Please note that references to line numbers are for the revised manuscript.

Scientific questions:

1. Can you comment on how representative the specific case studies (stable in combination with mountain waves, unstable in combination with a recirculation zone) are for the wind conditions at Perdigão and for complex terrain in general? Do you think the conclusions in terms of wind-turbine wake behavior (terrain following in stable, deflecting upwards due to recirculation) will hold for all stable/unstable wind conditions at this site and for complex terrain in general?

Mountain waves occurred for almost 50% of the nights during the intensive observation period (Fernando et al. 2018). For recirculation, Menke et al. (2019) found that reverse flow with wind speeds greater than 0.5 m/s occurred over 50% of the time when the wind direction was perpendicular to the ridges. We have added these details into the manuscript at lines 130-132.

We expect that the conclusions in terms of the wind turbine wake behavior would hold for most convective and stable atmospheric conditions at the Perdigão site as long as the phenomena of interest (recirculation zones and mountain waves) are present. Other phenomena could be modeled using WRF-LES-GAD to examine wind turbine wake behavior in other cases. With regards to other sites, the wind turbine wake behavior would depend largely on the vegetation (surface roughness) and steepness of the terrain. If the previously mentioned parameters are similar to those in the examined case studies, similar conclusions for wake behavior in convective and stable conditions could be made. We have added this discussion to the manuscript in lines 514-518.

2. The inverse of the Froude number as defined in Equation 1 is often called the non-dimensional mountain height and it represents the ratio of the mountain height to the (vertical) wave length of the mountain wave. When discussing the case where the mountain wave length is shorter than the width of the mountain (line 133-134), wouldn't it be more appropriate to use a Froude number based on the width rather than the height of the mountain like for example in the book of Stull (1988, section 14.2.3 Flow over hills). Obviously, this also affects figure 3 and later calculations of the Froude number in section 4.1.

We thank the reviewer for bringing up this interesting point. Since we are dealing mostly with flow over the mountain rather than flow around the mountain, we believe the Froude number based on the mountain height is more appropriate. Additionally, the Froude number defined using the mountain width is for an isolated hill as opposed to an extended ridge as is the case for Perdigão.

The Froude number based on the ridge height has also been calculated for previous studies at Perdigão (Fernando et al. 2019, Palma et al. 2019).

The wavelength of a mountain wave is defined as lambda = 2*pi*U/N and is actually independent of both the mountain height and width. However, we can quickly calculate a Froude number based on width during the stable case. If the mountain width is 800 m, N is 0.035 /s, and U is 9.8 m/s, then Fr = 3.14*9.8/(0.035*800) = 1.1. Because of the proportion of the height and width of the ridge at Perdigão, calculations of the Froude number using either the mountain height or width are very similar.

The discussion comparing the wavelength to the mountain width has been removed to avoid any confusion. The sentence, lines 139-140, now reads: "For small Froude numbers (< 1), when wind speeds are low or the stability is very strong, the wavelength of the mountain wave is short resulting in weak mountain waves."

3. It is not entirely clear how you setup the semi-idealized simulations. What pressure gradient force do you impose, or alternatively what wind speed and direction do you enforce (and at what height)? Do you apply a negative surface heat flux in the stable case? What is the domain height of these simulations? Do you use any damping layers at the top? For how long did you simulate these idealized cases?

The semi-idealized simulations are set up using geostrophic forcing with a specified initial wind and potential temperature profile. The wind speed for the stable case comes from a sounding, while for the unstable case it is uniformly specified as 7 m/s. In both cases, the geostrophic wind direction is aligned with the x-axis, such that the forcing is entirely longitudinal. For the stable case, a cooling rate of -0.25 K/hr was used. However, the effect of this cooling rate is minimal as the simulation is only run for 10 minutes. The domain height is just above 1400 m and we use Rayleigh damping within 500 m of the domain top with a coefficient of 0.003 /s. These details have been incorporated into the semi-idealized modeling section in lines 198-209.

4. Line 182-183: What do you mean with the stratification is self-destructive? Do you mean the stable stratification turns into a constant temperature profile because of turbulent mixing? Does the simulation become unstable due to inertial oscillations? Something else?

Because the idealized simulation uses periodic boundary conditions, the flow will recirculate and the effects of the mountains and the GAD will induce mixing which will erode the stable stratification. We have edited the manuscript to replace the term “self-destructive” with “eroded by turbulent mixing” in line 207.

5. Figure 12: Is there any averaging of model results or measurements? Under higher turbulent conditions, does it make sense to compare instantaneous velocity fields with the point measurements of the met towers given the chaotic nature of turbulence?

There is spatial averaging of 30 m in each spanwise direction (60 m total) in figure 12 but no time averaging. While we agree that exact agreement between the model and observations is not expected under highly turbulent conditions, we believe that this figure shows important qualitative agreement. The four transects included aim to be representative of the hour-long simulation period, highlighting that the general dynamics of the flow are captured despite differences due to the chaotic nature of turbulence.

6. Line 361 “Differences in 80m mean wind speeds … are less than 1 m/s …”: Is this the difference averaged over the entire time horizon, or the root-mean square, or something else? When I look at figure 13b around 13:55, I obviously see differences of more than 1m/s. Similar for the discussion of the differences in the wind direction and also for the plots for stable conditions. You seem to be talking about a certain average difference being below a certain value, but the instantaneous differences are clearly higher than this value and it is not clear how the averaging is done. Please clarify.

The reviewer is correct in that instantaneous differences can be large and this is due to the previously noted chaotic nature of turbulence. This is why we have included both bias and RMSE as metrics comparing WRF-LES-GAD with observations. The RMSE captures the cumulative magnitude of the errors over the entire simulation period while bias captures the average behavior. To clarify the text, the discussion regarding mean metrics has been removed. In line 392, the variability quantified in terms of standard deviation now explicitly states that the standard deviation is calculated over the hour-long period. Additionally, errors displayed in Table 4 are discussed in the sentences that follow in the manuscript (lines 394-399).

7. Line 445: The blockage addressed in the papers by Meyer-Forsting, Bleeg and Segalini refers to the global blockage effect far upstream of large wind farms that arises due to the combination of many wind turbines. Here, you only have one turbine and you are only looking at

> 2D upstream, so I would say that turbine induction might be playing a role, not blockage. I don't think it is appropriate to refer to these wind-farm blockage studies here, so consider removing these references.

We have removed the blockage references and added two references that provide more detail on wind turbine induction zones.

Medici, D., Ivanell, S., Dahlberg, J.-A., and Alfredsson, P. H.: The upstream flow of a wind turbine: blockage effect, Wind Energy, 14, 691–697, https://doi.org/https://doi.org/10.1002/we.451, 2011.

Simley, E., Angelou, N., Mikkelsen, T., Sjöholm, M., Mann, J., and Pao, L. Y.: Characterization of wind velocities in the upstream induction zone of a wind turbine using scanning continuous-wave lidars, Journal of Renewable and Sustainable Energy, 8, 013 301, https://doi.org/10.1063/1.4940025, 2016.

Minor questions and technical comments:

> Line 148: Please mention at least once how UTC relates to local time at this particular site. Maybe it is even more interesting to mention what time corresponds to sunrise.

The local time has been added in sections 2.2.1 and 2.2.2 at lines 167 and 177, respectively. In section 2.2.1, the local time corresponding to sunrise has also been added in lines 167-168.

> Line 148-149: Please mention what value of the wind direction corresponds to a direction perpendicular to the ridge such that the statement can actually be assessed by the reader.

A southwesterly wind of 215 deg. corresponds to being perpendicular to the ridges. This has been added in line 169.

> Line 149 reference to Fig 2a and 2b: I don't think this reference is correct. Figures 2a and b show wind speed in stable and convective conditions, while the sentence was talking about the wind direction.

The reference should be just Fig. 2c which refers to the wind direction during the stable case. This has been fixed.

> Line 155-156 and figure 2d: Wouldn't it make more sense here to use an actual stability parameter like the Obukhov length L or z/L to assess the atmospheric stability? Especially when talking about moderately convective conditions, how can you assess that based solely on the potential temperature gradient?

Assessing atmospheric stability in complex terrain is an active research area. Using a gradient Richardson number is difficult because of the effect of terrain-induced flow-speedup over the ridge affecting the shear terms as mentioned by Menke et al. (2019). Additionally, Bodini et al. (2020) calculated the Obukhov length at Perdigão and found that it was not very powerful in predicting dissipation rate. However, we have calculated the Obukhov length and added it to Fig. 2 along with new discussion regarding its limited applicability for assessing atmospheric stability in complex terrain (lines 147-157). Additionally, we have removed "moderately" from this section since our primary intention is to say that the case is convective, rather than stable.

> Line 158 reference to Fig 2d: Again this reference seems to be a bit misplaced as it refers to the wind direction plot, not the wind speed plot.

The reference should be Fig 2b and this has been fixed.

> Fig2: Please mention hub height of 80m (?) and measurement points for the temperature gradient (10m and 100m?) in the caption for clarity.

This has been added in the caption of Fig. 2.

> End of line 188: surface roughness length should be in units of meters.

Units have been added.

> Line 291, 356, and many others : You often start a new paragraph without clearly indicating that you will be talking about a new figure. For instance on line 291, are you still referring to figure 7, or are you rather referring to figure 8 or table 2?

In lines 292, 318, 333, 357, 377, 387, and 412, we have added explicit references to figures to improve readability.

> Table 2 and 3: the results in these tables are not really discussed it seems. Can you somehow use these results when assessing/discussing the results of related figures?

For Table 2, we have added language talking about the RMSE of the temperature gradient in line 331. This is in addition to the specific numbers already mentioned and discussed in the paragraph from lines 324-332.

Specific numbers from Table 3 have been weaved into the paragraph from lines 338-356, where they can be related to figures.

> Line 312: How high above the actual surface is the nose of the jet which you use to calculate the Froude number? The Brunt-Vaisala frequency is based on the temperature gradient between 10m and 100m, but it is not clear whether the nose of the jet is at about the same height above the surface.

The nose of the jet is at 650 m a.s.l. in the model and 720 m a.s.l. in the measurements (which is 350 m and 420 m above the valley floor, respectively). This has been specified in the text at line 340 to avoid any confusion. The sentence has been rewritten as: "Using the velocity at the nose of the jet, which is at 650 m a.s.l. In the model and 720 m a.s.l in the observations, we can calculate a Froude number at the beginning of the period of interest."

> Line 320: How high is the inversion layer above the valley floor (600m asl but at what altitude is the valley floor)?

The valley floor is just under 300 m a.s.l. This has been added to the manuscript in lines 357-358:

"The small negative bias of -0.3 K for potential temperature is also apparent for the second sounding, but the height of temperature inversion is accurately captured close to 600 m a.s.l or 300 m above the valley floor."

> Line 323: When talking about the striation of lower wind speed close to 600 asl in fig 6b and d, in what ranges of x can this be seen?

There is a coherent striation from the beginning of the transect until the rotor in the lee of the second ridge at x = 2500 m. This has been added to the manuscript in lines 351-352:

"This decrease in wind speed could be the turbine wake or the striation of lower wind speed seen close to 600 m a.s.l in the multi-Doppler lidar scans from the beginning of the transect until the rotor in the lee of the second ridge at X = 2500 m (Fig. 6(b) and Fig. 6(d))."

> Line 357 "there is less variability in the potential temperature gradient for the model compared with observations": I'd say there is more variability in the model output compared with the observations (this is also mentioned later in the text). The statement seems to contradict the results shown in figure 13 g,h,i.

This should read "there is more variability…" which we have corrected in the manuscript at line 387.

> Line 427: The year of the publication by Churchfield and Sirnivas is missing (also in the list of references).

This has been fixed.

We thank the reviewer for the comments. We think that the revised manuscript and the responses above help to address most of these concerns.

Please note that references to line numbers are for the revised manuscript.

Reviewer general comment: The current manuscript presents results of a suite of WRF-LES configurations around the complex topography of the Perdigão field site. Results represent the first time that WRF- LES is used to study the flow interaction in complex terrain and wind turbines. Results of the numerical simulations are qualitatively compared to the experimental measurements. According to the authors, the features of interest for the present work are: (a) mountain waves, (b) recirculation zones, and how these interact with a wind turbine and the corresponding wake. The manuscript is very nicely written, clear, and with superb "candy-to-the-eye" type figures. Therefore one could say that the article is excellent (despite a few typos or unclear elements).

However, to this reviewer's opinion, while the general idea of the work is interesting and filled with interesting challenges and scientific unknowns, the work presented here remains poor in scientific content, and falls short of addressing any of the initial science-based goals. Let me explain, intercomparisons between numerical models and experimental measurements are critical to be able to objectively determine the quality of the simulation results. However, if the effort stops at that, then it only becomes a mere technical work that anyone outside of the academic world could do. There are indeed publications where industry models are intercompared with experimental data, which have been used in the past to provide a sense of confidence or trust to industry. Alternatively, there are also publications from the academic world, where intercomparisons between models and experimental data are done, but in those there is traditionally a critical analysis of the influence of using different numerical schemes, subgrid models, filtering approaches, etc. So at the end there is an evident scientific gain. Unfortunately in the present work the potential scientific gain remains hard to find. What have we learned out of this manuscript? – That we are now entitled to do WRF-LES simulations in complex terrain with turbines? WRF simulations are done on a daily basis around the world, so how will this change what is done on a daily basis? One could have taken advantage of these great simulations to make a more robust objective/quantitative comparison between the simulations and the experimental data. For example, reading that there are wind differences observed at certain times of 1-2 m/s doesn't really mean much. That could be a 50% difference in a weak mean wind, or a tiny % difference in a strong wind scenario,... Or that the flow looks similar or dissimilar here and there, has little scientific rigor. Once the rigourous/strict comparison of results done, then one could have an additional section with more scientific insight. Research questions that come to mind could be:

As stated in the introduction of the paper, "The focus of this work is to model realistic atmospheric conditions and the associated turbulent flow phenomena to better understand wind turbine wake propagation in complex terrain, using the Perdigão site as the test location. Specifically, we analyze how the vertical deflection and dissipation of the wake varies based on atmospheric stability." This is an open question in the literature based on field observations, and WRF-LES-GAD is now applied for the first time to a real-case simulation with steep slopes to answer the question about wake deflection. The errors in the simulation are quantified via standard metrics (bias and RMSE) compared to a range of field observations. We are cautious to not generalize their results too broadly, but we have attempted to add language that addresses how the results could be interpreted for similar atmospheric conditions and for other sites that may be similar to the Perdigão site (lines 514-518).

1. What is the effect of the surface conditions? – I want to understand that your WRF-LES is using IBM for the topography in regards to the momentum, but is it using the same approach for the ther- mal/moisture field? Based on what I was able to gather from the manuscript it seems that there are no surface conditions for temperature,... does this mean that your flow is insensitive to the surface con- ditions? This is strange since one of the driving mechanisms to thermal stratification is the ground,... but not much is discussed about the impacts or not of the surface conditions on the flow?

The simulations do not use an immersed boundary method - they use the standard terrain-following vertical coordinate system in WRF. We have added language making this more clear in Section 3.3 in line 230. Additionally, the terrain-following coordinate system is mentioned in Table 1. The flow is sensitive to the surface conditions. We use the MYNN surface layer scheme (mentioned in Section 3.3), where the lower boundary conditions are determined from Monin-Obukhov similarity theory, see lines 253-255. The surface temperature is determined based on the Noah land-surface model and a radiative transfer model (the Rapid Radiative Transfer Model, RRTM, stated in Section 3.3). The land-use type is determined using the high-resolution CORINE dataset.

2. What is the effect of your turbulence initialization, and is it really worth it to run a suite of mesoscale WRF simulations just to provide time varying boundary conditions? Mesoscale WRF provides an ensemble flow solution that evolves with time, said otherwise, provides a more or less accurate

mean flow and thermodynamic conditions of the region. However, the LES simulations provide an instantaneous realization of the turbulent ABL, the question then could be posed as how does that compare to instead using profiles of experimental data to force the LES? Also, one can only wonder, what is the effect of the cell perturbation method to generate turbulence? Sure enough the mountains will spur some turbulence, but are the incoming turbulent flow conditions representative of all turbulent scales, including large scale perturbations?

The primary idea behind using multiple grid nests for the simulations is that large scale forcing and perturbations can be passed down to the finer nests where turbulence is resolved. The ultimate goal of such a setup is to use WRF in forecast mode over complex terrain. Although we do not attempt to run LES forecasts in the current work, using observations from the field campaign to force the LES would not achieve progress towards this goal. Furthermore, idealized simulations are unable to provide fully realistic turbulence conditions because the larger forcing scales are not captured (see further discussion in response to specific comment #7 below).

The role of the cell perturbation method has been addressed by Connolly et al. (2021), who studied the effect of using the cell perturbation method versus mountain-generated turbulence in weakly convective, strongly convective, and weakly stable atmospheric conditions for Perdigão. As we mention in line 242-244, they found that the cell perturbation method improves the representation of turbulence relative to the use of high-resolution complex terrain alone. Additionally, Muñoz-Esparza et al. (2014) found that using the cell perturbation method accelerates the generation of turbulence on the inner nest, with no adverse impact on the flow field and negligible computational cost. Also note that the CPM provides perturbations that lead to development of a full range of turbulent scales, as previously presented in several CPM studies, and this detail has been added to the manuscript at line 241.

3. An alternative, potentially the most interesting research question is how can one use the outcome of the rich simulations to understand flow configurations at other locations? Can results be scaled such that the results become generalizable in terms of stratification, mountain slope, terrain complexity? It would be a lot more interesting if the authors used the rich dataset to come up with generalizable relations that enabled one to extract conclusions at other locations without having to run expensive simulations,...

The major research question this article addresses is the vertical deflection of the wake in two distinct atmospheric conditions. Given conflicting literature (Barthelmie et al. 2019 and Menke et al. 2018) regarding the vertical deflection of the wake in different atmospheric stability conditions based on measurements, we use large-eddy simulations to address vertical wake deflection in complex terrain. The wake deflection has important ramifications for any turbines that could be located downstream.

As previously mentioned, we are cautious to not generalize their results too broadly, but we have attempted to add language that addresses how the results could be interpreted for similar atmospheric conditions and for other sites that may be similar to the Perdigão site (lines 514-518):

"We expect that the conclusions in terms of the wind turbine wake behavior would hold for all convective and stable atmospheric conditions at the Perdigão site as long as the phenomena of interest (recirculation zones and mountain waves) are present. Other phenomena could be modeled using WRF-LES-GAD to examine wind turbine wake behavior in other cases. With regards to other sites, the wind turbine wake behavior would depend largely on the vegetation (surface roughness) and steepness of the terrain."

More Specific Technical Comments

1. Line 42; when talking about 'length-scales' one should clarify that these are "turbulent length-scales"

This has been added.

2. Line 53; it is important to clarify the difference in time scales between the LES and the WRF mesoscale simulations. LES provides an instantaneous realization of the flow at high-temporal frequency, while WRF mesoscale provides results representing the outcome of an ensemble of flow marching in time, but that per the ergodicity definition can not be interpreted at the same frequency than otherwise the LES inout/output.

WRF is designed to operate as both a RANS and LES model, and in practice, the output of the model is a time-evolving 3D field, regardless of whether it is in RANS or LES mode. The timesteps in WRF are chosen according to the spatial resolution and stability limits. Multiple grid nests used to transition from RANS to LES are standard practice, e.g. Wiersema et al. (2020), Rai et al. (2017), and Arthur et al. (2020).

3. In line 57; can the authors provide a brief description of the GAD model? Meaning, I don't need to read the reference to find out whether the models is an actuator disk with/out rotation, etc.

We have added the following text to the manuscript at line 59-61: "In the GAD parameterization, thrust and rotational forces computed at the turbine's blades are averaged over a discretized two-dimensional disk formed by their rotation. These forces are then applied to the flow surrounding the turbine."

4. In line 82; the authors mention that "the goal of this work is to model realistic atmospheric conditions and the associated turbulent flow phenomena to better understand wind turbine wake propagation in complex terrain". At the end of the manuscript, what have we learned about this that maybe of use as a function of thermal stratification, or in other locations, mountains, terrain, etc?

As mentioned in the introduction (paragraph from lines 64-75), there is conflicting literature regarding the vertical deflection of the wind turbine wake in different atmospheric stability conditions. In this manuscript, we have learned that during stable conditions where a mountain wave occurs, the wake deflects downwards. During convective conditions where a recirculation zone forms, the wake does not mix with the recirculation zone and deflects above it.

Mountain waves occurred for almost 50% of the nights during the intensive observation period (Fernando et al. 2018). For recirculation, Menke et al. 2019 found that reverse flow with wind speeds greater than 0.5 m/s occurred over 50% of the time when the wind direction was perpendicular to the ridges. We have added these details into the manuscript at lines 130-132.

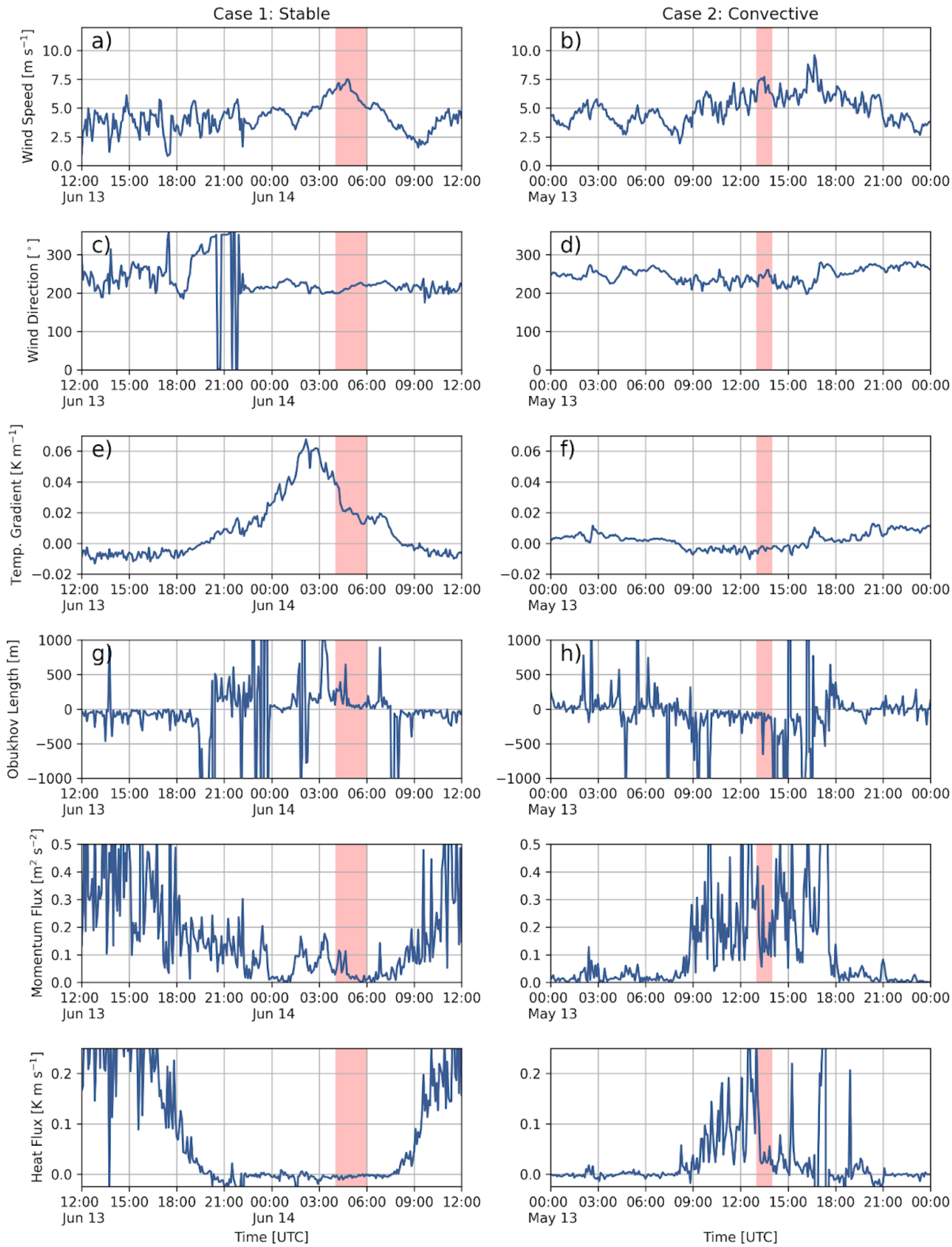
As previously mentioned in this response, these phenomena are site specific and depend on the characteristics of the terrain such as the slope and vegetation (surface roughness). But generally, we would expect that in other places with hilly terrain the wake would deflect up or down depending on stability.

5. By the end of section 2.1; what about the surface conditions? If they were not measured (besides roughness) how are they taken care of in the simulations? Is z0,t taken as a fraction of z0?

As mentioned in a response to a previous comment, we use the MYNN surface layer scheme (mentioned in Section 3.3), where the lower boundary conditions are determined from Monin-Obukhov similarity theory. The surface roughness lengths are based on the 100 m CORINE Land Cover dataset and we also use 30 m terrain from SRTM (mentioned in Section 3.3). Aside from the CORINE land cover which is specific to Europe, we have not changed any of the land-surface options which are standard in WRF. For the reviewer's reference, in WRF, z0,t is parameterized as a function of the Zilitinkevich parameter (Zilitinkevich 1995), z0, and Reynolds roughness number.

6. In Figure 2; I could only wonder but what happens to the near surface heat and momentum fluxes? – It is well known in the community the existence of counter gradient heat fluxes, specially in complex terrain. Why not include subpanels with the near surface heat fluxes?

In response to the other reviewer, we have added the Obukhov length to Fig. 2. Shown here is the new Fig. 2 with additional sub panels of the near surface heat and momentum fluxes (included here only for the reviewer - we do not believe the additional panels are needed in the paper). The near surface heat flux during the stable case is around -0.0025 K m/s and 0.025 K m/s during the convective case. During both case studies, the near surface heat fluxes are relatively constant. Comparing the near surface fluxes with large-eddy simulations is complicated above the surface because there is a combination of resolved and subgrid LES quantities. This analysis is the subject of our ongoing work, which focuses on turbulence quantities, but is not included in the present paper.



Case 1: Stable
Case 2: Convective
a)
b)
c)
d)
e)
f)
g)
h)
Wind Speed [m s^{-1}]
Wind Direction [$^\circ$]
Temp. Gradient [K m^{-1}]
Obukhov Length [m]
Momentum Flux [m^2 s^{-2}]
Heat Flux [K m s^{-1}]
12:00 15:00 18:00 21:00 00:00 03:00 06:00 09:00 12:00
Jun 13
Jun 14
00:00 03:00 06:00 09:00 12:00 15:00 18:00 21:00 00:00
May 13
Time [UTC]

7. Around line 185; It seems like simulations are initialized with a uniform heat flux. However, it is known also that the heat flux will be rarely uniform in complex terrain. How is that potential effect assessed? Are we left to interpret that the results are indifferent to that surface forcing?

The idealized simulations are indeed initialized with a uniform heat flux because they are formulated as relatively simple studies to inform the setup of the more realistic multi-scale simulation. A recent study showing that the multiscale setup gives better turbulence properties than the idealized setup is presented in Wiersema et al. (2020). This is because idealized turbulence is missing the large scale forcing that results from dynamic downscaling. A sentence in the introduction of the manuscript at lines 54-56 has been edited to mention this:

"Such setups can provide LES with more realistic time-varying inflow conditions directly from the mesoscale simulations, as these setups include the large scale forcing that results from dynamic downscaling capturing a wider range of atmospheric phenomena and more realistic turbulence compared to conventional idealized LES setups (Wiersema et al., 2020)."

In our multi-scale simulation setup, we use topographic shading and a land-surface model to account for non-uniform heat fluxes in complex terrain. This has been added in Section 3.3 from lines 255-256:

"Additionally, topographic shading is enabled to account for shading effects on the surface heat flux in the complex Perdigão terrain."

8. First line in Section 3.3; "Having demonstrated the ability..."; I dare say that to this point not much has been demonstrated besides showing two beautiful figures. Maybe the authors can tone this a bit down.

Our goal with the semi-idealized simulations was to see if the idealized model can capture the deflection of the wake with different phenomena of interest. We have edited the first line in Section 3.3 as follows: "Having demonstrated the ability of WRF-LES-GAD to capture the different types of wake behavior in a semi-idealized setup, we now proceed to the full multi-scale simulation."

9. Line 210; the authors mention that "adequate turbulence is developed", where is this shown? What is the premised/argument used

to judge that? A certain amount of energy at certain scales? A tke scaling of $k-5/3$? A certain spectral comparison of tke with the field experiment at different heights? – Some of this might be more "objective".

The text in the manuscript has been rephrased to say "spun up for 9 hours". This amount of spin up time is consistent with common practice for WRF. For example, Connolly et al. (2020) used an 8 hour spin-up time, used a 15 hour spin-up time, and Arthur et al. (2020) also used a 9 hour spin-up time.

10. It is unclear how the Cell Perturbation Method generates different type of turbulence for the stable and unstable stratification.

The cell perturbation method provides perturbations based over the boundary layer depth. The perturbation temperature is drawn from a uniform distribution within a range of potential temperatures that is based on an optimal Eckert number of 0.2 (see Muñoz-Esparza et al. 2015). This is the subject of the studies by Muñoz-Esparza et al. (2018) and Connolly et al. (2020) and more detail regarding the methodology for CPM can be found there and in Muñoz-Esparza et al. (2015).

11. From line 265 onwards, the text is riddled with subjective comparisons. I would suggest using percentages instead of absolute values when for example comparing wind speeds. Or avoid using comments like:"matches the measurements well", without an actual metric of it. See line 311; "errors are small" in comparison to what?

At lines 311, 317, and 388, subjective comparisons have been removed.

At lines 338, 346-349, 354, and 397-398, when discussing and comparing the errors, we have now quantified the errors with metrics of bias and RMSE within the text.

12. Why is the wind speed and direction outputted at different frequencies?

The wind speed and wind direction are output at the same frequency, 10 seconds. Perhaps the reviewer is commenting about the temperature gradient output frequency, which was 2.5 minutes, simply because of computational storage limitations.

13. Around line 367; the authors comment that the errors/discrepancies observed during the convective periods are larger than during the stable periods, but they don't provide any comment, hypothesis, argument, detailed inspection, trying to investigate that in details,...

The sentence containing this observation has been removed from the manuscript.

14. Figure17, great visualization for oral presentations, proposals or others where "candy-to-the-eye" is well accepted; but what is the use of such an image here? What is the intended message, outcome extracted? – I am not suggesting the authors remove the figure but instead include scientific argumentation around it.

Figure 17 shows the meandering and full development of the wake over complex terrain. The entirety of the wake cannot be visualized in two dimensions (vertical transects or plan slices) because of wind veer and horizontal/vertical wake meandering. The discussion for Figure 17 is qualitative while Figure 19, later on in Section 4.3, aims to be more quantitative regarding vertical wake behavior. We have added the following sentences in lines 429-431:

"The entirety of the wake cannot be visualized in two-dimensions because of wind veer and horizontal/vertical meandering. Three-dimensional visualizations provide insight into the wind turbine wake advection, meandering, and direction downstream as the flow evolves and develops over the first ridge and through the valley."

In conclusion, I am convinced the data presented in this manuscript is of high quality, and has the potential to become of high value for the community if made publicly available. My only concern is that there is not enough scientific value at presenting the data itself, when additional analysis (some of which does not require extensive work) could be added that would increase the scientific value of the work.

We thank the reviewer for the comments. We think that the revised manuscript and the responses above help to address most of these concerns.