

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

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Authors response to referee comments

Dear referees, we appreciate the time and effort you spend to give the constructive comments which help to improve our paper. Below we will discuss all comments in detail.

Stating the referees comment (RC) then answering it in the authors comment (AC) and if changes were made in the manuscript, these can be found in the boxes below the authors comment.

Authors response to comments from referee #1

General comments

RC1 *Extend the literature review, especially for FAST and LES coupling.*

Improve vague language by providing a quantitative assessment (e.g. “enormous reduction of the computing time”).

Shorten parts of the manuscript (e.g., combine figures and possibly remove some).

Improve the comparison between the turbine models (e.g. add plots for quantities along the blades)

AC Thank you for these general remarks, we will address them individually below in the specific comments.

Specific comments

RC2 *Abstract: “The comparisons of the simulations to the NREL literature values show very promising results.” It is hard to tell what this statement means. I would recommend removing it or talking about something more specific.*

AC With this statement we wanted to indicate that in our first approach in a comparison with the NREL turbine promising results were made. However, no details were given here in order to keep the abstract simple and short. We agree that this way there may not be enough information given in the abstract, so the sentence was adjusted to clarify the statement.

p. 1, ll. 10f: The basic test of the coupling with the NREL 5 MW turbine shows that the power curve obtained is very close to the one when using FAST alone.

RC3 *Abstract: “an enormous reduction of the computing time”: This statement should be backed up by a number. For example, “our coupling offers a 10X speedup in comparison to similar methods.”*

AC We only have numbers for certain cases, but we agree that a specification improves the abstract here. Therefore, to specify the statement we added a number without going into too much detail.

p. 1, ll. 12f: Additionally, our coupling offers an enormous reduction of the computing time, in one of our cases by 89%, and at the same time an extensive output of turbine data.

RC4 *Section 2(?): Section 2(?) should be shortened, and the authors should look into the literature. Coupling between FAST and LES has been done for a long time in other codes, see for example: <https://www.nrel.gov/docs/fy12osti/53567.pdf> <https://iopscience.iop.org/article/10.1088/1742-6596/1452/1/012071/meta>.*

AC Since this comment is aimed at the literature review, we assume that it is actually concerned with Section 1. This section aims to give an introduction to the topic to a wide range of readers with different backgrounds. We agree that the overview was too broad in some parts and not deep enough in others. We have addressed this by omitting some overly detailed information and adding more detail on LES/FAST couplings, as suggested.

RC5 *Section 2: Is there a subgrid-scale turbulence model? What are the numerics of the LES code?*

AC Yes, the LES code PALM offers different subgrid-scale (SGS) turbulence models. The default model was used, which in the revised manuscript was included in the description of the set-up in section 3. In addition, the description has been expanded to include information about the time stepping and advection scheme and the pressure solver.

p. 7, ll. 179f: Additionally, the surface condition is set to a free slip condition. PALM offers different possibilities for the subgrid-scale turbulence closure. For the simulations mentioned in this work the default model was used which is a modified version of Deardorff’s subgrid-scale model (Deardorff, 1980), as mentioned in (Moeng and Wyngaard, 1988) and (Saiki et al., 2000). The time stepping and advection schemes were used in the default settings as well, which is a third order Runge-Kutta scheme (Williamson, 1980; Baldauf, 2008) for time stepping and a fifth order upwind scheme, based on (Wicker and Skamarock, 2002), for the advection. The pressure solver was set to the multigrid option (Uhlenbrock, 2001).

RC6 *Section 3.1: It is not clear what the difference between the 4 modeling approaches is. A table would be a good way of describing them.*

AC A table giving an overview of the models is helpful to understand the differences more quickly. Therefore we have added table 1.

Table 1. Overview of the turbine models that were used in the comparisons. The new enhanced coupling method is ASM, the respective time steps in PALM and FAST are denoted as Δt_P and Δt_F respectively and the inflow wind speed as U .

Name	Time step	Wind speed information	Rotor model in PALM
ALM	coupled time step: $\Delta t_P = \Delta t_F$	U taken at positions of rotor blade elements in PALM	line
ASM w/o SWIRL	decoupled time step: $\Delta t_P = n \cdot \Delta t_F$	U taken at positions of rotor blade elements in PALM, from a frozen wind field	sector
ASM	decoupled time step: $\Delta t_P = n \cdot \Delta t_F$	U taken upstream of the rotor blade positions in PALM and use of the induction model SWIRL of FAST	sector
FAST	only FAST Δt_F	steady wind case in FAST, no LES	-

RC7 *Section 3.1: The authors only compare power output. To improve the comparison, the authors should look at some quantities along the blade such as velocity, lift, force, etc. Also, how many points along the blade are used?*

AC Thank you for this suggestion. We have added a comparison of dynamic pressure, lift and drag coefficient as well as the angle of attack along the blade for the laminar simulation of the NREL 5 MW turbine. Additionally, we had to renew the ALM curve in figure 3 as the previous one had a different input in comparison to the other methods, however, this does not affect the results.

p. 8, ll. 198f: A comparison of quantities along the 62 blade nodes show a difference between the methods using wind speeds at the rotor blade positions (ALM and ASM w/o SWIRL) and the two methods using a different inflow, namely ASM and FAST (figures can be seen in Appendix A). The distribution of the angle of attack shows a smoothed curve for the ALM and ASM w/o SWIRL, which is due to the smearing of the forces around the rotor blades. On the other hand ASM and FAST show a choppy curve due to the different airfoil profiles along the blade, here, it can be seen at which position a change of an airfoil profile and twist angle along the blade is predefined in the NREL model. These differences are transferred to the lift and drag coefficients. Additionally, for dynamic pressure, it can be observed that ALM and ASM w/o SWIRL overestimate the dynamic pressure at the blade tips and slightly underestimate it at the hub compared to FAST and ASM. These observations suggest that the smearing of the forces along the blade has a great influence on the lift and drag properties and thus the turbine response.

RC8 *Section 3.1: “The wind speeds at the rotor area are then estimated using the induction model SWIRL of FAST. SWIRL uses the so-called Taylor’s frozen turbulence hypothesis (Taylor, 1938) and calculates the induced velocity in axial and tangential direction”. It is not clear how this is done. I could not find the “SWIRL” model in the literature. FAST uses blade element momentum theory to compute induction.*

AC The documentation of FAST is incomplete regarding SWIRL. SWIRL is based on the calculation of the induction factors according to (Harman, 1994), which is only documented in the source code of FAST. We have clarified the respective sentence.

p. 5, ll. 141f: The wind speeds at the rotor area are then estimated using the induction model SWIRL of FAST. SWIRL uses the so-called Taylor’s frozen turbulence hypothesis (Taylor, 1938) and calculates the induced velocity in axial and tangential direction. In (Moriarty and Hansen, 2005) the Aerodyn model of FAST is described, including the blade-element momentum (BEM) theory to compute the induction. The calculation of the induction factors when using SWIRL is based on (Harman, 1994). With enabling SWIRL we assume, that the turbulent structures in the wind field do not change while moving to the turbine.

RC9 *Section 3.2: What is the power of the reference turbine? The y-label of figure 4 does not seem to be right.*

AC Yes, you are correct, there was a mistake in the y-scale of figure 4. Figure 4 shows the correct scale.

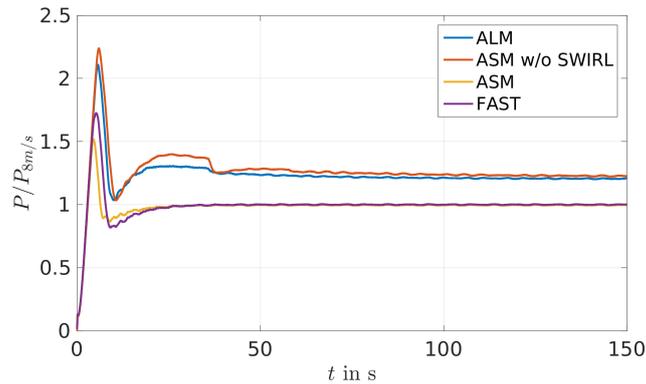


Figure 4. Comparison of different simulation methods for the generator power of the eno114 3.5 MW turbine in a laminar case with a wind speed of 8 ms^{-1} , normalised by the respective value of the eno power curve at 8 ms^{-1} .

RC10 Section 3.2.1: Figure 5 – This figure needs to be improved. Please add some distances and arrows indicating the wind direction.

AC Thank you for the hint about the missing distances, we have adjusted the drawing accordingly (see figure 5). However, an indication of a specific wind direction may not be useful, since measurement data from all wind directions was available and filtered in such a way that neither the forest nor the measurement mast influenced the used data. The wind directions remaining after the filtering are indicated in red. We have modified the figure caption accordingly.

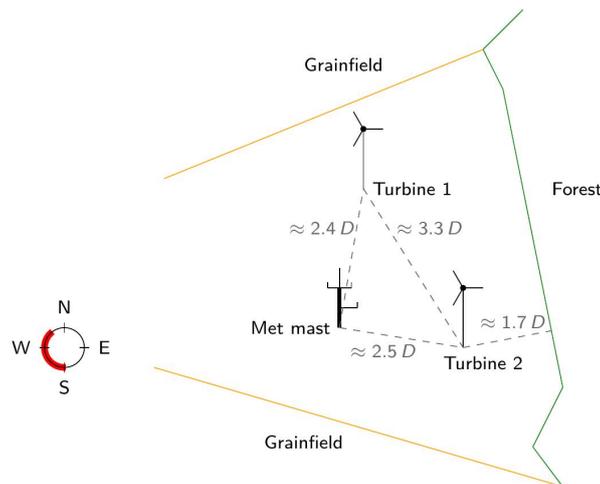


Figure 5. Schematic of the measurement site in Brusow. The remaining wind directions in the measurement data, after filtering, are indicated in red; D is the turbine diameter, here $D=114.9 \text{ m}$.

RC11 Section 3.2.3: There are too many plots in this section. Some plots can be combined into the same figures. For example, the different root bending moments M_x and M_y can be in the same figure next to each other.

AC There is a large number of plots in this section. However, they all show different information. The suggestion to combine some plots into one figure was very helpful and was implemented where possible.

Authors response to comments from referee #2

General comments

RC1 *The methodology is not properly described, limiting the impact and reproducibility of the present research. Examples of these are:*

a) PALM is only introduced as a “LES-tool”. No details about the solver/model are included, apart from the description of the mesh and the boundary conditions. Indeed, no figure of the flow solution is included in the manuscript.

b) FAST is introduced as an aeroelastic tool, but no details about the solver are given. The same comment applies for the FAST model used for the present work.

c) The wind turbine used in the experimental campaign is not properly introduced. For instance “Eno” is not introduced. Is it a wind turbine manufacturer?

d) The developed methodology is presented as a higher fidelity approach, with respect to other techniques (such as RANS for the flow modeling, or the actuator line for the coupling). However, the assumptions and limitations of the proposed methodology are not put into context. For instance, no mention of rotor-resolved simulations is made in the introduction. In that context, the reviewer is interested in the limitations of the “frozen” windfield (could that compromise the accuracy of the method for particular situations?). However, this issue does not seem to be discussed in the paper.

e) Is the ASM approach developed within the framework of the present work?

f) Line 164: The authors could give more details around the simulated case (such as omega and pitch).

g) Figure 5 could contain more info (such as the relative distance between the wind turbines).

h) No details about the controller are given.

AC

a) Thank you for that hint. As PALM is a complex model we referenced two PALM papers with further, detailed information. However, we agree that more detailed information can improve the quality of the paper, so we added some information on PALM in general and on the settings used by us in the description of the simulation set-up, i.e. the subgrid-scale model, the time stepping scheme, the advection scheme and the pressure solver.

p. 3, ll. 87f: PALM enables the simulation of an atmospheric flow for a wide range of different situations, like e.g. different stabilities using heating or cooling of the surface. It is based on the non-hydrostatic, filtered, incompressible Navier-Stokes equations in Boussinesq-approximated form and has seven prognostic quantities: the wind speed on a cartesian grid u , v , w , the potential temperature Θ , the water vapor mixing ratio q_w , a passive scalar s and the subgrid-scale turbulent kinetic energy e . The domain is divided into equidistant cells in horizontal direction, stretching of the cells is possible in vertical direction. To define the position of the quantities the Arakawa staggered C-grid (Harlow and Welch, 1965; Arakawa and Lamb, 1977) is used.

p. 7, ll. 179f: PALM offers different possibilities for the subgrid-scale turbulence closure. For the simulations mentioned in this work the default model was used which is a modified version of Deardorff’s subgrid-scale model (Deardorff, 1980), as mentioned in (Moeng and Wyngaard, 1988) and (Saiki et al., 2000). The time stepping and advection schemes were used in the default settings as well, which is a third order Runge-Kutta scheme (Williamson, 1980; Baldauf, 2008) for time stepping and a fifth order upwind scheme, based on (Wicker and Skamarock, 2002), for the advection. The pressure solver was set to the multigrid option (Uhlenbrock, 2001).

b) As far as FAST is concerned, most of the used set-up is described in section 3.2.2. However, more information about the AeroDyn module has been added.

section 3.2.2, Ende: In AeroDyn the Beddoes-Leishman dynamic stall model, based on (Leishman and Beddoes, 1989) and the "Equil" option, a BEM model, for the inflow was selected. Additionally, the tip-loss and hub-loss models were enabled and set to the Prandtl tip loss model (Prandtl and Betz, 1927).

c) Yes, eno is a wind turbine manufacturer. We added the information when first mentioning details about the turbine, further information can be found in the referenced data sheet.

p. 8, ll. 202f: Measurement data of an eno114 3.5 MW turbine, manufactured by eno (eno energy, 2019), with a hub height of 92 m and the corresponding FAST turbine model are used for further investigations.

d) We have made additions concerning this in the introduction and in the conclusion.

p. 2 ll. 59f: A fully resolved wind turbine simulation can lead to the same or even more detailed output, but is far more computationally intensive than the presented framework.

p. 26 ll. 463f: In the current work, the constraints of the frozen wind field, e.g. the assumption of Taylor's frozen turbulence hypothesis, does not limit the outcome, as in the current simulations, the statistics of the flow are not subject to varying wind conditions. However, there are also situations where the hypothesis will reach its limits, e.g. with temporally variable wind fields or changing wind direction. The case of a turbine in a wake also needs further investigation, as the recovery of the wake in the frozen wind field has not been considered so far.

e) This particular sector method was developed within the framework of the present work. However, different sector methods were already existent, like e.g. the version published in (Storey et al., 2015), as mentioned in the introduction.

p. 3 ll. 65f: We developed one variation of an Actuator Sector Method (ASM), where the blade movement is described as a segment of a circle. This allows for a larger time step in PALM than in FAST as the movement of the blade during that time step is captured in the area of the sector.

f) As the controller of the NREL 5 MW turbine was used as it is available from NREL and described in (Jonkman et al., 2009b), the information on the behaviour of the NREL 5 MW turbine is already described in more detail in other publications. However, the information, that the used controller does not deviate from the known turbine response was added to the paper.

p. 7, ll. 185f: The standard controller of the NREL turbine is employed as described in (Jonkman et al., 2009b), which means that at the prevailing wind speeds no pitching of the blades is enabled.

g) Yes, figure 5 needed to be improved, the modified version can be seen in figure 5.

h) The NREL turbine uses its standard controller, which is included when downloading the turbine model and is described in (Jonkman et al., 2009b). The controller of the eno turbine is confidential and was not made public to us, only a .dll file was used.

p. 16, ll. 315f: The turbine model of the eno114 3.5 MW turbine for FAST was provided by eno, including structural information and a pitch, a speed and a yaw control in the format of a Bladed .dll file, which was not accessible to us.

RC2 While the manuscript is significantly long, the discussion on the results is often very vague. Examples of these are:

- a) Figure 3: A discussion of a longer time series, based on statistics/frequency domain, would be necessary before concluding on the capabilities of the method.
- b) Figure 7: One could also quantify these differences, rather than only providing a qualitative statement.
- c) Line 407: Discussions around the effect of the wind turbine controller seem to be vague. Could this issue be overcome by a re-tuning of the employed FAST controller?

AC

a) The above mentioned time series was not intended for a deeper analysis of frequencies or statistics. The intention for using the NREL turbine in a turbulent simulation was to examine whether the method of using wind speed in front of the turbine and assuming Taylor's frozen turbulence hypothesis would lead to a useful result. In particular, it was intended to investigate whether the turbulence can be represented in a comparable way to other models in the power output. The used simulations were stationary due to their preruns and therefore a good starting point for a comparison. When running the different models for a longer time, the results will drift apart at some point due to the different turbine responses, as interferences in the wind field are transported upstream as well due to the pressure solver. However, this was not the objective of this particular simulation.

b) We have replaced the plot of the power curve with figure 7, here the differences can be seen more clearly and the deviation between the stabilities can be read directly from the plot. The new plot shows the power curves normalised by the respective eno power value, which is determined in simulations using FAST and a laminar situation.

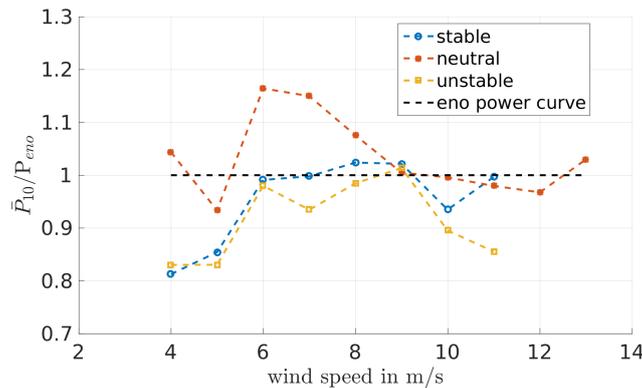


Figure 7. Power data determined from the measurement data for May/June 2017, normalised by the corresponding power of the eno power curve determined by FAST in laminar conditions, for different stabilities (determined from eddy-covariance data).

c) As mentioned above we did not have access to the eno controller and therefore no information on how the controller influences the results. However, on further dealing with the above mentioned paragraph we now think that the differences can be explained by the limited turbulence intensity (TI) variation. The simulations only cover a small TI range, the differences between the runs are not very large. In contrast, the measured data show a greater variability of the TI, which can lead to a higher power output in the neutral case than in the stable case at the same rotor speed, whereas this is not visible in the simulation.

p. 24, ll. 440f: However, the simulations did not reproduce the same dependency, which might be explained by the limited variability of the TI in comparison to the measurement data. As can be seen in figure 14 the simulations cover the lower limit of the TI in the respective wind speed.

RC3 *From the reviewer perspective, the methodology followed to illustrate the performance of the method is insufficient. The comparison exercise around the NREL 5MW is limited to the integrated loads of a very particular case, and a rather poor analysis of a very short time series. It should be emphasized that, contrary to the title of the section, that comparison cannot be classified as a validation (since no experimental results were considered). The reviewer also misses the presence of the other studied methods in Section 3.2. That could be particularly relevant, given the amount of uncertainty of the comparison itself.*

AC Yes, thank you for this suggestion, section 3.1 discussing the NREL turbine is misleading when using the title "Validation [...]". The section was not intended for a sole validation, but was seen as a trial of the general performance of the enhanced coupling. As we were using wind speeds in front of the turbine, assumed Taylor's hypothesis and used a force projection of a sector, in contrast to the previously employed methods, an evaluation of the general performance was our first intention. The purpose was to answer whether in general the output seemed reasonable, whether the coupling would work in a turbulent flow and whether the computational time is reduced. Once, the basis performance of the enhanced coupling was satisfying an extensive validation with measurement data was done. A further analysis, including more thorough simulations, with the NREL turbine did not appear to be very effective, due to the lack of measurement data of the NREL turbine.

Nevertheless, this was not made clear in this section and needed to be clarified in the text.

In section 3.2 the focus was set on only the ASM method as the intention was to validate the simulation results and not compare the behaviour of different methods. Also, the other methods, that were employed in section 3.1 each have a drawback, i.e. the computational resources for the ALM are very high and due to limited memory and computational time further simulations were not carried out. The ASM without the induction model SWIRL showed already in section 3.1 that the results are not as good as with the induction model, therefore it did not seem feasible to follow up this method. Lastly, using FAST without a coupling to PALM needs a different source for a turbulent windfield. Generating one with a different tool than PALM would complicate a possible comparison, as this would add a new variable. Furthermore, the result of a FAST simulation with a different inflow would not be helpful in validating the enhanced coupling method.

p. 6, ll. 164f: 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.

p. 8, ll. 226f: Through these simple simulations it can be seen that the sector methods offer savings in the computing time in comparison to the ALM. However, the ASM w/o SWIRL does not provide the expected results. Therefore, it is considered useful to compare the ASM with measurement data in the following.

Specific comments

RC4 *Line 37: "There are different ways to model a turbine, as can be seen in ..." -> Maybe this sentence could be more clear, by replacing by something along the lines "to model the presence of the wind turbine in the flow".*

AC Yes, the phrasing was a bit too vague. Thank you for the suggestion.

p. 2, ll. 29f: There are different ways to model the presence of a wind turbine in the flow, as can be seen in e.g. (Witha et al., 2014) and (Wu and Porté-Agel, 2013).

RC5 Line 55: “Computing routine” could be replaced by something more precise, for instance “computing framework”.

AC Computing routing might be too general for a simulation method, the suggested alternative seems to describe the program better and was changed.

p. 2, ll. 56f: To address the problem of losing information of either the turbine or the flow and provide a reliable tool, we present a newly developed computing framework here, with which it is possible to calculate LES in combination with a well resolved turbine model, i.e. apart from the power output also quantities for the blades and along the blades are available.

RC6 Line 153: would be more readable if references were grouped (citeref1,ref2,ref3).

AC Thank you for that suggestion.

p. 6, ll. 160f: The NREL 5 MW turbine (Jonkman et al., 2009b) is a generic turbine which has been used extensively in simulations (Churchfield et al., 2012; Storey et al., 2013, 2015; Vollmer et al., 2016; Sathe et al., 2013; Lee et al., 2012).

RC7 Line 172: “coincides with the expected value” -> “with the value computed by NREL, based on the same FAST model?”

AC Yes, all four models use the same FAST model of the NREL turbine, but different ways of retrieving the wind speed information or distributing the forces back in the flow field. This is now summarised in table 1. Related to specifically the method of using solely FAST it is correct, that it is the same calculation with the same tool and model as presented in (Jonkman et al., 2009b). As there are quite a number of possible input variations in FAST it seemed sensible to check whether our simulation set-up coincides with the one used in (Jonkman et al., 2009b).

p. 7, ll. 188f: The result calculated by FAST coincides with the value, as published by NREL (Jonkman et al., 2009b), based on the same FAST model.

RC8 Line 411: “Turbine answer” -> “turbine response”

AC Thank you for pointing that out.

p. 25, ll. 444f: In this paper we presented a new computing framework which combines the advantages of an atmospheric flow simulation using the LES tool PALM and the detailed calculation of the turbine response by FAST.

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