A Numerical Investigation of Multirotor Systems with Vortex-Generating Modes for Regenerative Wind Energy: Validation Against Experimental Data by Martins et al.

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The authors aim to provide a proof of concept of increasing the wake recovery of a multi-rotor system with vertical axis wind turbines using flow control devices, which is an interesting topic for the wind energy community. The authors perform Reynolds-averaged Navier-Stokes (RANS) simulations of a uniformly distributed thrust load in the form of a square, with and without additional vertical body forces, subjected to a uniform, laminar inflow. The simplified setup is validated qualitatively with wind tunnel PIV measurements. Based on the RANS simulations, the authors conclude that the wake recovery of the multi-rotor system can be enhanced by an order of magnitude. However, the authors do not provide sufficient evidence to conclude that the wake recovery of a utility scale multi-rotor system operating in atmospheric turbulence can be enhanced due to the chosen laminar inflow. Furthermore, the authors do not provide information of the power coefficient of the multi-rotor system with VAWTs nor wind farm simulation results, which may affect the author's conclusion of the potential increase in wind energy extraction per area of land. In addition, there are many labels and numbers missing in the plots, which makes it impossible for me to review the results. Therefore, I recommend a major revision. I have listed more detailed comments below.

Main comments

1. The authors have employed a uniform, laminar inflow for simplicity. While this is a logical first step, the obtained downstream wake recovery of the multi-rotor system is expected to be unrealistically slow and the effect of the additional atmospheric boundary layer (ABL) control devices may be exaggerated as a consequence. Figure 14 shows that the baseline case velocity deficit is about $1 - 0.2^{1/3} \approx 40\%$ at $12.5D$ downstream. (Note that I had to measure the numbers by a ruler due missing tick labels so I might have made a mistake.) This is extreme considering the employed thrust coefficient of 0.72, which translates to a maximum deficit of $1 - \sqrt{1 - 0.72} = 47\%$ following 1D momentum theory. In other words, the wake has only recovered by 7% at $12.5D$. Hence, the present results cannot be used to make conclusions about faster wake recovery for utility size multi-rotor systems operating in turbulence atmospheric conditions. Therefore, I would highly recommended to add additional numerical results including an atmospheric inflow, e.g. a neutral atmospheric surface layer with a homogeneous roughness length. I expect that such a setup will lead to a much smaller gain in wake recovery due to the effect of atmospheric turbulence, possibly by an order of magnitude. If additional simulations are not possible then the authors should modify the abstract and conclusion such that the reader cannot be tempted to translate the present results directly to the real world application. For example, the authors could add: *The present article employs a simple numerical setup to provide a proof of concept and the current results cannot yet be translated to real world applications. Additional research is required to study the wake recovery benefit in atmospheric inflow conditions.* The authors could also consider to choose a more clear title, for example: *A Proof of Concept of Multirotor Systems with Vortex-Generating Modes for Regenerative Wind Energy based on Numerical Simulation and*

Experimental Data Finally, the term *ABL control device* is misleading since the simulated inflow and wind tunnel experiment do not represent an ABL.

2. The authors conclude that the land usage could potentially be reduced due to the enhanced wake recovery. While this a logical extrapolation of the present simulation results, the actual benefit of the enhanced wake recovery in wind farms may be much smaller, following the reviewer's experience with wake recovery benefits of horizontal axis turbine multi-rotors in isolation and wind farms [van der Laan et al(2019), van der Laan and Abkar(2019)]. This is because the wake added turbulence typically reduces the benefit of wake recovery devices for downstream turbines, and there are many inflow cases where a wind farm does not experience significant wake effects (non-aligned inflow wind directions, above rated inflow wind speeds.) If the authors desire to make conclusions about reduced land usage, then additional wind farm simulations of multi-rotor systems are required, including a more realistic atmospheric inflow as discussed previously. Furthermore, information on the power coefficient of the multi-rotor VAWT systems are required in order to compare with the spacing of traditional wind farms containing horizontal axis wind turbines in terms of net power density.

3. Equation 4, how is $I_{u,\infty}$ defined? If it is based on the TKE, as Eq. (4) suggested, I would advice the authors the write $I_{k,\infty}$, as $I_{u,\infty}$ could be mistaken for the streamwise component, i.e. $I_{u,\infty} = \sigma_u/U$.

4. Section 2.1: The vertical axis turbines (VAWTs) are represented by a uniformly distributed source term in the form of a square. This choice could affect the conclusion of the article since the interaction of the VAWTs and VAWT generated turbulence are expected to influence the wake recovery. The authors have compared the simplified RANS setup with wind tunnel PIV measurements in Figs. 4 and 5, which provides a qualitative validation. The authors do not report a quantitative comparison and this makes it difficult to understand the errors in the simplified RANS model. I would strongly recommend the authors to add this. In addition, have the authors verified the simple uniformly distributed source term model with a numerical model where closely spaced VAWTs are represented by a high fidelity model, as for example an actuator line model? Such a comparison would provide useful information on the errors introduced by the simplified model.

5. There are many labels and numbers missing in the plots, which makes it impossible for me to review the results of Section 3 in detail.

Minor comments

1. Introduction, Line 26: *With the characteristic height of the ABL around one kilometer, wind farms covering a surface area of over 1020 km can approach the asymptotic limit of "infinite" wind farms.* Do the authors miss a "-" sign as in 10-20 km?

2. The authors depict the measured VAWT system by a diagonal white lines in Figs 4 and 5. This is confusing and the reader could think that the measurements actually represent a turbulence grid instead of VAWTs. The authors could add an explanation in the figure captions.

References

[van der Laan and Abkar(2019)] M. P. van der Laan and M. Abkar. Improved energy production of multi-rotor wind farms. *J. Phys.: Conf. Ser.*, 1256(012011):1–11, 2019. doi: 10.1088/1742-6596/1256/1/012011.

[van der Laan et al(2019)] M. P. van der Laan et al. Power curve and wake analyses of the vestas multi-rotor demonstrator. *Wind Energy Science*, 4:251–271, 2019. doi: 10.5194/wes-4-251-2019.