1 Response to comments of Referee #2

We thank the referee for their useful comments and feedback. We have carefully incorporated them, as the numbered points discuss in more detail below.

1. Because of the complexity of the tropospheric structure, they do not seem to have isolated the causal relationship between wind response and particular profile features. This random relationship is illustrated in Figure 8 and 9. This is a little disappointing.
   We have indeed not found this, and leave determining such a causal relationship to future studies. We go into more detail in the response to point 2.

2. On the above point, it might be worth checking the following idea. The impact of profile details on the lower troposphere is probably caused either by the way the waves are launched (the low level N and U) or by the way that waves are reflected downwards. If the latter is true, then their idealized abrupt tropopause might be important as it is probably the main reflector (Fig 2?). An early paper by Klemp and Lilly (1975, referenced here), tried to explain severe downslope wind based on a tuning related to tropopause reflection. If this is the case, one can define another “Froude number” using the critical speeds for deep wave resonance of this type. Keep in mind however, that tropopause reflection is probably overdone in this model due the assumed sharp tropopause.
   We would like to thank the referee for their interesting suggestions. We will provide an overview of our attempts to find a predictive parameter for the influence of vertical variations.
   Klemp and Lilly (1975) study an atmospheric model very similar to ours, with a CNBL capped by an inversion layer, and a two-layer buoyancy structure above. In doing so, they find that the velocity response to a hill scales with a factor they call c_1 (equation 16). Their model is 1D, so in our analysis we used the total velocity magnitude in their expressions. However, we found no correlation between η_t/H and $u'/\bar{u}_1$. For the inflow velocity perturbation, this is illustrated here: Teixeira et al. (2013) also analyzed two-layer atmospheres

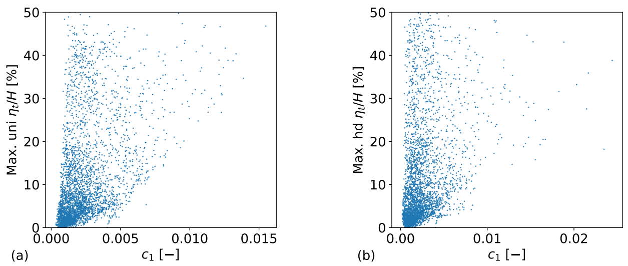

Figure 1: The parameter $|c_1|$ and the inflow velocity perturbation $u'/\bar{u}_1$ for the uniform (left) and non-uniform (right) ERA5 cases. There is no clear relation between the datasets.

   which could potentially capture the effect of the stratosphere. They looked into the influence of the Scorer parameters in both layers and the altitude of the tropopause through the parameters $l_1 H/\pi$ and l_1/l_2, but restricted their analysis to cases where $l_1 < l_2$, so that wave reflection would occur. This restriction leaves around 69% of the analyzed ERA5 cases. However, for these cases, we do not find a relation between the ABL flow perturbations and $l_1 H/\pi$ or l_1/l_2.
   One limitation of both studies mentioned so far is that they only consider one-directional flow. Teixeira et al. (2008, *Mountain Waves in Two-Layer Sheared Flows: Critical-Level Effects, Wave Reflection, and Drag Enhancement*, Journal of the Atmospheric Sciences) found that directional shear has a large impact on ground-level perturbations through the

presence of critical levels. As we took the full complexity of the ERA5 profiles into account up to the tropopause, such directional shear effects could explain the why the parameter of Klemp and Lilly (1975) don't predict the impact of the vertical variations. Future work could explore this further.
Aside from parameters from literature, we also investigated whether N_{strat}/N_{trop} and $||\vec{u}_{strat}|| / ||\vec{u}_{trop}||$ correlated with the TLM behavior. However, both of these parameters turned out to be independent from it.
Finally, we tried to see if the pressure feedback for a sine wave with spatial scales corresponding to the wind farm's dimensions could predict the size of the flow perturbations. The magnitude of this pressure is straightforward to compute as the sum of the stratification coefficients corresponding to the wavenumbers $\vec{\kappa} = (\pm 2\pi/L_x, \pm 2\pi/Ly)$. Unfortunately, this also did not correlate to the ABL displacement or velocity perturbations.
Within the paper, we modified the final paragraph of section 4.3 to mention these additional investigations, and to give a clearer suggestion for future work:

"*Figure 9 shows that the differences between the simulations seem to be independent from parameters that were good predictors of the TLM's behaviour in previous studies, such as Fr and P_N. We also investigated parameters that were found to correlate well with mountain wave drag in vertically non-uniform atmospheres, such as c_1 as found by Klemp and Lilly (1975, eq. 16), but could not identify a meaningful correlation in our case. Directional shear effects, such as those investigated by Teixeira et al. (2008), might explain the discrepancy.*"

3. One problem is that the authors discuss a large number of different model runs but the prrofiles are imprecisely described. I found it difficult to know the properties of each run. The problem begins with the Fig 1 where these plots do not match the equations just below (33 and 34). The problem gets worse from there as the reference to different wind and stability profiles are too casual and imprecise. This problem must be fixed for reader to follow the logic of the paper. Perhaps a table of run characteristics would help.
We added table 1, and reference it at the end of Sect. 3.3 to clear up any confusion:

Table 1: An overview of the different upper atmospheric flow profiles used for verification. The upper atmosphere set up by Wells and Vosper (2010) is also used in Sect. 4.1 and 4.2.

Upper atmospheric profiles	$u(z)$	$N(z)$
Wells and Vosper (2010)	Figure 2, left	Figure 2, middle
Two-layer Brunt-Väisäla frequency, constant wind	Constant	Eq. 34
Two-layer Brunt-Väisäla frequency, varying wind	Eq. 33	Eq. 34

4. I am not sure I see the point of figures 4 and 5. I think they are trying to show the impact of the Froude number based on the inversion strength (g'). It seems from these plots that that Froude number makes little difference. I kind of expected this. When tropospheric stability (N) is very small the inversion Froude number makes of big difference but for realistic values of N, the effect of the inversion is much less. There results in Fig 4 and 5 just seem to verify this general property. The properties aloft are more important than the inversion (g').
We agree with this assessment. We therefore leave out the subcritical case from the analysis, and only show and compare the uniform and vertically varying supercritical case. Section 4.2 has been rewritten, but the results of the analysis remain the same.

5. In broad terms I think this paper is valuable and significant as it points out that variable tropospheric wind and stability profiles significantly impact wind farm disturbance patterns.

We would like to thank the referee for the kind words, and for their helpful suggestions.

6. Minor points:

   (a) A little more explanation of Fig. 3 would be helpful. Why so many peaks?
   We added the explanation that was given for this on line 324 within the figure description:
   "*Above $k = 0.408km^{-1}$, the gravity waves become evanescent within some sublayers, leading to oscillations in A that do not correspond to resonant behaviour.*"

   (b) Line 122 Why does hydrostatic require the inversion?
   The hydrostatic assumption requires that pressure perturbations within the ABL are small compared to pressure variations imposed above. Without a capping inversion, there are wavenumbers for which these pressure perturbations are small, and the hydrostatic assumption might therefore no longer hold. In particular, we have noticed unphysical changes in ABL thickness for such cases.
   To clarify this in the manuscript, we changed line 122 to:

   "*Finally, we note that the hydrostatic assumption in the boundary layer ($\frac{\partial p}{\partial z} = 0$) is only reasonable as long as pressure effects within the ABL are negligible compared to those of the gravity waves. In particular in cases where a capping inversion is absent, we have noticed that this assumption may not be valid, which can lead to unphysical perturbations.*"

   (c) Line 130 How does it couple to an actuator disk model? Gaussain filter with L=1km
   The turbine forces are computed using an actuator disk model, with the wake-scale velocity gradients being incorporated using a wake model. These forces are then filtered onto the coarser TLM grid using a Gaussian filter with a length of 1km. To clarify this, we changed line 130 to:

   "*The turbine forces are computed individually using an actuator disk model.*"

   (d) Line 306 gives N=0.013 while the figure 3 caption gives N=0.0113. Are both correct?
   We thank the referee for noticing this mistake. The correct value is
   $N = 0.0113s^{-1}$. Line 306 has been changed to reflect this.

   (e) Line 227 to 230 are unclear.
   We changed them to clarify any confusion:

   "*since evaluating Ω and its derivative evaluated at the center of each sublayer in equations equations 28 and 29, as the use of a constant wavenumber implies, results in a discontinuous profile for W. As Pütz et al. (2019) found, these discontinuities cause the piecewise method to converge to a different solution as solving equation 16 with a simple finite difference solver. However, the physical reasoning behind the kinematic and dynamic boundary conditions is generally valid, indicating that they should always be used.*"

   (f) The title could be shorter and more attractive
   We propose the following alternative:

   "*Including realistic upper atmospheres in a wind-farm gravity-wave model*"