

Interactive comment on “Near wake analysis of actuator line method immersed in turbulent flow using large-eddy simulations” by Jörn Nathan et al.

Anonymous Referee #1

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General remarks:

This paper is of overall very good quality. A detailed discussion on the impact of the actuator line parameters is given. Even so no generic solutions are given, some metrics can be extracted from the work, regarding the optimal values of the Gaussian width parameter. Based on this validated actuator-line model, the authors propose a study on the impact of a homogeneous, isotropic turbulent inlet and shear layer turbulence. While the impact of the homogeneous isotropic turbulence is clear and well exposed thanks to the provided spectra, the additional impact of the shear layer is less obvious and should be discussed in more details. Specific comments: - P1 L7. Using a global

C1

performance data may hide some error compensation. Why not using local forces or a more physical basis for Epsilon, such as the local chord?

- P3 L9. Airfoil polars as a function of the Reynolds number are not given in Schepers et al. Please clarify.

- P3 L12. The root correction by Shen is mentioned, but supposed to have “known outcome”. Is it possible to clarify? Furthermore, other corrections have been proposed in the literature (see Snel, Chaviaropoulos, Bak, Dumitrescu. . .). Is it possible to include them?

- P3 L12. Coriolis / Centrifugal forcing enhance the lift of the airfoil near the root of the blade, not over the whole blade. It should be clearly stated here.

- P3 L12. It is disappointing to see that purely 2D airfoil polars are used, while 3D effects are discussed.

- P3 L16 → P4 L3. The authors argue the Glauert tip correction should be used due to the low resolution. According to Churchfield et al. (2017), this is due to the isotropic kernel that is used, and the virtual projection of forces outside of the “blade domain”. It could be interesting to see which phenomena is dominating, i.e. the lower resolution or the isotropic projection. Furthermore, I was not able to find reference (Nathan, 2018). Is it already published? Otherwise, please mention it in the references.

- P6 L7. The domain is rather small in length compared with standard recommendations. As a comparison, N. Troldborg (2009) uses a domain length of 18R, Martinez-Tossas (2015) use a domain length of 21D. . . It could be useful to provide some proof of convergence.

- P6 L25. If possible, provide some orders of magnitude for the time step.

- P7 L6. The under-prediction of the axial induction is not clear to me; results are almost super-imposed to the NewMexico measurements.

C2

- P8 L5 → L7. I do not understand the link with the actuator surface method. Even so epsilon over dx is adapted, depending on the Cartesian grid refinement, it is still an actuator line, and no chord-wise meshing is used.
- P8 L14 → 18. Discussion regarding the impact of the epsilon parameter is very instructive. Giving a look at the results, it seems to me there is an almost linear relation between the optimal epsilon value (leading to $T/T_{\text{ref}} = 1$) and the mesh refinement parameter N in the range $50 < N < 150$. It could be interesting to derive an empirical law from it. In case the results presented in Figure 6 are not “rotor specific”, this could lead to a very simple law to derive the value of epsilon “on the fly”.
- P9 L13→L16. The “bad” resolution near the tip is, according to the authors, attributed to the actuator-line representation of the blades. In Blondel et al. 2017, a lifting-line model is used together with a vortex model of the wake, and better correlation with experimental data was obtained (compared to SOWFA simulations). Thus, from my point of view, discrepancies should be attributed to the isotropic kernel in use, which is unrealistic near the tip, or the potential excessive diffusion of the finite volume scheme. The effect of the isotropic Gaussian kernel and the mesh effects, as discussed P10., should be further analyzed (not necessarily in this publication, as this is not the main topic).
- P12. In this part, synthetic turbulence is imposed at the inlet of the domain. I guess results presented in Figure 11 are based on simulations without the wind turbines, considering only the evolution of the TI in a channel. A net decrease of the turbulence intensity is observed along the channel. The simulations are not really described here. It could be interesting to provide some proof of convergence. Is the simulation time long enough to transport the characteristics defined at the inlet? This could be a basic explanation to the decrease of the TI that is observed. Also, it seems something is happening at the outlet, with some kind of sharp TI recovery. If this belongs to ghost cells, it should not be present in this plot. The effect of the ratio between the CFD grid and the SEM grid is discussed. One can wonder about the turbulent length scales used

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in the SEM algorithm, and their relation with the size of the computational grid. Is the CFD grid fine enough to catch the vortices given at the inlet?

- P14. Figure 15 is not useful and could be removed. Same remark holds for Figure 17, results are similar to the homogeneous isotropic turbulence case.
- P15. In 2.2.2., the evolution of mean velocity with height is presented. However, it seems to me, based on the contours of vorticity, that the TI is constant with height. Is that correct? From a physical point of view, higher TI is expected near the ground. This should lead to higher vorticity. Can the author provide some insight? In figure, y label should be z/R.
- P16. L7. I do not understand the point here. Please reformulate.
- P16. L11. Are the authors talking about the global rotor power? No metrics are given. The impact on vortex structures is not clear to me. Differences in the spectra between figure 19 and 14 are rather small. I would suggest including an additional metrics here to clarify the impact of the shear on the near wake.
- P17. Conclusions. Based on the observation of the turbulence decay (fig. 11) with axial position, it seems that at high TI, a large part of the TI is included in the subgrid scales. Therefore, I am not totally comfortable with the conclusions that are given: the length of the near-wake is determined based on the observation of the vorticity. However, the subgrid-scales are not included in the vorticity. Therefore, it seems difficult to draw definitive conclusions. As a more general remark, this work emphasize on the impact of the TI on the near-wake. However, blade loads may also be impacted, even at the airfoil polar level. One might expect a delay in the stall at high TI, which could impact the blade root loads. Also, it could have been interesting to use the NTNU Blind Comparison experiments as a complementary validation case.

Technical corrections:

- P1 L2. Noticable → Noticeable

C4

- P1 L3. This works uses → This work uses
- P1 L24. To to assess → To assess
- P2 L12. “As done the former approach”
- P2 L29. Change “as the ...” → With F representing... U the... (etc).
- P2 L32. Reformulate. “Finally, a summary is given...”
- P3 L8 Gaussian Kernel G → G the Gaussian Kernel
- P3 L8 Parenthesis: (f_{tip}) → f_{tip}
- P3 L10 This data → These (plural)
- P4 L14. Latin abbreviations should be in italic (e.g.)
- P4 L15. Acronym HIT has never been defined
- P8 L6. .This can... (missing space)
- P8 L10. “here chosen method” (reformulate)
- P8 L16. E.g. → *Italic*

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