Validation of a coupled atmospheric-aeroelastic model system for wind turbine power and load calculations

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DOI: 10.5194/wes-2020-114

Authors response to referee comments

Dear referees, we appreciate the time and effort you spend to give constructive comments. Following your comments and suggestions lead to a considerable improvement of our manuscript. Below we will refer to all of your comments in detail. The referees comments (RC) are listed with the authors comments (AC). If changes have been made in the manuscript, these can be found in the boxes below the authors comment.

Authors response to comments from referee #2

Major comments

RC1 *L67: It is mentioned that the implemented methodology can be seen as an enhancement with respect to Storey (2015). However, this is not further detailed in the manuscript. Is there any novelty, rather than the implementation on a different set of solvers, on the computational methods used?. Whether is the case or not, it should be clearly stated in the text.*

AC In the paper (Storey et al., 2015) an Actuator Sector Method is presented. The paper describes the approach of using a sector as a compromise between an Actuator Line and an Actuator Disk method, just as our approach, but the realisation on how the forces are projected as a sector shape into the flow are different to the PALM-FAST coupling and Storey did not present an analysis of loads obtained by applying his coupling approach. Therefore, it is important to mention the paper of Storey, but we do not consider our method an enhancement with respect to Storey. The relevant phrase in line 67 states that: "A similar method is suggested in (Storey et al., 2015), where an ASM is tested in simulations." To clarify the differences and similarities we have made some additions to this phrase.

> p.3, ll. 67f: A similar method is suggested in (Storey et al., 2015), where an ASM is tested in simulations. In order to combine the respective advantages of an ALM and an ADM (Storey et al., 2015) presents a sector method, that uses a different approach of projecting the forces into the flow than is presented in this paper.

RC2 *L148: "this can be resolved in the postprocessing of the results by shifting the results in time" -> would this imply a certain level of approximation? In other words, is it a "model" that could be applied in the post-processing step? In that case it would more an "estimation" rather than a "solution" itself.*

AC In order to be able to compare different turbine models, as in figure 3, the time shift of the ASM needs to be addressed. To resolve this we shifted the results in time and mentioned this as a possible step in the postprocessing of the data. However, at this position (ll. 148f) in the paper this might be confusing. Therefore, we have moved and modified the information to the section where the shifting of the results in figure 3 is mentioned.

> p.8, ll. 217f: Therefore, when comparing the turbine output the result of the ASM simulation is shifted by 34 s for a better comparison to the other results. This does not affect the statistics but is a simple method to make the

time series obtained from the different models comparable to each other. A model or tool that automatically fixes this time shift is not included in the current version of the coupling.

RC3 *L172: "For FAST on its own, the inflow wind option "steady wind conditions" is used.": this should be re-phrased in conceptual terms, rather than as the naming used for the FAST software. Does it simply mean laminar inflow? The reviewer believes that this should have been consistent with the setup of the other methods, for the laminar comparison. So that it is something related to the load case itself, rather than to FAST alone.*

AC To mention the "steady wind conditions" option seemed relevant, as multiple options are possible for the inflow in FAST. However, it might not have been clear enough, that by choosing this, the simulation is comparable to the others as the inflow is laminar as well.

p.6, ll. 171f: As fourth method, just in the laminar case, FAST on its own is used (denoted as FAST). For FAST on its own, the inflow wind option is set to match the PALM simulations, i.e. the power law variables are set to a wind speed of 8 m/s constant with time and with height.

RC4 *L173: "To evaluate the different methods, at first, a laminar case with the same wind speed over height is considered.": could be interesting to give more details about the specific profile that was used, e.g. relating it to a shear factor.*

AC The considered laminar case had a constant wind speed with height, i.e. zero vertical gradient of the streamwise velocity. As this might have been not made clear enough, this was added in the text.

p.6, ll. 174f: To evaluate the different methods, at first, a laminar case with a constant wind speed with height, i.e. zero vertical gradient of the streamwise velocity, is considered.

RC5 *L177: " However, no differences in the results ": This could be quantified, in order to be more precise. Maybe the authors can show the relative difference of a targeted quantity, just as an example.*

AC Different sized model areas were tested and the wind speed and turbine response were compared, no significant differences were seen here and therefore the smaller one was chosen for further simulations.

p.7, ll. 178f: However, no significant differences in the conditions of the flow in the turbulent case (i.e. a deviation of 2% in the wind speed at 92 m) or the turbine output, were detected and therefore the smaller model domain was used for the simulations.

RC6 *L188: "The result calculated by FAST coincides with the value, as published by NREL (Jonkman et al., 2009b), based on the same FAST model.": The reviewer wonders why is this comment important. There is no quantification of the relative differences, and from an academic perspective is probably not that relevant. It is assumed that a simulation of the same inflow conditions and with the same code, will lead to the same results. If the comment had to do with software versioning, then it should be clear in the text. If it was more related to potential discrepancies while building the aeroelastic model, the possible origin of those should be mentioned.*

AC As FAST has numerous possibilities to be set-up, this information was considered as important. However, as the comparability to NREL literature values has no impact on the comparison to the PALM-FAST coupling we have now decided to omit this statement as it seems to be more confusing than helpful.

RC7 *L207: "A turbulent case is calculated as well.": Could be interesting to lift this comment up in the text, while introducing the load cases. Note that several comments on the FAST inflow have been already made at this point, and the reader could lack of context.*

AC Thank you for pointing this out. In order to make the workflow more comprehensible, we have added the information earlier in the text and adapted it where necessary.

> p.6, ll. 163f: As this is a generic turbine, no comparison with measured data is possible. But the availability of the turbine data allows an evaluation of our enhanced coupling method, also in terms of turbulent flows. Additionally, the availability of the turbine data offers the opportunity to compare different methods and their computational resources. Therefore, two cases were considered, firstly a laminar and secondly a turbulent flow. [...]
> p.6, ll. 171f: As fourth method, just in the laminar case, FAST on its own is used (denoted as FAST). [...]
> p.8, ll. 208f: As a second case a turbulent flow is calculated.

RC8 *L219: "Also, roughly the same peaks and therefore structures of the flow are present in the ASM results. This implies, that the coupling works in a turbulent environment as well.": The reviewer thinks that there is not enough evidence for such a statement. The phrase "the coupling works" is very vague, as it relies on a qualitative observation. There is room, for instance, for an implementation bug that could eventually shift the loading spectra during the coupling, or that accounts for a bad projection. To comment on the flow structures a qualitative measure should be used.*

AC Thank you for pointing this out. As mentioned in our previous response this was a first test of the coupling in order to see whether this might lead to a successful model. For the NREL turbine no deeper analysis was intended due to the lack of measurement data. However, you are correct, that the conclusion we drew from this short test case went too far. Therefore, we adjusted the statement to only relate to the power output.

> p.8, ll. 222f: Also, roughly the same peaks and therefore structures of the flow are present in the ASM results. This indicates that the coupling also works in a turbulent environment insofar as the turbulent structures are reflected in the power output.

RC9 *L234: " The reference power curve is obtained from stand-alone FAST runs, with a laminar inflow. The FAST turbine model is provided by eno. The calculated reference power curve coincides well with the published power curve of eno (eno energy, 2019).": the reviewer wonders what is the added value of this statement. There is no plot shown, and no discussion on how the power curves were computed from the manufacturer side.*

AC Reproducing the power curve of the manufacturer shows, that the coupling and in particular the turbine model, match the manufacturer's data. This might seem of little importance, but the manufacturer's controller was not readable (only available in binary form) to us and a wide range of input options are possible in FAST, hence it was a valuable check. A plot of the matching power curve was not seen as more informative than the statement itself.
Neither information on how the manufacturer calculated the power curve nor the data, apart from the published curve, was available to us.

> p.9, ll. 238f: The reference power curve is obtained from stand-alone FAST runs, with a laminar inflow. The FAST turbine model is provided by eno, but the source code of the turbine controller was not available to us, only an executable file was provided. The calculated reference power curve coincides well with the published power curve of eno (eno energy, 2019), without figure. Of the published power curve no further information on the computation or data is available and therefore no comparative plot is possible.

RC10 *L360: "The results of the simulations correspond well with the measurement data": In the opinion of the reviewer, there is not enough evidence to support this statement.*

AC In order to compare the atmospheric conditions between simulations and measurement data, the wind speed, the shear and the turbulence intensity were used. The simulations provide situations that could have been measured at that site, see figure 13. For the comparison of the corresponding loads the measurement data were filtered in order to find the best matching situations (c.f. table 5 and 6). To clarify we have adjusted the statement.

> p.18, ll. 366f: As shown in figure 13, the results of the simulations show realistic data, even though they are not centrally located within the measurement points, therefore, other turbine parameters available are compared.

RC 11/12 *L368: "This can be seen in figures 19 and 20. ": These figures are not properly introduced, and their numbering does not match the text sequence.*

L368: "Apparently, the rotor speed curve at the start of the peak shaver region is slightly different (c.f. figure 20). Therefore, it is only possible to compare loads at either the same rotor speed or the same wind speed.": There is no explanation given for this particular point. While the controller was not provided by the manufacturer, it was previously stated that the power curves matched perfectly.

AC Since the above comments refer to the same line, we have combined the answers here. You are right, that the figures should have been introduced more properly. The plots are mentioned at this point in the text, as it is important to know the criteria for the selection of the data intervals used in the load comparison. This selection was limited by i.a. the discrepancy in the rotor speed between the simulation and measurement data.

The differences in the rotor speed curve are shortly discussed in line 438. However, this might not be enough to address this issue. There are differences between the results of the simulations and the measurement data, even though the power curve (figure 11) matches very good. Apart from the differences that can be seen in figures 19 and 20, there are also differences in the standard deviation of the power (figure 12), the blade root bending moments (figure 14) and the load spectra (figures 15 to 17). Therefore, we do not claim that the results match perfectly. But the results show in many respects a very good agreement.

The differences that can be seen presumably stem on the one hand from the slightly different flow conditions (lower TI) and on the other hand from the modelling of the controller. The provided controller might not be a perfect match to the real turbine controller. But as we do not have access to the source code of it, we can only assume, that its aim was to reproduce the power from the wind speed and that the rotor speed behaviour does not match the real behaviour entirely.

> p.19, ll. 375f: This can be seen in figures 19 and 20 showing the measurements in Brusow. While figure 19 presents the relationship between the wind turbine power output and the rotor speed, figure 20 shows the relationship between rotor speed and wind speed. The combination of the respective values obtained from the simulations is provided by marks in these figures. Evidently, for the power output the values obtained for the simulation are within the standard deviation of the measurements that are indicated by bars. In that sense our setup seems to be successful. We point out that we did not set up our simulations in such a way that they would lead to the reproduction of the mean behaviour of the wind turbine for the specific bins of measured data. We simulated just a few selected cases within the neutral and stable range of atmospheric stability. Thus, a deviation of the turbine response from the mean behaviour in the measurements can be expected. Note that the cases simulated by us are cases with a comparatively low turbulence intensity. We do not know the details of the controller of the wind turbine, so a verification of any hypothesis why our cases show a smaller rotor speed in comparison with the mean rotor speed for the next bin of measured data is hard to verify.

RC13 *Figures A.1, A.2: Could be more useful to switch the x-axis by another variable, such as radius or projected length. The nodes might not be equidistant for every aeroelastic code.*

AC Thank you for this suggestion. We have adjusted the figures accordingly.

Minor comments

RC14 *L33 "with comparatively simple models, like e.g. TurbSim" -> TurbSim seems to be a software, rather than a model. Consider using another word here*

AC You are right, we have adjusted the relevant phrases.

> p.2, ll. 31f: The models currently used to calculate loads on entire wind turbines, like e.g. FAST (Jonkman and Buhl Jr., 2005) or Bladed (DNV GL, 2020), require wind fields as input, which are generally computed with comparatively simple tools, like e.g. TurbSim (Jonkman, 2009a). TurbSim and comparable software commonly use the Mann-Model (Mann, 1998) or the Kaimal-Model, c.f. (Kaimal et al., 1972), (IEC, 2005), to model turbulence.

RC15 *L48 "Here, the use of an ALM, moving meshes and fluid–structure interaction (FSI) lead to very detailed results but also requires a further reduction of the computing time" -> the reviewer found this phrasing a bit cumbersome. Could it be simpler to state that the consideration of FSI implies an increase of the required computing time?.*

AC Thank you for pointing this out. We have implemented your suggestion.

> p.2, ll. 48f: Here, the use of an ALM, moving meshes and fluid–structure interaction (FSI) lead to very detailed results but also implies high computational demands.

RC16 *L56 "losing" -> should it be "loosing" instead?*

AC The word "loose" is used when something is not tight, e.g. loose clothes, but "lose" is used when something is missing. As the intention of the phrase is "missing information", we think that the correct spelling is "losing".

RC17 *L135 " that occur at the blades" -> experienced by the blades?*

AC We have adjusted the sentence.

> p.5, ll. 137f: In general, the forces acting on the blades are calculated based on the wind speed that is present at the blade position, i.e. the positions in the rotor plane.