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WIND ENERGY SCIENCE DISCUSSIONS

# *Interactive comment on* "Wind inflow observation from load harmonics" *by* Marta Bertelè et al.

**Marta Bertelè et al.**

carlo.bottasso@tum.de

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**Reply to Reviewer 2**

We thank Reviewer 2 for the detailed analysis and constructive inputs. A list of point-by-point replies to the Reviewer's comments is detailed in the following.

**Reviewer** *P 2 L 10: "The problem of mapping the information from a met-mast to the rotor disk of a wind turbine is in general very difficult to solve, and it will clearly be always prone to possibly severe inaccuracies." What make the inverse, sensing the wind from the rotor response, easier ? One reason evocate by the authors: rotor response will be non-local and rotor-effective! but the global rotor response*



*is reconstructed from local sensors at the blade which may encounter local phenomena such as the flow locally separated . . .*

**Authors** We agree, but the sensors of all blades are used simultaneously, which has the clear effect of averaging the information over the rotor disk. In addition, load harmonics are used, which has the additional effect of implying an azimuthal averaging. Hence local effects are filtered out, and only rotor-effective quantities are obtained.

**Reviewer** *P3 L13 "motivated by the very promising results in the field . . . " Certainly a confusion with the work of the same authors in 2015?*

**Authors** We are unsure about the meaning of this comment. With that sentence we refer to the approach developed in Bottasso and Riboldi (2014), which was partially validated with field tests in Bottasso and Riboldi (2015). Specifically, the results obtained in the field validation showed that the model formulation was promising, although still incomplete as already clearly explained in the introduction: "Notwithstanding these very promising results, the same study also showed a marked sensitivity of the results on the wind upflow angle, indicating the probable need for a richer description of the wind field." Therefore, instead of observing only vertical shear and yaw misalignment as in Bottasso and Riboldi (2014) and (2015), in the present paper we include two new states in the formulation, in order to fully parameterize the wind inflow and improve the observer performance.

**Reviewer** *P3 L22: authors uses the simplified notation 1-Rev and 2-Rev that is not introduced, please be more explicit. Such as "first rotor revolution". . .*

**Authors** The meaning of the notation $n \times$ Rev was added in the introduction.

**Reviewer** *P7 L22: Can you please give the relation between y and m ?*

**Authors** This has now been added to the text.

**Reviewer** *P8 L30-33: "State-of-the-art aeroservoelastic codes used for the design and certification of wind turbines ... with experimental data". Can you please give some references of this type of comparison. In particular it is interesting to know the limitations.*

**Authors** We are not aware of one single good review on the problem of validation of aeroelastic codes, also probably because of a lot of these activities are done internally by wind turbine manufacturers. In any case, this seems to be beyond the scope of the present paper. To accommodate the request of the reviewer we have eliminated part of the sentence.

**Reviewer** *P9 L25: "Separating the effects of gravity ... affecting load measurements."? Figure 2 do not exhibit a significant sensitivity to rho in contradiction to what is said here. Please explain.*

**Authors** We do not agree. Figure 2 shows that at 3 m/s a typical derivative decreases of about 20% when moving from $\rho = 1.1$ to 1.35 $\mathrm{kg/m^3}$, while at 11 m/s the same quantity increases by 30%. This is only to be expected: aerodynamic loads do depend on density, which does indeed depend on ambient conditions.

**Reviewer** *P11 L12: region II and III are not explicitly written in figure 2, where are they ? I guess they are delimited by red lines ?*

**Authors** That is correct, and we have now noted this in the caption of the figure. Details about the operating regions of this machine can be found in Section 2.3.4.

**Reviewer** *P12 L14: "A rather wide transition ... " Does this description correspond to figure 2 ?*

**Authors** Yes. In Fig. 2 the transition region is delimited by red lines, as now explained in the caption.

**Reviewer** *P12 L27: section 2.3.5 describes an example of the linear/non linear models (Eq. 11A and 22) identification using a dataset from the aeroelastic simulation*



*model. However, it is not indicated where loads m are taken to identify the model ? Unless all degree of freedoms of the code are used ? If so, it would be interesting to know the minimal set of data required and where they should be placed for an accurate identification.*

**Authors** Loads are taken from measurements, for example through strain gages. The beginning of Sect. 2.1 states "... the present observer is based on the lowest load harmonics. Although other response indicators could be used in principle, as for example accelerations, loads are considered in this work because they are now often measured on board modern large wind turbines for enabling load-feedback control, and load sensors will probably be standard equipment available on most future machines." In addition, the Introduction explains at length the concept.

We do not understand the comment "Unless all degree of freedoms of the code are used". Clearly the number of degrees of freedom of the model will in general affect its fidelity to reality. However this has nothing to do with the measurement of blade loads in the context of the present paper.

**Reviewer** *P15 L11: "The reason for the very poor results of the turbulent case ... this information cannot be separated from the pollution brought by smaller scale wind field fluctuation." Does it mean that turbulent small scales do not contribute significantly to the load 2-Rev harmonic in the aeroelastic model? Or maybe the turbulent fluctuation can't be described by the wind state equations given by the authors?*

**Authors** None of the two. It means that both wind states and turbulent fluctuations induce 2-Rev harmonics of comparable magnitude. Hence, the two effects cannot be separated. We believe this is clearly explained in our text: "It should be remarked that, as previously illustrated in Fig. 4, the model is perfectly capable of capturing with good accuracy these higher harmonics in steady wind conditions. The reason for the very poor results of the turbulent case is due to the fact that small scale turbulent fluctuations in the wind field cause $2\times$Rev harmonics that are comparable to, if not larger than, the ones caused by the wind states used for the parameterization. Therefore, although $2\times$Rev harmonics carry information on the wind states, this information cannot be separated from the pollution brought by the smaller-scale wind field fluctuations. In this sense, $2\times$Rev harmonics are not good candidates for the observation of wind states."

**Reviewer** *P20: It is not clear if results of the observability analysis are presented from the linear or non-linear model formulation. It seems that results 37 are only expressed from the linear formulation. Is it correct?*

**Authors** The last sentence of Section 3.1 states "Similar results, not shown here for the sake of brevity, were obtained with the nonlinear model.".

**Reviewer** *P20 L9-10: "In the third case, the noise covariance . . . and the ones measured on the simulation model, msim, ie". It is not clear here what wind state inputs are taken to get mobs and msim.*

**Authors** This part was rephrased to improve clarity.

**Reviewer** *P20 L24: "On the other hand, . . . do not indicate a predominant effect on some load components" From results indicated in the paper, I see a significant dominance of Inplane loads (up to 3 times higher than out-of-plane loads). This is not "a bit higher" as indicated later.*

**Authors** We agree, and removed "bit".

**Reviewer** *P20 L27: "A side observation, . . . in the response of the machine"*

1. *I see a line inversion (cosinus components of loads becomes sinus components of loads from one wind state couple to the other wind state couple) but no obvious type of symmetry. Please be more explicit.*
2. *Also, this 90-symmetry in the wind states indicate that one couple can be determined by the other couple, so that the problem may be reduced? If so,*



*this is in contradiction with the introduction p3 L15 "First extensive numerical experiments have shown that the load-wind model on which the estimator is based must consider at least four wind states instead of two"*

3. *What about the non-linear model? I guess we expect less symmetry and then a higher problem to solve, but what about the distribution of the observability on load components for the different wind states?*

**Authors** The text was updated to make this point clearer. In particular:

1. With the term symmetry here we are referring to the fact that the rotor response to a vertical shear is the same as for a horizontal shear, but shifted by 90 deg. The same holds for the angles. That is why we obtained $U_{11} = U_{22}$ and $U_{21} = -U_{12}$. The text was improved to clarify this point.
2. It is true that, given the symmetry in the response, the identification problem can be simplified. However, this is not in contradiction to what is written at page 3 line 15. In fact, the model should consider the entire set of wind states, made by four parameters, to coherently describe the wind field. This can be seen even by the SVD analysis: each wind state is affecting at least one of the 1-Rev components. What could be simplified/improved is only the identification step. This point has now been briefly added to the text. Furthermore, we have cited an additional reference that further explains the use of symmetry in the response of the rotor (Cacciola, Bertelé, Schreiber and Bottasso, Journal of Physics: Conference Series 753(3):032036)).
3. How to exploit the isotropic behavior of the rotor in the case of a nonlinear model is a point that deserves further investigation. However, as explained above, the symmetry in the response can only be used to simplify the identification, but will not drastically change the results. Therefore, the fact that we did not use symmetry here for the linear and nonlinear models does not affect results and conclusions of the present work.

**Reviewer** *P22 L2: "Given the behavior of the linear and nonlinear observers" From what I read of the paper, only results from the linear observers are given, I'm right? Please be clearer in the observer type you are using in section 3.1.*

**Authors** As previously explained, both linear and nonlinear models were considered in the a priori observability analysis, and this is stated in the text.

**Reviewer** *P24 figure 11: It is impossible to see the legend and plots are too small. Please make it larger.*

**Authors** Figure and legend were made bigger.

**Reviewer** *P26 figure 12 and 13: "left" and "right" should be replaced by "up" and "down".*

**Authors** The captions were modified as suggested.

**Reviewer** *P25: For this part (section 4.2) are you using all degree of freedoms (2500 blade loads) ? It is not clear in the paper as you are talking about "wind conditions at the location occupied by each single blade". Please make it clearer.*

**Authors** As stated in the paper, linear and nonlinear observers use as input the $1\times$Rev cosine and sine components of the in- and out-of-plane bending moments at blade root. Therefore we do not understand the reference to the 2500 dofs of the model. As the number of dofs in the model is totally irrelevant, we have eliminated its mention from the text.

**Reviewer** *"Finally, it was found that the error means are not significantly influenced by TI" This is not shown in figure 15.*

**Authors** We changed the sentence to make it clearer.

**Reviewer** *P28 section 4.2.1: I guess you don't have any load measurements, so you use the model identified previously from the aeroelastic simulations? This means that you evaluate the observability from the off-shore platform dataset assuming*



*the identified model is identical as the wind turbine is identical, i'm right? Please make it clearer.*

**Authors** Here we are assessing the precision of our method if it were employed on a machine like the one used in this work but located at the off-shore platform FINO 1. Wind measurements of a period of about 4 years were considered to observe the probability of each TI to occur at each wind speed. With such information, along with the Weibull distribution, we can know which TI characterizes the site and therefore calculate which error deviation to expect. A sentence was added before the explanation of Fig. (16), as suggested by the Reviewer, to clarify this point.

**Reviewer** *P30 L1: Please report figure 5.21 of Emeis(2013) in the article, it is just annoying to have to find (buy) a book to follow what the authors wants to say.*

**Authors** Figure 5.21 of Emeis(2013) shows nothing but the variation of TI as a function of hub height, which is more or less linear at the height of interest. Since it is a really simple plot, instead of inserting another figure (which would also necessitate obtaining the necessary authorization because of copyright), we preferred to add a better description.

**Reviewer** *P31 figure 16: from the article of Türk and Eimeis (2010), TI is reported to be measured at 90m not 80m.*

**Authors** Correct, and this is why the data was scaled down to 80 m with a factor equal to 1.028, as explained in the text.

**Reviewer** *P34 figure 19: change left/right to up/down*

**Authors** This was corrected as suggested.

**Reviewer** *P36 L3: "the expected average error in angles is below 1 deg" This is the expected mean estimation error based on observability formulations, you are not able to compare with real measurements, I'm wrong ? This also suppose you measure loads from 2500 sensors on the blades, I'm wrong ?*

**Authors** This result is not based on real measurements but on the simulated behavior of the machine, which however is quite realistic and performed with state of the art tools, and on the computation of the "life-time performance" according to the procedure explained in Section 4.2.1. The sentence was slightly changed to make it clearer.

Again, these results are based on a model whose input are the 1xRev harmonics of in and out-of-plane blade root bending moment. The number of dofs in the model is irrelevant, as previously noted, and was eliminated to avoid confusion.

We have taken the opportunity to make several small editorial changes to the text, in order to improve readability. A revised version of the manuscript is attached to the present reply, with the main changes highlighted in red.

Best regards.
The authors