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Interactive comment on "Simulation of transient gusts on the NREL5 MW wind turbine using CFD" *by* Annika Länger-Möller

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Dear Anonymous Referee #1,
Thank you very much for your comments that certainly will improve the paper. In the following I will answer your questions and indicate which information is added to the original paper.

Overall

RC:*The paper presents CFD(URANS) simulations of the NREL 5MW wind turbine. The aim is to examine transient gusts and the associated load implications. The underlying research question is interesting with some interesting conclusions, e.g. that the presence of the tower suppress separation. However, the article appears incomplete and somewhat unfocused, hence confusing at times. The written language is mediocre and could be improved for readability. Therefore, the recommendation is to reject the paper in the current state, but encourage to resubmit at a later stage. The author addresses a number of issues in the conclusion and these should be answered for resubmission. Hence, a more clear focus should be applied, e.g. how related is the tip vortex transportation actually to the transient loading during a gust?*

AC: To improve the language, I shortened the sentences and reordered them to strictly match the subject-verb-object order. Moreover, I changed some wordings to improve the readability of the paper. This hopefully will clarify the focus on the validation of the method, which is the main purpose of the paper.
I also fixed the main issue in my conclusion, namely the validation of the resolved-gust approach. The CFD(U-RANS)-computation is now compared to FAST and shows very good agreement in flow regions with attached flow. I added the according figure to my response. Besides, everything else would mean to try to run before I can walk.
The tip vortex transport is an indication whether the method works correctly. It has little effect on the rotor loading. This information is added to section 5.3.

General Comments

1. RC: *Why are the gusts propagated with the speed of sound? This appears an odd choice as a real gust would propagate by its own velocity.*

RC: Yes you are right. It is the weak point of the chosen modelling approach. In combination with an incompressible solver it becomes severe as the speed of sound is infinite. This point has already mentioned in section 1, page 2, line 27. The use of the field approach by Parameswaran et al. or the velocity disturbance approach would overcome this issue but introduce other problematic questions. I will expand the explanation in section 1.

2. RC:*Why is the floor modelled, but no ABL? If a no-slip condition is applied, why not have a shear? The dependance on height appears to disappear without explanation in the equtions. Otherwise, investigate a uniform inflow with the turbine "suspended" in space, which would give symmetry.*
   AC: To be able to introduce the ABL in future computations and to perform a comparison of both computations on the same grid, the grid has already been prepared accordingly. I added this information to section 3.2. Nevertheless, the first step in introducing a new method is to validate the simulation method as isolated as possible. Thus, the height dependency of the velocity is neglected to be able to evaluate the gust impact on the rotor aerodynamics. I added the information more clearly in the paper in section 3.1 4.1, 4.3.

3. RC: *The turbine is stiff in the computations. Is this choice appropriate when examining extreme loads, particular for large wind turbines as the 5MW? This also affects the observed symmetry in Table 1 and whether this is to be expected.*
   AC: The first step in introducing a new method with interdisciplinary character is to validate each part of the method on its own. The combination with structure dynamics and/or speed controllers can only follow after the issues in remark [1] and [13] are clarified. The motivation is clarified in section 1, page 2. Moreover, a remark about the plausibility of symmetric results is added in section 5.2, 5.3.

4. RC:*Improved description of flow solver. What does dual cells, projection methods, prism layers, and C functions imply?*

AC: Dual cells imply a cell-centred scheme of the solver. With the projection method the momentum equations are first solved with an approximated pressure field and do not fulfil continuity. The pressure field is then corrected by solving a Poisson equation to fulfil the continuity conditions. The c-functions enable a high flexibility on the definition of inflow conditions on the boundaries of the flow domain. The description of THETA in section 2 is extended to answer your questions. Prism layers are part of the meshing topology.

5. RC: *Why are there meshing issues involved for the nacelle, but not the hub nor tower?*
   AC: Due to the narrow gap between rotor and nacelle a valid chimera overlap-region could not be achieved. Thus the nacelle of the NREL 5MW turbine is neglected while the tower is respected. This sentence is added to section 3.2

6. RC:*How many cells in first grid?*
   AC: 11.6 Mio in the chimera child grid and 13.3 Mio points in the parent grid. The information has been already on page 4, lines 20 and 26 in the first version.

7. RC:*The boundary condition definition is unclear, e.g. "the remaining farfield surface"?*
   AC: It is the surfaces on top, left and right of the flow domain. I clarified this in the paper, section 3.2.

8. RC: *How is such a small gust interesting? The cos gust basically results in a TI of* 1.5% *(=(sqrt(2)/2*0.25m/s)/11.4m/s, hardly a defining design case.*
   AC: It represents the turbulence level of the atmospheric flow around wind turbines as measured by Schaffarczyk et al. (2017). A remark is added at the end of section 4.2.

9. RC: *How are the characteristic times chosen? And why would they be sinusoidal?*

AC: The characteristic time for the 1-cos gust was chosen to generate an uncompressed cosinus larger than one rotor revolution and smaller than a 10s interval. The characteristic time for the EOG is taken from the IEC standard. A comment is added in the sections 4.2 and 4.3.

9. RC: *And why would they be sinusoidal? It might follow the standards, but does this correspond to measured gusts or gusts from LES?*
   AC: Well, this opens up a wide research field and several attempts are made to find profound answers to this question. One example is Bierbooms et al. (1999) who investigated the gust shape from wind measurements and simulation. By the use of statistical methods they found that a sinus fairly represents gust. Alternatively, Zbrozek (1953) gives an overview on different possible gust shapes. Concerning the agreement of the standards and results from LES or field measurements, Mücke et al. (2010), for example, showed that in field measurements velocity-changes with high frequency occur. These are not represented in the IEC standard. Apart from that, the question to be answered by my paper neither is whether or not the implemented gust corresponds to any field measurement or LES computation with the specific related uncertainties. Nor it is the purpose to answer the question how the IEC standard may be improved. The aim of the paper is to validate the resolved gust approach, which requires

   - low uncertainties in the inflow conditions which is achieved by perfect control of the inflow conditions
   - low uncertainties in the inflow conditions by applying a gust that is independent of the horizontal and vertical position
   - comparability to results of other methods such as FAST by using well-defined inflow conditions

   All of the above requirements are best matched by the IEC standard gust. The introduction of realistic fluctuations which result from LES or measurement can be applied as soon as the validation of the aerodynamics is finished, aero-elasticity has been included and a speed controller is available.

10. RC: *Explain spikes in e.g. Figure 3 around t = 3230 sec.*
AC: The high-frequency oscillations occur in a time step wherein the elliptic pressure equation has not reached the required residual of 10^{-5} in the maximum allowed number of iterations. The number of iterations were chosen to guarantee an efficient computation of the test case. The information about the criteria is added to section 2. Moreover, a remark on the high-frequency oscillations is added to section 5.1.

11. RC: *Is the use of average loads correct? Most people use equivalent loads.*
AC: To be able to compute and discuss equivalent loads assumptions on the wind field, wind velocity bins and statistics have to be included to the discussion in the paper. This would create new questions and unnecessary uncertainties to the results. Therefore I included a more profound comparison of averaged and peak loads to section 5.2 that clarifies the introduction of the averaged gust loads.

12. RC: *Details are difficult to see in Figures 7-12.*
AC: I modified the figures to better illustrate the content. Moreover, I enlarged the figures.

13. RC: *In terms of experiments, why not validate the setup against the MEXICO experiments or Krogstad as there is nothing "special" about the NREL 5MW.*
AC: The validation of the setup against any experiment is not a matter of question because a validation against the NREL 5MW is performed in section 5.1 and a successful validation of THETA against the NREL phase VI UAE has been presented by Länger-Möller in 2017. It is the gust simulation that needs validation. Meanwhile I found a possibility to validate the gust simulation against FAST results. I included the results to figure 5 and according comments to section 5.3. The Conclusion in section 6 has been changed accordingly.

14. I added a short description of FAST and the modelling approaches in section 2. That includes a modification of the section title to "Numerical methods" and a shift of the former section 2 to 2.1

Technical corrections:

1. RC: *Why are all references written twice? Please correct.*
   AC: References are now written once.

2. RC: *Wording is often rather strange, e.g. use of "regarding", "promising"(page 3, line 27), "respecting",*
   AC: I changed the wording you indicated.

3. RC: *Sentences are back-to-front, e.g. page 2, line 9-10. Please correct*
   AC: I changed the wording in sentences on page 2, line 9-10 and some other back-to-front sentences.

4. RC: *. Consistency. On page 2, there are mentioned "CFD" several times, while aero-elastic tools are denominated. Please also specify which CFD tools were used as there are large differences between "CFD" tools.*
   AC: I specified the used CFD tools more clearly. This implies the title. Moreover I added a short description of the most popular aero-elastic wind turbine tools.

5. RC: *Periodic and initial is not the same(page 7, line 15).*
   AC: Of course, periodic and initial is not the same. But the periodic state of the flow field is mandatory as starting (initial) conditions to start a gust-computation. I clarified this in the mentioned paragraph.

Moreover, I performed the following changes:

- I changed the word "hexahedra" to "hexagon"
- I adapted wind velocity to wind speed
- I adapted gust speed to gust velocity

throughout the entire paper.