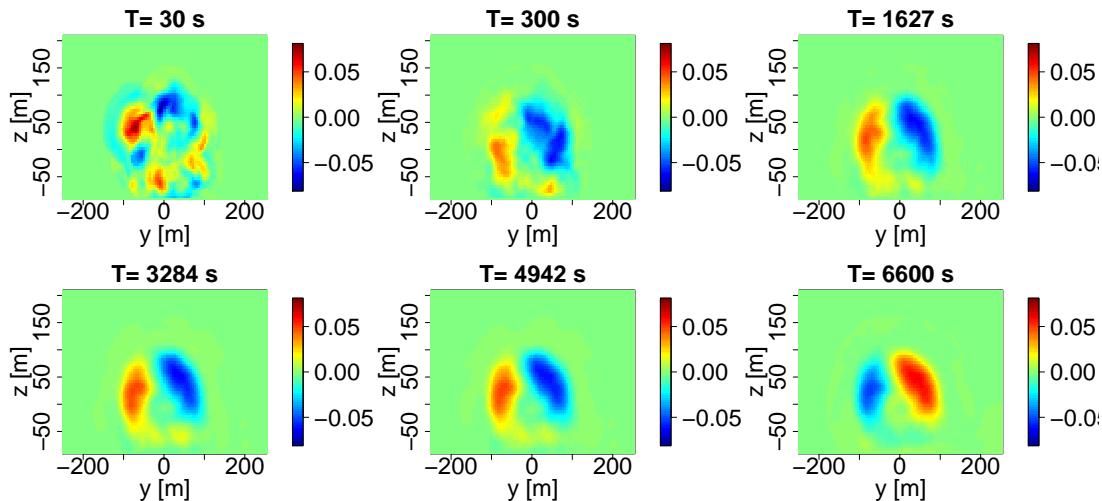
Fig. 2. Convergence of POD modes: Mode 1 estimated using temporal averaging over different times T. Here, the time between two snapshots used is 0.6 s.
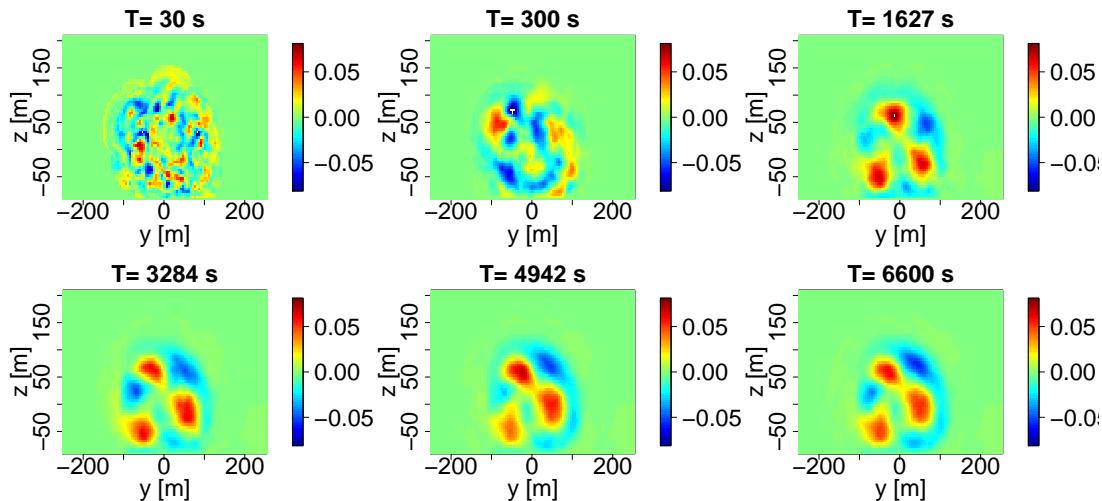
Fig. 3. Convergence of POD modes: Mode 6 estimated using temporal averaging over different times T. Here, the time between two snapshots used is 0.6 s.
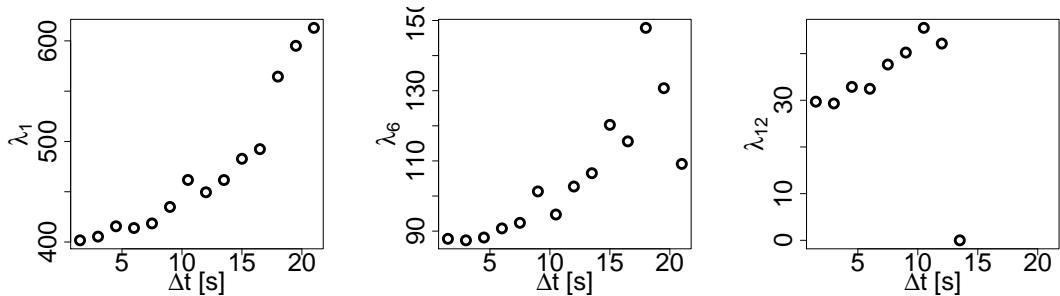
Fig. 4. Estimated eigenvalues dependent on the time difference between two snapshots used for the estimation. Snapshots from a time window of width T=1000 s are used.