Comments from the reviewer 1

The manuscript discusses the use of a digital twin to perform wind turbine and airfoil tests in wind tunnels with turbulent inflows. Hot-wire anemometry is first used to characterize grid-generated turbulence in the wind tunnel. Pressure and force sensors are then applied to record the pressure, drag and lift coefficients of an airfoil under different angles of attack at a chord-based Reynolds number of $2.0x10^5$. On the other hand, RANS simulations at the same Reynolds number are used to capture the kinetic energy decay of the grid-generated flows and the different coefficients within the airfoil. Good agreement is found between all quantities, provided that the Taylor microscale is used in the RANS simulations as the length scale required to simulate the grid-generated turbulent flow.
I find the thematic of this manuscript within the scope of the journal. Furthermore, it is well written and organized, with a theoretical discussion and numerical results that are of interest for the wind energy community. Nevertheless, before recommending publication the authors should assess the following points:
Response: We are grateful for the reviewer's remarks on our manuscript that helped to improve its quality. The issues they pointed out have been duly addressed, and corresponding adjustments have been made in the updated version of the manuscript.

Comment 1: Several arguments used to deduce equations 23 and 27 rely on the presence of a fully developed grid-generated turbulent flow, that is only found far downstream the grid. The range studied here ($x < 30$M, with the airfoil placed at $x \sim 20$M) may present some differences in terms of the approximations made in equations 10, 13 and 20. While the authors refer to a previous publication from the group, these points should be addressed in the present manuscript.

Response 1: The authors would like to thank the reviewer for mentioning this important point. In response we have added the following statement in the section 2.3 line 245

"The only assumption taken while deriving the equation (23) is statistical stationarity of fully developed GGT which is believed to start from $x/M \approx 20$ downstream of a grid (Comte-Bellot and Corrsin, 1966; Bailly and Comte-Bellot, 2015). Upstream of $x/M \approx 20$, one may expect some changes in the form of equations (10), (13), and (20) which are used to derive equation (23). However, between $x/M \approx 10$ and $x/M \approx 20$ the turbulent flow can be considered approximately developed (Frisch, 1995).Therefore, for all practical purposes, equation (23) is valid downstream of $x/M \approx 10$."

Following the reviewer's remark, we extended the application of equation (23) to additional data sets with experimental measurements exceeding $x > 30$M. This comparison are illustrated in figure 1. It can be clearly seen that equation (23) matches very well with the experimental data. Note that the deviations are over-accentuated when visualised in the log-log plot. More details on this comparison are also presented in the section 3.4. of the manuscript.

Comment 2: Related to my previous point, several papers discuss the decay of kinetic energy in terms of invariants (Sinhuber et al, PRL 2015; Krogstad and Davidson JFM 2009), and also the role of the integral length scale on such models. Furthermore, the power laws predicted contain a virtual origin that is not present in the theoretical discussion of the present work. First, these approaches should be mentioned at the section 'Brief theoretical description of decaying grid turbulence'. Second, they should be at least addressed when figure 4 is presented. Have

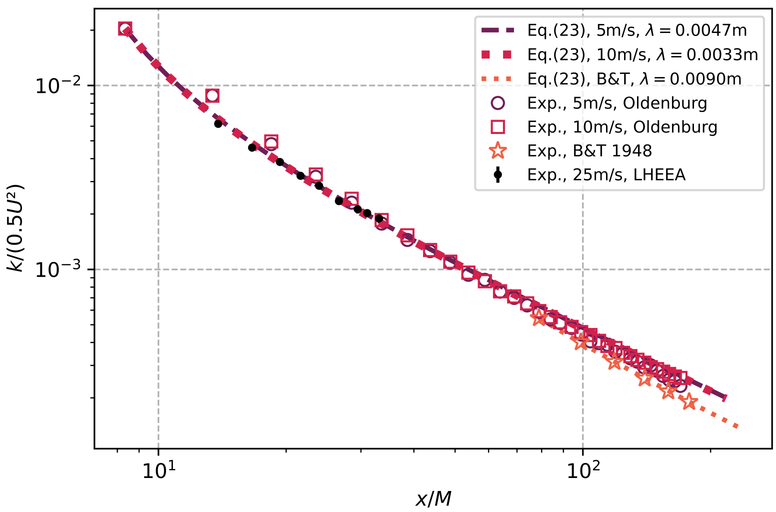

Figure 1: Comparison of experimental decay of normalised TKE decay obtained from experiments performed in the University of Olden- burg, and data from Batchelor and Townsend (1948) (B&T) with equation (23). For the application of equation (23) to the B&T data, we estimate $\lambda \approx 9$ mm from their manuscript.

the authors tried to compare their data with them? Are the decay exponents near the ones predicted in such papers?

Response 2: The authors would like to thank the reviewer for suggesting these interesting papers. In response to this question, we have added the following sentence in the section 'Brief theoretical description of the decaying turbulence' line 60:

"Krogstad and Davidson (2009) showed that grid turbulence is Saffman turbulence (Saffman, 1967). They improved the decay exponent of TKE from 1.2, which Saffman gave for perfectly homogeneous isotropic turbulence, to 1.1 for GGT. Sinhuber et al. (2015) performed experiments with one grid for different Reynolds numbers ($Re_M = \frac{UM}{\nu}$, where M is the grid mesh size, and U the mean velocity) and found that the decay exponent of TKE was equal to 1.18. They also showed that the decay exponent was independent of Re_M ."

On the reviewer's suggestion, we did compare our decay exponent with the decay exponents presented in the suggested papers and have added the following sentence in the section 3.4 line 345 where figure 4 is presented:

" The TKE's decay exponent was determined to be 1.087 in the equation (23) and simulation, while in the experimental data, it was observed to be 1.09. These values are in close proximity to the Saffman decay exponent of 1.1 for grid-generated turbulence, as reported by Krogstad and Davidson (2009)"

We have also made it explicitly clear that the equation (23) does not contain any fitting parameters including a virtual origin. We have added the following statement in section 2.3 line 242:

"The authors wish to emphasise to the readers that, unlike the equations commonly encountered in prior literature, such as those referenced in Comte-Bellot and Corrsin (1966), Kurian and Fransson(2009), Krogstad and Davidson (2009), or Sinhuber et al. (2015), equation (23) does not have any fitting parameter, and it is neither an empirical equation nor does it have any virtual origin."

Comment 3: Presenting figure 4 in logarithmic scale (or using an inset for that) would help to assess the quality of the power-law adjustment. Also, if I understand correctly, the theoretical curve has no fitting parameters? If that is the case, it should be made more explicit on the text as it is a relevant result.

Response 3: Figure 4 has been revised to include the plot also with a logarithmic scale. Indeed, the theoretical curve has no fitting parameters, which is now emphasized in the manuscript 2.3.

Comment 4: To properly address the relevance of the Taylor scale on the RANS models, a more systematic study with several grids (producing different values of integral and Taylor scales) should be performed. The present study is an interesting contribution pointing towards the relevance of the Taylor scale in RANS modelling, but gives no conclusive proof. I consider that the conclusions should emphasize this point.

Response 4: We see the reviewer's concern, and we would like to discuss how figure 1 shows the generality of equation (23) as it matches very well with different experimental data obtained for different grid sizes and inflow velocities. Equation (23) and the TKE decay equation derived within the $k-\omega$ framework, equation (27), are equivalent and give the same decay. It can therefore be understood that $k-\omega$ models will also give the same decay as equation (23) provided we use Taylor micro-scale as inlet length scale. Thus, this shows the generality of using the Taylor micro-scale as inlet length scale in RANS simulations to get the proper downstream TKE decay for GGT.

Comment 5: The accuracy of the numerical simulation in the estimation of different coefficients is discussed in terms of the experimental results but not compared with other numerical works/schemes. I suggest that the authors discuss other works from the literature when presenting figures 16 to 22. Also, those figures should have error bars added.

Response 5: As far as the authors are aware, there has been no previous numerical study conducted on this particular airfoil. It makes us unable to compare the obtained lift and drag coefficients with other numerical studies. The simulations presented in this paper were executed using the standard AVLSMART numerical scheme. In response to the reviewer's recommendation, we conducted an additional simulation at the 14° angle of attack (AoA) employing the BLENDED numerical scheme. However, we observed no disparity in the values of lift and drag coefficients. Error bars are now added to the figures

Comment 6: I also suggest that figures 11 to 15 are merged onto a single one.

Response 6: Following the reviewer's suggestion we have combined figures 11 to 15 into a single figure.