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DISCUSSIONS

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

Anonymous Referee #2

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A linear and non-linear formulations of a black box model, which relates wind turbine loads and wind state inflows (shears and misalignments), were formulated. Coefficients of this model were identified using a dataset from an aeroelastic model. First results of the identification were validated using different wind state inflows (steady, turbulent). It was found that 2-Rev harmonics of loads are not well reproduced by the model (even when using the non-linear one), while the 1-Rev harmonics is very well reproduced. The system was then inverted to infer wind states from wind turbine loads. From this formulation, an observability analysis was performed which main conclusion is that shears contributes in majority to out-of-plane bending loads while misalignments are more related to in-plane bending loads. Also shears are much more observable than misalignments angles. This is attributed latter to a greater sensitivity to changes in shear on the inflow angle (and thus lift or loads) than to changes in wind state angles. The inverted system was finally tested on a dataset from an aeroelastic model

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Discussion paper

using non-turbulent or turbulent (Kaimal distribution with different turbulent intensities from TURBSIM software) wind state inflows. Note that authors uses an instantaneous parametrization of the turbulence using wind state equations described in the paper. Results show that the shears are better inferred than misalignments as previously expected. Also, an increase of the turbulent intensity increases the standard deviation of the estimation error, mainly for wind state angles. From an observability analysis of the model using a much longer inflow dataset (4 years), the estimation of the wind state angles is expected to be improved. Also, simulations were performed with a progressive yaw misalignment to evaluate the model to infer the wind state angle changes. It is found that, with high turbulence intensity, applying a moving averaged filter on the inferred wind state angles delay the response but predict well the level of changes.

This is an interesting subject and a consequent work on ways of using the wind turbine observers (here load sensors) to predict wind inflow states. It follows articles from the same authors: [1] C.L.Bottasso and C.E.D.Riboldi (2014) “Estimation of wind misalignment and vertical shear from blade loads”, Renewable Energy, 62:293-302, https://doi.org/10.1016/j.renene.2013.07.021 [2] C.L.Bottasso and C.E.D.Riboldi (2015) “Validation of a wind misalignment observer using field test data”, Renewable Energy, 74:298-306, https://doi.org/10.1016/j.renene.2014.07.048

Compared to reference [1] the present study uses a black box model and 4 degrees of wind state inflows (2 shears and 2 misalignment angles). Also a non-linear formulation of the model is tested. It has to be emphasis that this study is based on simulations from an engineering software (with all associated simplifications) and uses 2500 degrees of freedom as potential load sensors. Maybe an interesting future work would be to evaluate the minimal load sensors required for an acceptable infer of the wind inflow states.

I think the additional work performed in the present study compared to previous work (reference 1) is significant and interesting, so that the article worth being published in the Wind Energy Science journal with however some corrections listed below (ordered as appearing in the document):

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 ...

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

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” . . .

P15 L18 “V=(u,v,z)T” Replace z by w.

p7 L22: Can you please give the relation between y and m ?

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.

P9L25: “ 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.

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

P12 L14: “A rather wide transition ...” Does this description correspond to figure 2 ?

p12L27: 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.

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 ?

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 ?

P20L9-10: "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.

P20L24: "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 In-plane loads (up to 3 times higher than out-of-plane loads). This is not "a bit higher" as indicated later.

P20L27: "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 p3L15 "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 ?

p22L2: “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.

p24 figure 11: It is impossible to see the legend and plots are too small. Please make it larger.

P26 figure 12 and 13: “left” and “right” should be replaced by “up” and “down”.

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.

P28L31: “Finally, it was found that the error means are not significantly influenced by TI” This is not shown in figure 15.

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.

p30L1: 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.

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

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

p36L3: “the expected average error in angles is below 1deg” 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 ?

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