Review of *Effect of tip spacing, thrust coefficient and turbine spacing in multi-rotor wind turbines and farms* by Niranjan S. Ghaisas, Aditya S. Ghate, and Sanjiva K. Lele.

Reviewer: M. Paul van der Laan, DTU Wind Energy

The authors employ large-eddy simulations (LES) to quantify the difference in wake effects between a four-rotor wind turbine and an equivalent single-rotor wind turbine, for both an isolated wind turbine and a row of five wind turbines. The present work is a continuation of a conference article, where more parameters are investigated. An engineering wake model is calibrated with the LES data and its performance for a row of five four-rotor wind turbine is investigated. The article is well written and provides interesting results. I have written a list of main and minor comments below:

Main comments

1. Pages 1-2, Lines 57-64. You explain the difference choices compared to van der Laan et al. (2019), however, there are some misunderstandings:
   (a) You are right that in van der Laan et al. (2019) the thrust coefficient was different between the top and bottom rotors due to the shear and the rotor interaction, and the thrust force distribution was non-uniform for most of the published results by using body forces based on airfoil data. However, in the wake recovery analysis (which you focus on in this work) based on the Reynolds-averaged Navier-Stokes model, the same thrust force distribution and total thrust force was prescribed for the multi-rotor wind turbine and the equivalent single-rotor wind turbine, in order to make the comparison of the wake recovery more fair.
   (b) The inflow in van der Laan et al. (2019) is logarithmic, which corresponds to a neutral atmospheric surface layer (where the boundary height is infinite and the flow solution is independent of the Reynolds number). This means that the flow solution scales with the rotor diameter (d) and hub or multi-rotor reference height (H_t). In other words, the normalized wake deficit for using $d = 29.2$ m or $d = 50$ are the same, as long as H_t is also up-scaled accordingly and the turbulence intensity, thrust coefficient, and tip clearances are the same. However, you can mention in your work, that you actually use a finite boundary layer height, instead of logarithmic profile with an infinite boundary layer height, as used in van der Laan et al. (2019).
2. You lack a recent reference to van der Laan and Abkar (2019), where the work of van der Laan et al. (2019) has been extended to multi-rotor wind farms. (The work was presented at the Wake Conference 2019 in May, but has been published online with some delay, in July, so you probably have missed this.) In this article, simulations suggest that multi-rotor wind farms produce 0.3-1.7% more annual energy production compared to equivalent single-rotor wind farms. The increase is mainly caused by the first downstream wind turbine in a wind farm and for row-aligned wind directions. This is in contradiction with your previous work Ghaisas et al. (2018) and it is related to the large rotor spacing of $1d$ that you had chosen, as shown in van der Laan and Abkar (2019). I think you should reference this work and highlight (in the introduction) that the difference between the single-rotor wind farm and multi-rotor wind farm in Ghaisas et al. (2018) was large due to the chosen tip clearance of $1d$. You could mention that the present work is used to investigate more realistic tip clearances. It is nice to see that you get similar trends in the power deficit (Figures 9 and 10), as published in van der Laan and Abkar (2019).

3. Page 4, Line 79: The relation $C_T = 16C_T^{'}/(C_T^{'}+4)^2$ is only true for an axial induction factor a_x of 1/3, since 1D Momentum Theory for a single-rotor in a uniform flow gives: $C_T = C_T^{'}(1-a_x)^2$ (which you also show in equation A1). In Table I: you investigate three different thrust coefficients (0.64, 0.75, 8/9). For 1D Momentum Theory for a single-rotor in a uniform flow we have that $C_T = 4a_x(1-a_x)$ or $a_x = 1/2(1-\sqrt{1-C_T})$. Hence, for $C_T = 0.64, 0.75, 8/9$ we get $a_x = 0.2, 0.25, 1/3$, respectively. I would replace $C_T = 16C_T^{'}/(C_T^{'}+4)^2$ with equation A1.

4. Lines 275-277: When you change the tip clearance, the amount of potential power changes due to the shear. Have you investigated how this affects your results? My guess is that the shear is the main reason why the power of the first wind turbine changes with different tip clearances (as you shown in Figure 11a).

5. Figure 6c: The single-rotor wind turbine seems to have a much higher disk averaged ambient turbulence intensity. Why is this the case? When I looked at the integrated added wake turbulence intensity, I found that the four-rotor wind turbine has a higher integrated added wake turbulence intensity in the near wake compared single-rotor wind turbine, while the opposite is found for the far wake, see Figure 18 in van der Laan et al. (2019).

6. Pages 18-22: Engineering wake model vs LES:

   (a) Please motivate the use of a spatial varying wake expansion parameter k_*.

   (b) Do I understand correctly, that you only fit k_* from the single-rotor wind farm simulations and use this directly to predict the deficits in a multi-rotor wind farm (Figure 13)?

   (c) Figures 21 and 22: Please define the numbers colored in red in the caption, I guess it is the relative error. How did you compute it? $(P_{2-5}^{\mathrm{model}} - P_{2-5}^{\mathrm{LES}})/P_{2-5}^{\mathrm{LES}}$?

   (d) How large are the differences between the wake model and LES if you had used a constant k_*? This is a relevant question because it is the standard usage of the chosen wake model.

7. Page 20, Discussion: Some wording needs to be changed here:

   (a) Line 337: Please remove the word *novel*, since the four-rotor wind turbine design is not new.

   (b) Lines 338-340: Please remove *for the first time*, since there are several authors (including yourself) have investigated the four-rotor wind turbine design.

8. Line 354: Please change *between different units* to *between different wind turbines*, or do you mean something else?

9. Line 286 and Lines 363-364: What do you mean by *The effect of the axial spacing on the benefit is ambiguous, since it is non-monotonic.*?

10. Appendix A: It is nice that you have added this appendix in order to make the comparison of a single-rotor and multi-rotor wind farm more fair, but you forgot to refer to it in the text.

Minor comments

1. Figures 5, 14 and 15 have very large labels compare to the rest of the figures. I would look nicer to keep the same label size.

2. You have normalized the velocity deficits by u_*, but this makes it hard to see how large the deficit actually is. It would be more interesting to normalize by the freestream velocity (which could be an integral over the disk area). When you normalize the turbulent kinetic energy by u_*^2, you could instead plot the turbulence intensity or added wake turbulence intensity, which is more common for wind turbine wake studies.

3. You both mention thrust coefficient and local thrust coefficient when you talk about $C_T^{'}$. I would stick with local thrust coefficient everywhere to avoid confusion.

4. Figures 8, 9, 10, 12 and 13: I would write the wind turbine number (1, 2, 3, 4, 5) on the x-axis instead of x/D. This also corresponds better to the text, because you often talk about wind turbine numbers.

5. Figure 8: There are additional numbers plotted in Figure 8a.

6. You could refer to Niayifar and Porté-Agel (2015) when you talk about the relation between the local turbulence intensity and the wake recovery parameter k_*. Niayifar and Porté-Agel (2015) derived a relation between the freestream turbulence intensity and k_* based on LES data.

References

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