Second review comments for "Effect of tip spacing, thrust coefficient and turbine spacing in multi-rotor wind turbines and farms" by Ghaisas et al. in Wind Energy Science, 2019.

Thanks for your detailed response, and the added information, particular in the appendices. I only have a couple of follow-up comments.

1. Resolution and degree of detail.
- Thanks for the clarification in terms of grid points per rotor and the additional thoughts behind. However, I'm a little surprised that this has not been led to any changes in the article, but only explanations to me as a reviewer. I still believe this information should be stated explicitly in the article. Using established rule-of-thumbs are of course fine in general, but these rule-of-thumbs are for simulating single actuator discs. Actuator disc theory is based on 1D momentum theory, which comes with a number of assumption, e.g. the wake can expand freely afterwards. This is not the case in the multi-rotor. The combined induction effect of the multi-rotor is also different. My concern is essentially that some of the basic assumptions might be violated, hence general rule-of-thumbs are no longer valid. It is great that you have performed a grid convergence study. However, the difference in mean velocity(Figure 3) is not quantified, but it is discernible. If I zoom in and actually measure the difference for the red and blue grids in Fig. 3b) for x/D=6, I get an estimated difference of 5% in mean velocity, see attached sketch. Similar difference are seen in the other plots for the multi-rotor as well as the single rotor. If one simply assumes P ~ U^3, that would then correspond to a difference of 1.05^3 ~ 15% in power. This appears to be comparable to the differences shown in Figure 9 and larger than your estimated errors between LES and model(Fig. 15-16), as well as the numbers reported in Appendix B. In general, your article would actually benefit from quantifying the results a bit more for better comparison. So to sum up. I understand that it is not necessarily feasible to perform the entire study on a fully converged grid and that a 5% difference in the mean velocity might be acceptable, if one is comparing results from the same numerical setup. However, the setup changes here and as previously mentioned the change in tip spacing are often less than the grid size. I believe it is good practice to discuss the limitations and possible violations of fundamental assumptions, so I encourage you to include these considerations in your article. It does not take anything away from your otherwise interesting results, on the contrary. It shows a cautious approach and critical sense of scientific results.

- My previous comment was: "The authors state "It is seen that P2-5 is larger for all 4-rotor wind farms...". This is not correct. If you look at Figure 10(c) there is actually a cross-over for the 3rd turbine, where the single rotor produces more. Be careful, when you do the aggregate statistics, because it gets lost. Please rephrase."
=> New comment: I understand that the data is aggregated, and the statement itself is not wrong. But it is a little "dangerous" to simply aggregate and conclude that the power production is larger for all multi-rotors wind farms(line 299-300) when the power difference is occassionally negative, i.e. sometimes it is better to have a single rotor. If you included more turbines the advantage migth disappear all together.

Finally, I also wish to point the authors attention to a newly published paper, which examines many of the same things and have comparable findings.

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   title = {Multirotor wind turbine wakes},
   language = {eng},
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   journal = {Physics of Fluids},
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   doi = {10.1063/1.5097285},
   author = {Bastankhah, Majid and Abkar, Mahdi}
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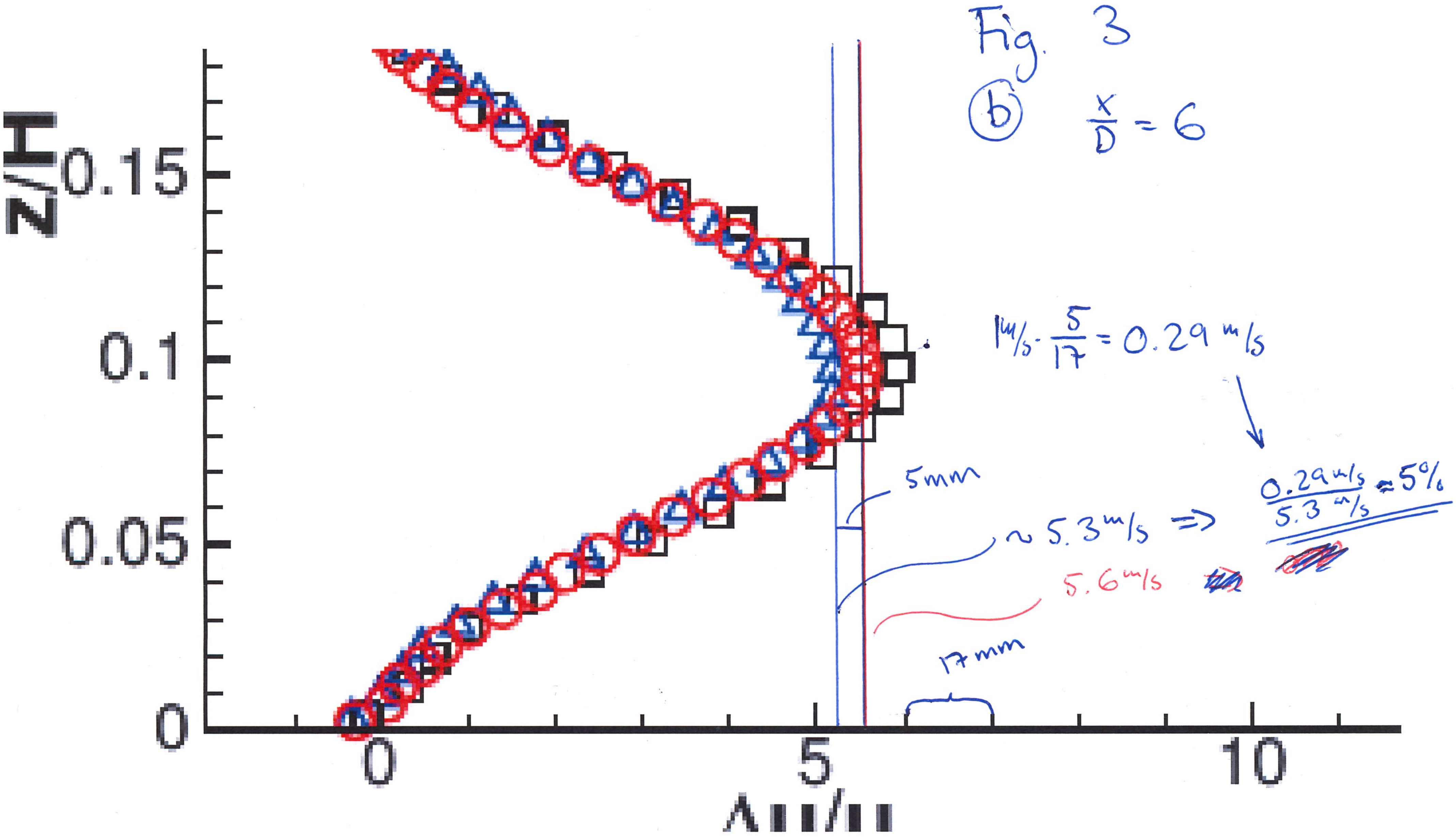
Fig. 3
b $\frac{x}{D} = 6$
Z/H
0.15
0.1
0.05
0
0
5
10
$1 m/s \cdot \frac{5}{17} = 0.29 m/s$
5mm
~ 5.3 m/s ⇒
5.6 m/s
$\frac{0.29 m/s}{5.3 m/s} \approx 5\%$
17mm