

# Comments from the reviewer 2

## 1 General comments

**Comment:** This manuscript is an interesting study on grid generated turbulence, its modelling, and its effect on aerodynamic forces in the flow around an airfoil. The topic is investigated using a combination of theoretical, computational, and experimental approaches, and the results and conclusions are of interest of the readership of Wind Energy Science and the fluid mechanics community in general. The recommendation of using the Taylor micro-scale to define RANS inlet boundary conditions is very useful for high-fidelity modelling in wind energy.

So, the work constitutes a contribution that deserves to be published, but there are many corrections and improvements that need to be made before. They are listed below.

**Response:** We thank the reviewer for their encouraging comments on our manuscript that helped improve its quality. The highlighted issues have been addressed, and the necessary changes have been incorporated into the revised manuscript.

## 2 Specific comments

**Comment 1:** Page 2, line 25: The manuscript says “Alternatively, experiments can be conducted in a wind tunnel by subjecting a Reynolds-scaled wind turbine rotor or a blade section from a real wind turbine blade to turbulent inflow under different inflow conditions, such as homogeneous inflow or gust inflow.” Wind turbine rotor models for wind tunnel tests are usually not scaled according to the Reynolds number, because in this case, the wind speed in the tunnel would have to be prohibitively high. They usually obey Strouhal similarity (i.e., keep the same tip speed ratio). Some sort of measure has to be taken or assumption has to be made to account for this Reynolds number discrepancy. Roughness elements can be added to the blades, or it the experiments can be run in a way that it guarantees that the flow will be fully turbulent at the rotor blades boundary layer (wind speed or turbulent kinetic energy high enough). For rotor blade sections tested in fixed conditions, as the case of the tests reported in this manuscript, the situation is different. So, I believe this sentence should be rewritten.

**Response 1:** Indeed, for wind turbines, the Strouhal similarity is used for scaling down. Based on the suggestion, we have revised the statement to: “Alternatively, experiments can be conducted in a wind tunnel by subjecting a Reynolds-scaled blade section from a real wind turbine blade to turbulent inflow under different inflow conditions, such as homogeneous inflow or gust inflow”.

**Comment 2:** Page 2, line 52: The manuscript says “The Navier-Stokes equations are highly nonlinear. . .”. The Navier-Stokes equations have just one nonlinear term, and it is a quadratic term. In my point of view, this does not make them “highly” nonlinear. I suggest the authors remove the term “highly”.

**Response 2:** We thank you for bringing this to our attention. We have removed the word “highly” and retained the term “nonlinear”.

**Comment 3:** Page 2, line 53: Why do the authors use GDT as acronym for grid-generated turbulence? Would not be more appropriate to use GGT?

**Response 3:** Certainly, opting for GGT instead of GDT is not only more suitable but also enhances clarity. Consequently, we have replaced GDT with GGT.

**Comment 4:** Page 3, line 58: The authors should mathematically define TKE and dissipation at this point.

**Response 4:** We have now provided the mathematical definition of TKE and dissipation on Page 3, line 58.

**Comment 5:** Page 5, section 2.1: I believe the manuscript would benefit from an explanation of the physical meaning/definition of the Taylor micro-scale at this point. The authors should also mention how it could be calculated or measured.

**Response 5:** The authors extend their appreciation to the reviewer for their input. Physically, Taylor micro-scale represents small eddies (Batchelor and Townsend, 1948). This physical meaning of Taylor micro-scale is written in the line 144-145 of section 2.1: “Batchelor and Townsend (1948) proposed to call the Reynolds number based on the Taylor micro-scale ( $\lambda$ ) the “Reynolds number of turbulence”, and also suggested that  $\lambda$  is representative of the eddies of large wave-number, i.e., small eddies, before viscosity becomes relevant.”.

Given that the mathematical definition and experimental methodology for calculating  $\lambda$  are presented in Section 3.3, the authors have incorporated the following sentence: “The mathematical definition and the experimental methodology for calculating  $\lambda$  are detailed in section 3.3.” in line 147 at page 5, section 2.1.

To improve the clarity the statement in line 328, page 13, section 3.3, the original statement “where  $\langle (\frac{\partial u}{\partial x})^2 \rangle$  can be estimated from the spectrum in the wave-number ( $\kappa$ ) domain” is now improved to “where  $\langle (\frac{\partial u}{\partial x})^2 \rangle$  can be determined from the spectrum in the wave-number ( $\kappa$ ) domain, derived through hot-wire measurements using Taylor’s hypothesis”

**Comment 6:** Pages 10-11, section 3.2: The authors should present the equations that are solved in the RANS simulations, and the values of the constants that were employed in the turbulence quantities transport equations.

**Response 6:** The section 3.2 now contains the presentation of the transport equation for URANS, as well as the transport equations for turbulent kinetic energy ( $k$ ) and specific dissipation or turbulence frequency ( $\omega$ ), including the associated constant values.

**Comment 7:** Pages 12-13, section 3.4: The authors must run the same validation test for other wind speeds and add the results to this section.

**Response 7:** For regular grids with a given grid mesh size  $M$  it is an established observation that the decay of normalized turbulent kinetic energy, expressed as  $k/U^2$  (in our specific case,  $k/0.5U^2$ ), when plotted against downstream distance normalized by the mesh size, denoted as  $x/M$ , remains independent of the inflow velocity  $U$ . This is very well documented in Batchelor and Townsend (1948) [1]. A relatively recent work from Sinhuber et al. PRL (2015) [3] also confirms this observation. Here the authors have performed experiments for Reynolds numbers ( $Re_m$ ) ranging from  $10^4$  to  $5 \times 10^6$  for a given grid mesh size. On the, reviewer’s recommendation, we performed simulations for  $30\text{ms}^{-1}$  and  $40\text{ms}^{-1}$  and we find the same evolution of the normalised decay of TKE for different inflow speeds (see

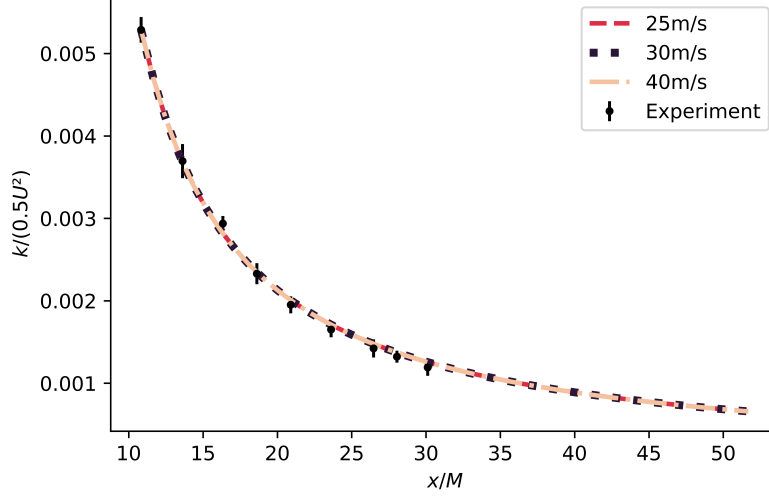


Figure 1: Normalised TKE decay for multiple velocities

figure 1).

Also, to perform an independent validation of equation (23), we tested it against hot-wire experiments performed in the wind tunnel at the University of Oldenburg, and data given in Batchelor and Townsend (1948). The experiments from Oldenburg were performed for a passive regular grid with  $M = 115$  mm for 33 downstream positions spanning from  $x/M \sim 8$  to  $x/M \sim 170$ . Two different inflow velocities,  $5 \text{ ms}^{-1}$  and  $10 \text{ ms}^{-1}$ , were used. Figure 2 shows the log-log plot of the comparison of the TKE decay obtained experimentally with that from equation (23). It can clearly be seen that the evolution of TKE given by equation (23) matches very well with the experimental data. Note that the deviations appear more pronounced when depicted in the log-log plot. We have included this supplementary validation in section 3.4.

**Comment 8:** Pages 17-19, section 4.2: The mathematical expressions of the boundary conditions must be presented (for velocity, pressure, and turbulence quantities). What was the initial condition used for the flow simulations? What were the wall functions employed at the top, bottom, and side walls? What were the value of  $y+$  on those walls?

**Response 8:** We thank the reviewer for pointing this out. Following the reviewer’s remark we have made following changes in the section 4.2:

We have added the following statement in response to the question on the inlet boundary condition and the inlet condition, also rephrasing the description of the values of  $y+$  to make it clearer:

“We have applied a Dirichlet boundary condition at the inlet, and the values are given in table 1. These values correspond to the values obtained at  $x/M = 11$  from the hot-wire measurement (see section 3.4). For pressure, we applied the Neumann boundary condition at the inlet,  $\frac{dp}{dn} = 0$ , where  $n$  is the normal vector to the inlet. These same values have been used as the initial conditions as well. In addition, we also use the integral length scale ( $L = 25$  mm) to investigate the impact of using the “wrong” length scale at the simulation domain inlet. At the outlet, the velocity is found using Rhie and Chow interpolation. We applied the Dirichlet boundary condition at the outlet for pressure  $p = p_o$ , where  $p_o = 0$  by default. For TKE and turbulence frequency, we applied the Neumann boundary condition as  $\frac{dk}{dn} = 0$  and  $\frac{d\omega}{dn} = 0$ , respectively. We have applied a no-slip boundary condition on the

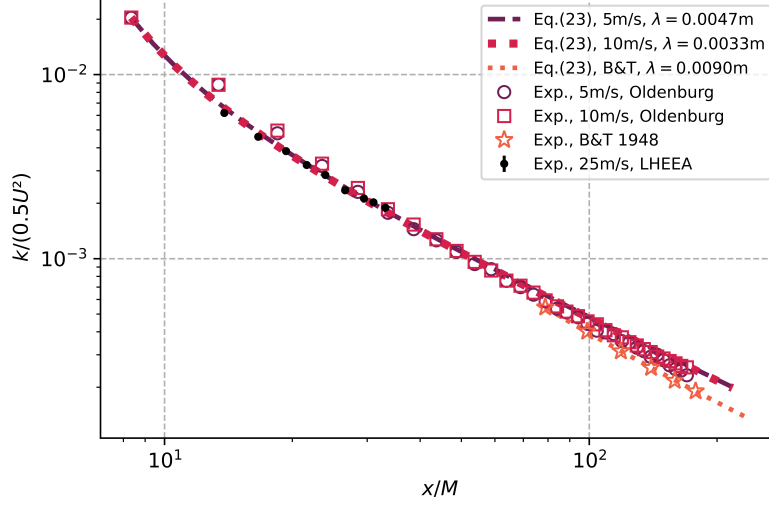


Figure 2: Comparison of experimental decay of normalised TKE decay obtained from experiments performed in the University of Oldenburg, and data from [1] (B&T) with equation (23). For the application of equation (23) to the B&T data, we estimate  $\lambda \approx 9$  mm from their manuscript.

airfoil, and imposed wall functions

$$\frac{\partial U}{\partial y} = \frac{\tau_s}{\kappa \rho c_\mu^{1/4} \sqrt{k_w} y_w}, \quad (1)$$

on the top wall (TW), bottom wall (BW), and side wall (SW) to avoid explicitly simulating the boundary layer. Here,  $U$  is the velocity,  $k_w$  is the TKE at the cell centre of the first cell from the wall,  $y_w$  is the perpendicular distance of the cell centre of the first cell from the wall,  $\tau_s$  is the wall shear stress,  $\kappa = 0.41$ , and  $c_\mu = 0.09$ . The  $y^+$  values for airfoil, BW, TW, and SW is given in the table 2.

Table 1: Boundary conditions at the simulation domain inlet. Note that the for the inlet length scale  $L_S$ , the Taylor micro-scale is used.

Variable	Value at inlet
$U$	$25 \text{ ms}^{-1}$
$k$	$1.859 \text{ m}^2\text{s}^{-2}$
$\omega$	$657.4 \text{ s}^{-1}$
$L_S$	$2.54 \text{ mm}$

Table 2:  $y^+$  values for the simulation.

Boundary	Applied $y^+$	Average $y^+$	$y^+$ Range
Airfoil	0.15	0.05	0.01 - 0.30
Top Wall (TW)	50	15	0.5 - 30
Bottom Wall (BW)	50	15	0.5 - 30
Side Wall (SW)	1	2	0.2 - 12

**Comment 9:** Page 21, line 400: The manuscript says “However, there is a slight tendency towards higher pressure on the pressure side, which could be attributed to difficulties in defining the reference pressure (which was measured inside the blade).”. What data shows this tendency? I guess it is the experimental set, but this should be stated clearly in the text. If the reason is difficul-

ties in defining the reference pressure, why does not that affect the other measurement points?

**Response 9:** The authors recognise that the statement is a bit misleading as we do not fully understand what is causing this difference between  $C_p$  curves obtained from our digital twin and experiments. However, we do expect that differences in the extraction of reference pressure and the dynamic pressure in simulations and experiments may lead to these differences. Further investigations are required to address factors that extend beyond the scope of this paper and cannot be solely explained by force coefficient measurements. We have replaced that statement with the following statement: “However, a slight tendency toward higher pressure on the pressure side is evident. The exact cause of this variation remains unknown; nonetheless, differences in extraction of reference pressure and dynamic pressure in simulations and experiments might contribute to these variations.”

**Comment 10:** Page 21, line 404: The sentence “Nevertheless, these differences are not significant for these local quantities.” is unclear. What are the authors trying to say?

**Response 10:** Following the changes in response to Comment 9 authors have removed this line from the paper.

**Comment 11:** Page 23, Figure 20: I believe the caption of the figure is wrong, because that graph probably refer to an angle of attack other than  $0^\circ$ , since results for that AoA are already shown in Figure 18.

**Response 11:** Thank you for pointing out this typo. The figure is associated to the AoA  $12^\circ$  and the caption of the figure is corrected

**Comment 12:** Page 23, line 414: “The discrepancy in  $C_l$  values between the 3d and 2d digital twins can be traced back to the escalating significance of 3d effects at and above  $14^\circ$  AoAs, which are unaccounted for in the 2d model.” What 3d effects are those? It is necessary to provide a clear physical explanation here.

**Response 12:** Experiments performed on this airfoil at high Reynolds number have shown that span-wise flow organisation is highly correlated in space and time at AoAs  $14^\circ$  and more, leading to bi-stability and affecting the position of flow separation(see, Neunaber et al. (2022) [2]). Since, by design, 2D simulations cannot simulate these 3D effects, we see differences in  $C_l$  between 2D and 3D.

We have updated the statement mention in the comment to the following: “The difference in  $C_l$  values between the 3D and 2D digital twins can be attributed to the growing importance of 3D effects, for example flow bi-stability, which impacts the position of flow separation at and above  $14^\circ$  AoAs in high Reynolds number experiments at the same airfoil in neunaber2022wind, something which cannot be reproduced in the 2D digital twin.”

### 3 Technical comments

- Page 2, line 30: Change “Veers et al. (2019)” to “(Veers et al., 2019)”. The same correction applies to line 90.
- Page 2, line 31: Change “millions” to “million”.
- Page 2, line 34: Change “a key” to “key” (in this sentence, key is an adjective, not a noun).
- Page 3, line 57: Change “(Kurian and Fransson (2009))” to “(Kurian and Fransson, 2009)”. This type of mistake happens in many other places of the manuscript (lines 27-28, 73, 74, 75, 91 ...), please perform a thorough check.

- Page 3, line 62: Change “computational fluid mechanics” to “computational fluid dynamics”.
- Page 3, line 79: Delete “have”.
- Page 4, line 97: Change “a perfect” to “an adequate”.
- Page 5, line 136: Delete “on”.
- Page 5, line 145: Change “This is confirmed” to “This was confirmed”.
- Page 8, line 210: Remove paragraph indentation.
- Page 11, line 273: Change “the ones” to “those”.
- Page 13, figure 4: Add “(23)” after “Equation” in the figure legend.
- Page 17, line 353: Change “airfoil’s cross section” to “model planform area”.

**Response:** The authors express their gratitude to the reviewer for providing technical corrections. All suggested amendments have been fully integrated into the updated manuscript.

## References

- [1] George Keith Batchelor and Albert Alan Townsend. Decay of isotropic turbulence in the initial period. *Proceedings of the Royal Society of London. Series A. Mathematical and Physical Sciences*, 193(1035):539–558, 1948.
- [2] Ingrid Neunaber, Frédéric Danbon, Antoine Soulier, Dimitri Voisin, Emmanuel Guilmineau, Philippe Delpech, Sébastien Courtine, Claire Taymans, and Caroline Braud. Wind tunnel study on natural instability of the normal force on a full-scale wind turbine blade section at reynolds number  $4.7 \cdot 10^6$ . *Wind Energy*, 25(8):1332–1342, 2022.
- [3] Michael Sinhuber, Eberhard Bodenschatz, and Gregory P Bewley. Decay of turbulence at high reynolds numbers. *Physical review letters*, 114(3):034501, 2015.