Review of the manuscript, titled "*Transient LES of an offshore wind turbine*"

In this manuscript, a methodology is presented, in which a large-eddy simulation (LES) model is combined with a numerical weather prediction (NWP) model to simulate the flow through an offshore wind farm. The results of this combination of models are compared with field measurements (both lidar and met mast) in an offshore wind farm in the North Sea. The topic is interesting and it is a direction, in which wind energy community should/will eventually move. Nevertheless, there are some issues that the authors should address, before this manuscript can officially get published. These issues are listed below:

- The statement that the authors have made about LES in their discussion section (Page 16, L 5-8) is very premature and too generalizing. The only thing one can conclude about the comparison of the LES results and the measurements reported in this paper (in its current form) is: the type of LES used in this paper <u>and</u> the methodology used in this paper to combine LES and NWP <u>and</u> the way LES is set up in this paper <u>and</u> the way LES was run in this paper <u>and</u> the way the results (both LES and measurements) were post-processed in this paper, resulted in the comparison reported in this paper. In fact, the accuracy of LES in prediction of wind-turbine wakes has been well tested and proved to be satisfactory in the literature. I think, with respect to these results, the authors should explain and explore what has gone wrong or what the inherent limitations are, rather than simply stating "*We find however no proof that the LES can improve the quality of the comparison…*" or "*…LES is not able to push these values closer to the measured values.*"

  Here, another question comes to one's mind: have the authors tested and validated their LES for flow through wind turbines against more controlled and classical wind-tunnel experiments (which exists in the literature)? (if yes, please mention the reference in the text) It is very important that authors first make sure their LES model combined with their actuator disk model works well, and only then they can aim to test the accuracy of LES in such a complicated case that is described in this paper. In other words, the steps should be taken one by one.

- Regarding the inflow boundary condition the authors have written:
  " *… we made simulations without the turbine in parallel and simulated only intervals of 30 min maximum with wind turbine with a 3 min precursor phase for the development of the wake.*"
  It is clear that the authors have used a precursor simulation to generate the inflow; however, it is very vague how they have done it. Please state very clearly how you have performed your precursory simulation and how you have linked your precursory simulation to your main simulation. Have you used a buffer/fringe zone to overcome the periodic boundary condition? How have implemented it? In validation of LES results against measurements, it is well known that, an appropriate inflow is crucial. For example, one can consider this quote from Andreas Kempf:
  "*In general, LES inflow-conditions involve far more detail than those for a RANS, and if the LES is not provided with this data, the results cannot be expected to be superior to RANS results.*"[1]

[1] Kempf, Andreas M. "LES validation from experiments." *Flow, Turbulence and Combustion* 80.3 (2008): 351-373.

- In your LES formulation (Eq. 1), please indicate the term responsible for the SGS stresses. Is "kinematic viscosity of momentum" the turbulent viscosity?
- In Page 10, Lines 5-10, you have discussed about the choice of the relaxation time. You have finally chosen tau=4 h. You have first mentioned that the errors with respect to FINO1 measurements are not a good criterion to assess the value of tau, and then you have said that the choice of tau was a qualitative decision. Please substantiate the choice of tau. Here it seems tau=1.5 h is a better choice based on the errors, and with tau=48 h "LES boundary layer develops more independently". So what was your criterion/criteria for choosing tau=4 h?
- Define exactly and mathematically the "wake width" and "wake deficit". Explain how you have calculated these quantities both for lidar data and for LES data.
- The correspondence of Fig. 12 and Fig. 9 seem questionable. For example, based on Fig. 12 the width of the wake measured by lidar in the evening period has doubled from y=200 m to y=500 m; however, one cannot see such an increase in Fig. 9. Moreover, in the night period, based on Fig. 12 we have a quick 0.5D jump in the wake width between y=300 and 400 (for the lidar data); this jump is again not clear in Fig. 9. It seems to me a bit of inconsistency. Furthermore, the differences in the wake width in Fig. 9 (between LES and lidar) does not seem as large as in Fig. 12.
- In Fig. 10, the difference between panel (a) and (b) is not clear. It is not mentioned in the caption, and it is not even discussed in the text.
- Please indicate in Fig. 1 the position of the met mast "FINO1". In the same figure, please indicate the North direction.
- Some editorial comments:
    - Make a distinction between RMSE symbol used in Table 1 and the RMS of fluctuations used in the rest of the paper (e.g. Fig. 7, 8, etc.).
    - Define all the abbreviations the first time you use them in the text. For example: DEWI, PPI, PALM.
    - "u_g" in Eq. 1 has the "LS" subscript, but in Eq. 3 does not. Are they the same variables? If yes, use a consistent notation for both.
    - Page 5, Line 10: I think you need a comma after "Compared to onshore"
    - Page 2, Line 3: "are" should be changed to "is".