Response to the reviewer

We thank the reviewer #1 for their useful comments and the time invested in reviewing our manuscript. We have addressed each of the referee comments as detailed point by point below, which we believe has significantly improved the quality of the manuscript.

Reviewer 1

Main Comments

Reviewer Point P 1.1 — Koblitz et al. (2013) used a range of turbulence models that differ in complexity and it is not entirely clear which elements you have adopted. For example, do you use an active temperature equation or do you only use the global turbulence length scale limiter of Apsley and Castro (1997) [2]? Do you use ambient source terms to avoid zero turbulence values above the ABL (see for example van der Laan (2020))? For example, what are the chosen values of $G, f_c, \beta_B, \alpha_{t0}$, etc? What was the set inflow wind direction (at a certain reference height)? propose that you write down the full model description of the momentum equations including possible source terms, the Boussinesq hypothesis and the $k-\varepsilon$ (and ω) turbulence transport equations (also including all possible source terms). Then you can write in Table 2 which source terms are active by referring to the variable name ($S_?$). You could have a look at a recent article of my own where I tried to do this [6]. In addition, I strongly recommend to add a table including the chosen values of all turbulence model constants. Furthermore, not all parameters are defined.

Reply: We agree with the reviewer on this important point on clear explanation of the different elements adopted in the turbulence models. Table 3 has been added in the Methodology section of the manuscript detailing all the different turbulence model constants.

More specific details with regards to the buoyantBoussinesqsimpleFoam solver (BBSF) model, which uses a precursor developed inflow, is described as follows. For the BBSF model, Ambient source terms was added to avoid zero turbulence values above the ABL as mentioned in [6] and applied to the entire domain. The values were set to $k_{Amb} = 0.001$ and $\epsilon_{Amb} = 7.208e-08$ as summarized in Table 3 in the manuscript. The global turbulence length scale limiter of Apsley and Castro (1997) [2] is utilized and an active temperature equation is also solved.

The Coriolis force source term is included as a momentum source term (U and V momentum equations), this has been corrected in the manuscript in the Methodology Section 3.2. The Coriolis force is calculated based on the planetary rotational period ($\Omega = 24\ h$) and the latitude (λ) for Perdigao (39.68 $^\circ N$).

The inflow wind direction at the inlet at a reference height was set to calibrate the model at a reference Tower 20, at a height of 100 m above the ground which is 573 m above sea level. The inflow profiles are shown in response to Question 6 P 1.6.

The equations solved for the buoyantBoussinesqSimpleFoam solver are the continuity equation (Eqn. 1), momentum equation (Eqn. 2) and turbulence transport (Eqn. 4, 5), temperature (Eqn. 3) as described by Alletto *et al.* [1]. The pressure gradient (π_i) drives the momentum equation, the source terms for Coriolis and Canopy effects are included in momentum equation. The buoyancy terms are only added in the turbulence transport equations, but are set to zero for the present neutral case. The equation for turbulent dissipation rate contains the maximum length turbulence scale limiter (l_{max}) to modify the mixing-length scale estimations for setting different atmospheric stabilities. This description has been added to the Appendix of the manuscript.

$$\frac{\partial \rho \bar{u}_i}{\partial x_i} = 0 \tag{1}$$

$$\frac{\partial \rho \bar{u}_i}{\partial t} + \frac{\partial \rho \bar{u}_i \bar{u}_j}{\partial x_j} - \frac{\partial}{\partial x_j}\left(\rho\left(v + v_t\right)\left(\frac{\partial \bar{u}_i}{\partial x_j} + \frac{\partial \bar{u}_j}{\partial x_i}\right)\right) = -\frac{\partial \bar{p}}{\partial x_i} - \pi_i - \rho \epsilon_{ijk} f_j \bar{u}_k - \rho c_d \Sigma \bar{u}_i |\bar{u}| \tag{2}$$

$$\frac{\partial \rho \bar{\theta}}{\partial t} + \frac{\partial \rho \bar{u}_i \bar{\theta}}{\partial x_i} - \frac{\partial}{\partial x_i}\left(\rho\left(v + \frac{v_t}{\sigma_\theta}\right)\frac{\partial \bar{\theta}}{\partial x_i}\right) = S_\theta \tag{3}$$

$$\frac{\partial k}{\partial t} + \bar{u}_j \frac{\partial k}{\partial x_j} - \frac{\partial}{\partial x_j}\left(\left(v + \frac{v_t}{\sigma_k}\right)\frac{\partial k}{\partial x_j}\right) = P_k - \varepsilon + B \tag{4}$$

$$\begin{aligned}\frac{\partial \varepsilon}{\partial t} + \bar{u}_j \frac{\partial \varepsilon}{\partial x_j} - \frac{\partial}{\partial x_j}\left(\left(v + \frac{v_t}{\sigma_\varepsilon}\right)\frac{\partial \varepsilon}{\partial x_j}\right) = \left(C_{\varepsilon 1} + \left(C_{\varepsilon 2} - C_{\varepsilon 1}\right)\left(\frac{l}{l_{\max}}\right)^a\right)\frac{\varepsilon}{k}P - C_{\varepsilon 2}\frac{\varepsilon^2}{k} + \\ \left(\left(C_{\varepsilon 1} - C_{\varepsilon 2}\right)\alpha_B\right)\frac{\varepsilon}{k}B - \left(C_{\varepsilon 1} - C_{\varepsilon 2}\right)\frac{\varepsilon}{k}S_d\end{aligned} \tag{5}$$

Reviewer Point P 1.2 — You use sources of buoyancy in the BBSF1 model while you are only considering a neutral case. Are these sources then set to zero? If this is the case, wouldn't it make sense to remove them from the article and also remove the word buoyancy in the abstract and elsewhere, since you have not yet investigate its effect?

Reply: We agree with the reviewer on this point. Indeed the surface heating on the ground was set to zero, as the simulation period can be assumed as a near-neutral condition. We have instead renamed the models as a idealized (log-law) and a precursor inflow model. The word buoyancy has been removed from the article in the abstract and elsewhere, since we have not yet investigated its effect.

Reviewer Point P 1.3 — Are you aware that the global turbulence length scale limiter of Apsley and Castro (1997) [1] can have problems when it is applied to complex terrain where the turbulence length scales of the hills are in the order of the maximum value that is set by turbulence model? When this is case one could observe non-physical large hill wakes or even numerical convergence problems. I think this is worth mentioning in the article. (Unfortunately, I couldn't find a good reference for it other then a brief discussion in an article of my own [7].

Reply: Indeed, we encountered numerical convergence difficulties when using the global turbulence length scale limiter when applied to the present case for simulations over the complex terrain of Perdigão. This is especially true when setting the values of the maximum limiting length scale to low values. Here, the model is trying to restrict the turbulence scale, but physically the large length scales of turbulence are produced from hills or features that are in the order of the maximum value that is set by turbulence model. This has been mentioned in the article in Section 3 at lines 195-200.

Reviewer Point P 1.4 — Abstract, Line 10: I would rewrite the following sentence: The inclusion of a canopy model is shown to improve predictions close to the ground for most of the towers, while reducing prediction accuracy on top of the ridges, illustrating the need to represent terrain heterogeneity., because the second point is not in favor of representing terrain heterogeneity.

Reply: This sentence has been re-written in the Abstract, we thank the reviewer for the suggestion. The inclusion of a canopy model is shown to improve predictions close to the ground for the towers on the South-West ridge and inside the valley. However the model, performs poorly for predictions on the North-East ridge.

Reviewer Point P 1.5 — Section 3: You mention that a grid refinement study was performed, but I could not find any results in the article. Also note that a reference to the grid refinement study of Laginha Palma (2020) is not sufficient because they have used a different solver and setup. (A grid refinement study is solver dependent.) In addition, the chosen turbulence model might also influence the grid study; you could show results of a grid refinement study using the most demanding turbulence model.

Reply:

A mesh refinement study has been performed as shown in Table 1, by increasing the number of cells in the x and y direction thereby increasing the horizontal mesh resolution. The case has been simulated using the standard $k-\epsilon$ turbulence model with an idealized log-law inflow and no Coriolis source term.

Table 1: Grid refinement study

Case	Nx	Ny	Nz	NCells (million)
Coarse	227	227	120	12.7
Medium	332	332	120	21.19
Fine	469	469	120	30.6

Negligible differences are seen in the velocity and turbulent kinetic energy profiles on top of the ridge at the Tower 20 (Calibration tower) on the South West ridge seen in Fig 1 and at Tower 29 close to 100 m on the North East ridge seen in Fig 2. However, larger differences are seen close to the ground as the topology of terrain is further resolved near the surface upon grid refinement. The structured grids are generated using the `terrainBlockMesher` tool [11], which interpolates the SRTM terrain data and creates the terrain patch which is blended into a cylindrical domain. Increasing the number of cells refines and slightly modifies the surface mesh close to the ground. Similar differences are seen, when using different terrain databases in the grid refinement study by Palma *et al.* [9]. Furthermore, a small change in the prediction of extent of the re-circulation zone could have a significant change and uncertainty in the predictions inside the valley in Fig 3 on top of the North-East ridge.

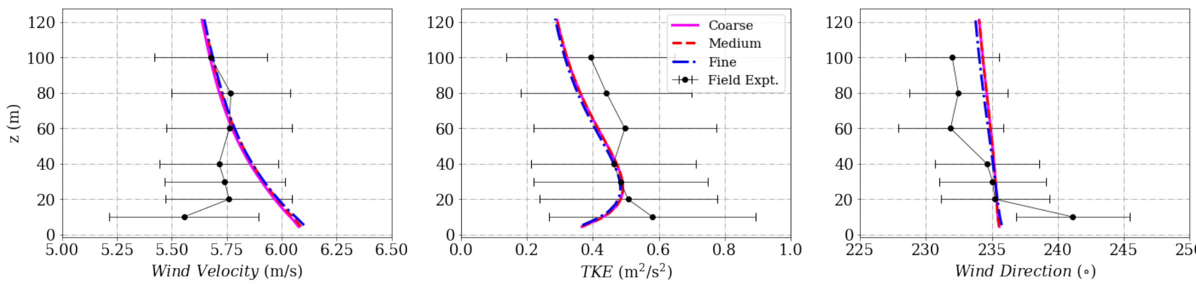

Figure 1: Profiles for a) Velocity Magnitude b) Turbulent kinetic energy c) Meteorological wind direction at Tower 20 (tse04) on the South West ridge, shown for different levels of grid refinement.

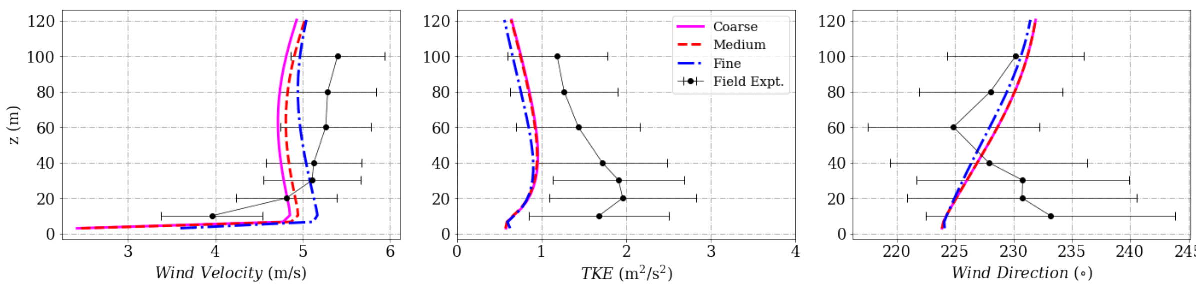

Figure 2: Profiles for a) Velocity Magnitude b) Turbulent kinetic energy c) Meteorological wind direction at Tower 29 (tse13) on the North East ridge, shown for different levels of grid refinement

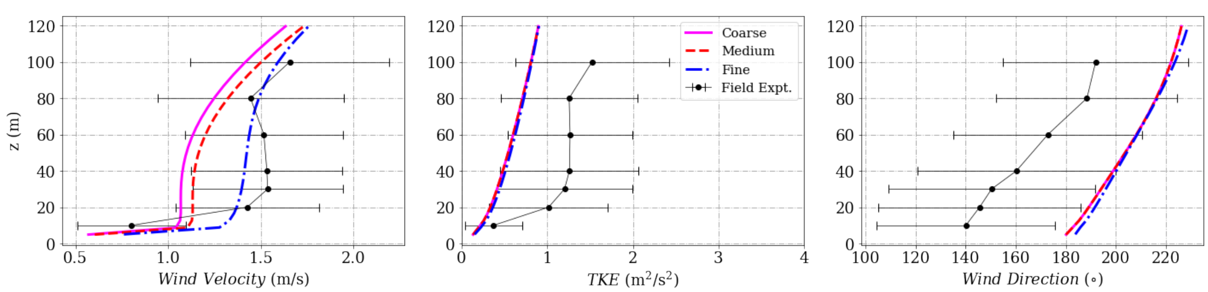

Figure 3: Profiles for a) Velocity Magnitude b) Turbulent kinetic energy c) Meteorological wind direction at Tower 25 (tse09) inside the valley, shown for different levels of grid refinement

Reviewer Point P 1.6 — You forgot to add results of the inflow (precursor). It would be useful to compare the two inflow models in a plot for wind wind speed, wind direction, TKE, turbulence length scale and temperature.

Reply: Thank you for your suggestion, and we agree that this information is useful for the readers. Therefore we have added figures of the inflow profiles for both the sets of inlet profiles used in the study in the Appendix.

Two sets of inflow profiles are utilized as shown in Fig 4- an idealized log-law profile for wind velocity, and a developed profile using a precursor driven by a pressure gradient. The inflow velocity profiles are calibrated to reach the desired inflow conditions at the met-mast Tower 20 at a height 100 m, for a time period identified as neutral based on the bulk Richardson number. The wind velocity magnitude profiles is close to logarithmic. For the wind direction, the idealized profile fixes a uniform wind direction, but the precursor has source term to account for the Coriolis effect, so a wind veer is seen over the entire height. Finally, for the TKE, a profile is set in the idealized case while for the precursor developed profile it is limited by the maximum mixing length scale, hence there is a decrease in turbulence levels over height.

The temperature plots are not shown as this is a neutral case with the source term for buoyancy not activated.

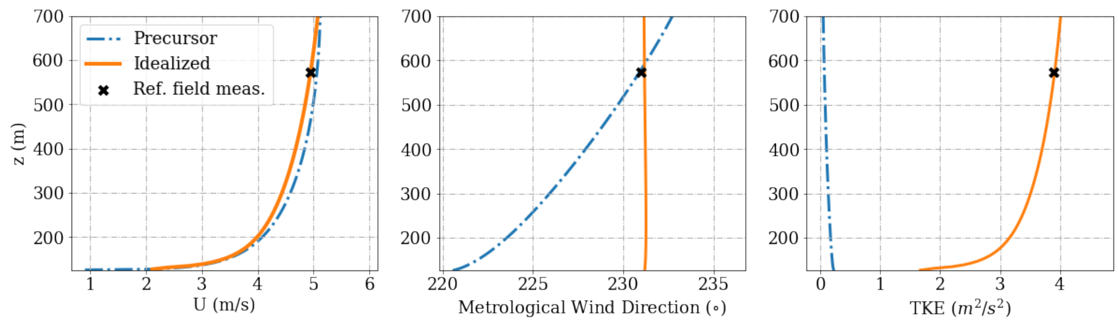

Figure 4: Idealized and Precursor (developed) input profiles for a) Wind velocity magnitude b) Wind direction c) Turbulent kinetic energy tuned to reach calibration at height 573 m corresponding to 100 m at Tower 20 (tse04)

Reviewer Point P 1.7 — Do you have an idea how much the wind direction varies in the observations? If this is significant you might need to account for it in the models by running a set of wind directions and then you can average the results (profiles) using a weighted averaged following a Gaussian distribution with a standard deviation representing wind direction uncertainty. A typical value for wind farm wake studies is 5° but it depends on the site and the distance between the location at which the reference wind direction was measured and the location at which the profile was measured. For more info you can have a look at Gaumond et al. (2013) [4] and a work of my own [8] (Section 3.1.3). For complex terrain, the effect of wind direction is often quite significant and a small difference in wind direction (distribution) between the measurements and models can result in large differences.

Reply: Yes, the instantaneous wind direction within the 30-min interval has a standard deviation of around 7 degrees as seen in Fig 5 (Figure 8 in the manuscript). This uncertainty is also dependent on location and height, for example there is a very high variance for Tower 34 close to the ground as expected for a complex terrain. We agree that simulating a set of wind directions measured within the time-period and then performing some statistics on the data would likely provide a more accurate representation of the conditions in the period. However, we have decided not to do this as this is not the scope of our article. Our scope is to study the effect of the different source terms on the wind simulations for only one inlet wind direction (with or without veer), which represents the conditions in a 30-min interval. This highlight limitations in the models' ability to represent actual conditions for such a complex site. Therefore, comments in the discussion part have been added in the Results.

There have been studies performed on the effect of inflow wind direction [4, 8]. . As a part of future work, an uncertainty quantification using the tool DAKOTA similar to the strategy adopted by Garcia-Sanchez *et al.* [3] can be performed, with an input matrix of wind directions and wind velocity uncertainty intervals. The range of inflow variability accounted for, needs to be less than the variability in the field measurements, an inherent variability in the inflow wind direction is already included in the turbulence model as shown by Vervecken *et al.* [12].

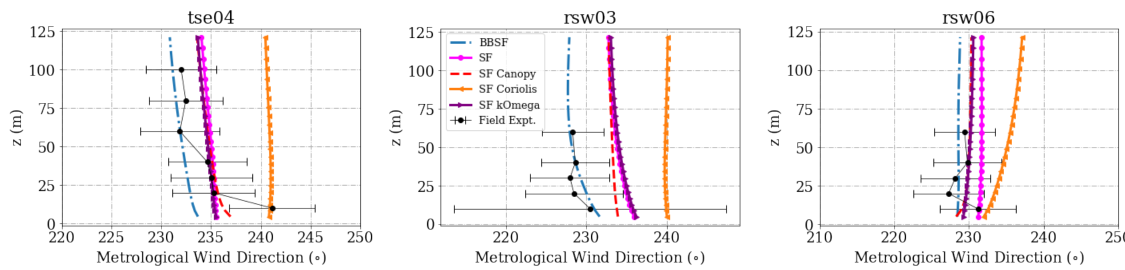

Figure 5: Simulation results and experimental data for wind direction on the South-West ridge for a) tower 20/ tse04 b) Tower rsw03 c) Tower rsw06

Reviewer Point P 1.8 — Line 115: You mention that you use a log-law inflow with TKE profile that varies with height following a modification of Yang et al. (2009). If you want a varying TKE profile with height then you could just model a pressure-driven boundary layer with a constant pressure gradient, which should result in logarithmic wind speed profile near ground and a varying TKE profile. Such a model would require a precursor simulation to generate the inflow. In addition, I am not convinced that the model of Yang et al. (2009) is a solution of the standard k - ε model, meaning that the inflow will most likely develop downstream (especially of the domain is large). Have you checked this?

Reply:

The use of both the Idealized (log-law) inflow and a precursor inflow model has been done. The inflow profile does develop downstream, however since the domain is cylindrical with a smoothing region applied and since it it is not flat, we indeed expect the profile to develop and have a speed-up. Hence we calibrate the inflow by trial and error to reach the expected velocity magnitude and direction at the Tower 20, at a height of 100 m above the ground level. This is indeed the motivation to utilize a precursor developed inflow using a pressure-driven boundary layer and Coriolis forces. Weerasuriya *et al.* [13] showed that the wind veer (twist) of inflow profiles plays an important role in wind speed predictions for urban areas.

Reviewer Point P 1.9 — Line 134: You write The direction of the flux automatically determines the inlet and outlet regions. I thought that the set wind direction would determine the inlet and outlet regions, or this is a misunderstanding from my side? How do you handle wind veer for determining the inlet and outlet boundaries?

Reply: Yes, the direction of the mass flux, either from precursor or idealized profiles, determines whether a cell on the side boundary is set to be an inlet or an outlet [5]. This gives the same classification as to whether the inlet wind direction points inward or outward of the domain for most cases. However, for some theoretical cases where the simulated flow is flowing back out of the cell, this cell is redefined as an outlet. This is mainly avoided by choosing such an extensive domain with a smoothed outermost part of the terrain patch.

For the simulations using profiles obtained through the precursor, there will be wind veer such that not all points along a vertical straight line on the side patch are defined to be the same boundary type. This method has been validated on the OpenFOAM tutorials [10] that have been verified and validated for atmospheric boundary layer modelling on which we base our simulations.

Reviewer Point P 1.10 — Section 3.2, Line 144 : What is the reason that you use a constant leaf area density? You could easily use a varying leaf area density based on the forest point cloud data.

Reply: The objective of this study was to study the influence of canopy, and to test if the use of a canopy model could improve predictions close to the ground. Simulations are planned with the use of a varying leaf area density based on the forest point cloud data as a next step. Indeed a more realistic representation of the forest, using different patches with different heights and removing the trees from the higher zones of the ridges could improve the results.

Reviewer Point P 1.11 — Section 3.2, eq. (3): Is this really how the Coriolis force source term is implemented in your model? If you follow the ABL model of Koblitz et al. (2013) I would expect that you include U and V-momentum source terms that represent a balance between a (constant) geostrophic wind speed and the Coriolis force (see for example van der Laan (2020)), as you also briefly discuss.

Reply:

As explained in Question 1, the Coriolis force source term is included as a momentum source term (U and V momentum equations), this has been corrected in the manuscript in the Methodology Section 3.2. The Coriolis force is calculated based on the planetary rotational period ($\Omega = 24\ h$) and the latitude (λ) for Perdigao (39.68 $^{\circ}N$), where $f_c = 2\Omega sin(\lambda)$. Exactly similar to Koblitz et. al, the Coriolis force in vertical direction is neglected since it is small compared with the gravitational acceleration.

Reviewer Point P 1.12 — Section 3.2, eqns (2) and (4) : I think you need to define different source terms, for example $S_{p,m}, S_{p,k}$ and $S_{p,\varepsilon}$.

Reply: We thank the reviewer for the suggestion, $S_{p,m}$ is defined as the source term for the momentum equation, $S_{p,k}$ is defined as source term for turbulent kinetic energy and $S_{p,\varepsilon}$ is defined as source term for turbulent diffusion. This description has been removed, as these terms are not activated for the neutral case.

Reviewer Point P 1.13 — Table 2: The skipped entries (−""−) are unclear to me. For example, does the BBSF1 case include forest source terms or not? I would just fill in the entire table for clarity. In addition, what do you mean by using SKE for BBSF1? I though that you use a global length scale limiter in the epsilon equation, which is different from a standard model.

Reply:

Table 2, showing the different simulation case has been updated with all columns clearly filled. The BBSF model does not contain the canopy source term. The turbulence constants for the different models are further listed in a separate Table 3 in manuscript as shown below. Indeed, we use a global length scale limiter in the epsilon equation renamed as KE-Lim model.

Table 2: List of simulation cases simulating a period of neutral atmosphere, 22:00-22:30 (04.05.17).

Case name	**Solver**	**Inlet profiles**	**Source terms**				**Turbulence mod.**
			Canopy	Coriolis	Pres. gr.	Buoy. for.	
SF1	SF	Idealized	No	No	No	No	SKE
SF2	SF	Idealized	No	No	No	No	KO
SF3	SF	Idealized	Yes	No	No	No	MKE
SF4	SF	Idealized	No	Yes	No	No	SKE
BBSF	BBSF	Precursor	No	Yes	Yes	Yes	KE-Lim

Table 3: Turbulence constants for different turbulence models

Coefficient	**Turbulence model**			
	SKE	MKE	KO	KE-Lim
C_μ	0.09	0.033	0.09	0.09
C1	1.44	1.44	-	1.44
C2	1.92	1.92	-	1.92
σ_ϵ	1.30	1.85	-	1.30
σ_K	1.0	-	0.5	-
α_K	-		0.5	-
α_ω	-	-	0.6	-
$\beta*$	0.09	-	-	0.09
β	-	-	0.072	-
ν	1.5e-05	1.5e-05	1.5e-05	1.5e-05
L_{max}	-	-	-	62.14
k_{Amb}	-	-	-	0.001
ϵ_{Amb}	-	-	-	$7.208e-08$
T_{Ref}	-	-	-	300
Pr	-	-	-	0.9
Prt	-	-	-	0.74

Reviewer Point P 1.14 — What do the error bars on the measurements represent? Is is the standard deviation of the uncertainty of the mean? I would recommend to use the latter.

Reply: Yes, the error bars represent one standard deviation of the mean measurements. This description has been updated in the manuscript in the introduction of Section 4 (Results). **Re-**

viewer Point P 1.15 — You could group the profile plots in the results into three figures, where each row of sub plots represents a mast: SW ridge (combine Figs. 6 to 8), valley (combine Figs. 9 to 12), NE ridge (combine Figs. 13,14). I think this make sense because you also discuss them as met mast groups in the text. In addition, you could consider to plot normalized results of wind speed U/U_{ref}, turbulence intensity $\left(\sqrt{2/3k}/U_{ref}\right)$ and wind direction $(wd - wd_{ref})$ instead of dimensional results. Finally, you could zoom the x-axis of the wind direction, since it is hard to see the difference between the models and measurements in Figs. 6c, 7c, 8c and 14c. This also applies to some of the wind speed and TKE plots.

Reply: We thank the reviewer for the suggestion. We have regrouped the profile plots in Figures 6-18 in terms of met-mast. We have also zoomed in the figures to clearly see the difference between the models and measurements.

Reviewer Point P 1.16 — Line 194: You mention: A good match is obtained in between the measured and computed velocity, turbulent kinetic energy and wind direction profiles for the calibration Tower 20 as seen in Fig. 6. However, the results of the model including forest are not matching well, especially for the TKE and the wind speed near the ground.

Reply: We thank the reviewer for spotting this oversight. The sentence has been re-written in Section 4.1 of the manuscript.

Reviewer Point P 1.17 — Figure 13: Nice plot. You could add the relevant mast location(s) in the plot, so the reader can better understand the results of Figs. 9-12.

Reply: We thank the reviewer for the suggestion. The nearest mast locations are now shown in Figure 15. Also, the locations of all the used met masts are presented in Figure 2. And a reference to this figure is added to the figure captions.

Reviewer Point P 1.18 — I am missing information on code and data availability, which is normally added at the end of the article. In addition, I was wondering if is possible to provide the numerical setup using a DOI of a git hub repository through Zenodo or something similar. By proving the numerical setup/ run scripts, one could easily redo the work since the numerical solver (OpenFOAM) is publicly available.

Reply: A community has been setup in Zenodo (https://zenodo.org/communities/zephyr/). A repository of the numerical setup shall be added through Github (https://github.com/kartikv95/WESC-Perdigao).

Minor

Reviewer Point P 1.19 — Introduction, Line 13: You could rewrite the following Lack of terrain availability in flat terrain pushes wind-farm developers to look for alternative sites along complex terrains., since you use the word terrain three times and I think complex terrains could be rewritten as complex terrain sites. The latter also applies elsewhere in the paper.

Reply: This has been modified. Thank you for the suggestion.

Reviewer Point P 1.20 — Line 117: log-low → log-law.

Reply: This has been fixed. Thank you for the suggestion.

Reviewer Point P 1.21 — There are quite a lot of other typos in the article but these can be fixed in the proof reading process.

Reply: The article has been proof-read. Thank you for the suggestion.

References

[1] Michael Alletto et al. "E-Wind: Steady state CFD approach for stratified flows used for site assessment at Enercon". In: *Journal of Physics: Conference Series* 1037 (June 2018), p. 072020. DOI: 10.1088/1742-6596/1037/7/072020.

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[12] Lieven Vervecken, Johan Camps, and Johan Meyers. "Accounting for wind-direction fluctuations in Reynolds-averaged simulation of near-range atmospheric dispersion". In: *Atmospheric Environment* 72 (2013), pp. 142–150.

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