This document contains the responses to both reviewers. Significant additions were made to the manuscript to address their feedback. This has increased its length by ten pages – though seven of those pages are in the Appendix. All changes in the revised manuscript are redlined relative to the original with the exception of changes to Table 1 and instances where text has been moved around a figure (e.g. from just before a figure to just after). The revised and redlined manuscript is appended to the end of this document.

Reviewer 1

Review of 'A numerical study of the influence of terrain on wakes, blockage, wind farm efficiency, and turbine efficiency'

The article investigates the effect of an idealized terrain on the wake, blockage, and efficiency of wind farms. As articulated by the author, the study aims to extend the existing knowledge on terrain effects on individual wind turbine wakes and efficiency to a wind farm scale. This is a very interesting and important step forward in understanding the effect of terrain-induced pressure gradient on the wind farm performance and flow. Overall, the article is well written and easy to follow. I will recommend publication of the article, given the following questions/comments are adequately addressed:

Thank you for this review. It has led to improvements to the paper. I respond point-by-point below.

1. Terrain affects the flow in two ways: by imposing pressure gradients (which then lead to flow acceleration/deceleration) and by changing vertical flow shear – which alters turbulence production in the flow. The first of these effects is thoroughly discussed in the article; however, the second one appears to be completely ignored. It is suggested to at least discuss why terrain-induced turbulence can be ignored/does not play a significant role in this study. In addition, the effect of terrain-induced turbulence is also missing from the discussions/limitations section. I think this effect is worth discussing as well.

I have made some substantial changes to address this omission from the original manuscript. These changes include the following:

- **The Introduction now acknowledges the impact of terrain on turbulence and how this can affect wake recovery. Supporting references are included.**
- **Section 3.1, which discusses the two-row wind farm simulations, now addresses the influence of turbulence on the wake recovery between the first row and the second row. More specifically, we compare terrain-induced turbulence and terrain-induced pressure with respect to their impact on case-to-case wake recovery trends. The discussion in this section is based**

on the results of control volume analyses of the RANS solutions used in Section 3.1. The control volume analyses are described in detail in the Appendix.

- **Section 3.2, which focuses on blockage-related loss in single-row wind farms, now includes a sentence reporting the correlation between wind farm efficiency and freestream turbulence intensity, which is affected by terrain. The correlation is weaker than the correlation with pressure coefficient and Froude number.**
- **In part due to the feedback related to turbulence from the reviewers, there was a major change to the end of Section 3.1 where the paper evaluates a hybrid approach to calculating turbine interaction loss (i.e. combine results from freestream simulation of terrain with wind farm simulation without terrain). The evaluation reflects the fact that in practice a hybrid approach may account for terrain-induced turbulence. This is now acknowledged in the revised manuscript. The revision also now includes results from testing a second hybrid approach. The errors from the second hybrid approach are notably smaller on average than the errors from the first. Based on the above, I decided to shorten the write-up on the evaluation of the hybrid approaches and soften the related conclusions.**
- **There are other smaller additions, including a few related to the ability of engineering models to account for terrain-induced turbulence in Section 3.1 (twice) and Section 4.1. The revised manuscript also acknowledges the impact of the much higher background turbulence on wakes in the main valley case (Case J).**

These additions materially increased the length of an already long paper, but I believe they are needed to sufficiently address the reviewers' reasonable concerns regarding this point. Much of the additional content is in the Appendix.

2. The simulations use a spanwise inter-turbine spacing of 2D, which sounds very small. It is recommended to add an explanation or justification for choosing such a small spacing. On a similar note, the streamwise spacing in two row wind farm is chosen to be 15D. A possible explanation behind this choice will be beneficial.

I added text to the first paragraph in section 2.3 to address this:

The inter- and intra-row spacing is intended to be representative of wind farms in places with one dominant wind direction sector or two opposing dominant sectors. At these wind farms, the spacing between turbines in a row is typically 1.5 D to 3.0 D; the distance between rows is typically 10 D to 20 D. Such wind farms are common in Brazil, Chile, the U.S. Southern Plains, the western U.S., and the Taiwan Strait. Tight spacing between turbines is also common when a wind farm is located along a ridgeline.

3. Given the large number of simulations, I would suggest adding a table summarizing the distinguishing features of different simulations. I see that some selected simulations are summarized in a table in section 3.2. Perhaps a complete description earlier in the study could be added.

I have expanded the table to include all 28 single-row cases. I have also added columns to the table based on feedback from both reviewers. The table is not redlined even though about 75% of it is new.

The new, more complete table revealed an error I made in the description of Case A, which was supposed to be Case A with the buoyancy term in the vertical momentum equation zeroed out. What I had done instead was zero out the buoyancy *and* change the geostrophic wind speed in order to achieve freestream wind speeds at the turbine locations that are similar to those in Case A (the turbines are at the crest of the hill). Rather than change the text, I reran Case A** to be consistent with how it is described in the paper (same as Case A but with buoyancy off). The change made very little difference in the results. The turbine interaction loss went from 1.36% to 1.29%. C_P went from 0.445 to 0.443. Figure 16 and Figure 18 were updated accordingly. The change did not materially affect the correlation coefficients derived from these figures. I did have to update the reported difference in η between Case A** and Case D** (line 674). The change is larger than the change in η for Case A**. It appears I made a mistake when determining (or simply typing) the originally reported value, which should have been 1.72%, not 1.92%.**

4. A brief description/quantification of the free-stream flow in terms of speed-up and turbulence levels will benefit the reader in understanding the flow conditions wind farm/turbines are exposed to.

The freestream turbulence intensity at the turbine locations is now reported in the table. The table also includes the hub-height wind speed at the inflow boundary. Comparing this value with the freestream wind speed at the wind farm, which is already in the table, provides an indication of the speedup between the inflow boundary and the turbine locations.

5. In figure 7, it will be interesting to point the position of turbine rows to give the reader an idea of the pressure gradient experienced by the turbines in different rows.

Good suggestion. This has been done.

6. The discussion in lines 289-294 on the effect of wake trajectory is very qualitative. Perhaps a more quantitative approach could be used to support the argument that the trajectory effect is negligible in the current study.

I have gone ahead and added some analysis and text to substantiate the assertion that the turbine interaction loss trends are more the result of differences in the wake deficits than to differences in the vertical positions of the wakes. Along with the new text, a white line has been added to each of the three plots in Figure 5 to indicate the height of the maximum wind speed deficit as it varies laterally for each of the three cases. The added text in Section 3.1 is as follows:

The average deficit along the maximum-deficit white lines in Figure 5 is 12% for the *flat* case, 9.6% for the *hill windward* case, and 19.5% for the *hill leeward* case. When these deficits are run through the turbine power curve, they translate to power loss factors of 0.68, 0.74, and 0.52, respectively. These trends strongly resemble the case-to-case variation in the turbine interaction loss factor for the second row seen in Figure 6a (0.74, 0.81, and 0.57, respectively), suggesting that despite some differences in the vertical positions of the wakes between the cases, differences in the magnitudes of the wake deficits still appear to be the main driver behind the power loss trends across these simulations.

7. A lot of the contours are shown at the 'hub height'. How is the hub height defined? Is it a local one, meaning at each x, whatever the hub height would be, thereby a terrain-following hub height? Also, as mentioned by the author, and seen in previous studies, the wake trajectory may not fully follow the hub height trajectory. How would that influence the interpretation of some of the results?

The other reviewer asked a similar question about the "hub height" plots. To clarify the situation, text has been added to Section 3.1 that reads as follows:

This section also includes many references to *height*, which here means the vertical distance above the local ground level. For example, a hub-height surface is one where all points on the surface are 115 m directly above the local terrain.

I also added a couple mentions of hub-height surfaces later in the section to clarify what is meant by plotting the horizontal variation of a quantity "at hub height".

In the second part of comment, the reviewer points out that wakes may not (and in fact don't) fully follow the terrain and asks then how this might affect the interpretation of the hub-height contour plots. It's a good and fair question. The hub-height contour plots do not provide a complete view of the wakes and how they vary from case-to-case. The top-view hub-height plots in Figure 4 are supplemented with contour plots on vertical constant-x planes in Figure 5, which offer a more complete, though not fully complete, view of the wake differences between the three two-row cases in Section 3.1. The top-view hub-height contour plots of the single-row results in Section 3.2, however, do not in the original manuscript include additional contour plots on vertical constant-x planes. The main point of these top-view plots is to give the reader a qualitative sense for how

much terrain can affect wakes and blockage at hub height, along with an idea of the direction of the trends. Since terrain can materially affect the trajectory of a turbine wake, it is reasonable to wonder whether these top-view hub-height plots are sufficient to draw qualitative conclusions about the case-to-case trends in wake deficits at other heights. I have tried to address this with an additional figure now included at the end of the Appendix along with text in the Section 3.2 (lines 471-477):

The hub-height plots in Fig. 8 are intended to show the effect of terrain on blockage and wakes. In this section, there are eleven of these top-view hub-height plots, corresponding to first eleven cases in Table 1. To supplement these plots, and in consideration of the expected effect of terrain on wake trajectory, Fig. A-3 at the end of the Appendix shows the wake deficits on constant-x vertical planes 8 D downstream of the turbine row for each of the eleven cases. These plots confirm that terrain affects the vertical trajectory of the turbine wakes, but they also indicate that the hub-height plots in this section provide a reasonable representation of how the wake deficits vary from case to case through the rotor layer, at least close to the wind farm.

8. More details on the assessment for the real wind farm are needed. Specifically, the discussion in lines 620-630 needs to be expanded. For instance, which turbines were chosen for assessment, and what were the criteria for this assessment? Also, it seems that for these turbines, only individual power coefficient changes were assessed. Is this because the turbines were randomly apart from each other?

Lines 620-630 in the original manuscript corresponded to a discussion on the impact of terrain on the power coefficient (C_p) of a turbine simulated in isolation. Figure 19, which shows the wind farm layout, now highlights the turbines simulated in isolation. With the selection of the eight turbine locations, I was just trying to represent the geographic range of the wind farm. In the original manuscript, I only used two of the isolated turbines in unstable conditions and four of the isolated turbines in stable conditions to assess the impact of terrain on C_p. The original manuscript was not clear about how this subset of isolated turbines was selected. In the nominal C_p curve, which informs the performance curves used in the simulations as described in Section 2.3, C_p varies just 0.1% between 7.5 m/s and 8.5 m/s. In the original manuscript I only considered the isolated turbines operating within this wind speed range, leaving just two for unstable conditions and four for isolated conditions. The variable ground elevation across the wind farm causes significant wind speed variations through the wind farm (the wind speed varies much less in the flat terrain simulations, so none of the flat terrain isolated turbine simulations were filtered out). While answering this reviewer comment, I revisited this filtering criterion and found that the nominal C_p curve still only varies 1.0%

between 7.0 m/s and 9 m/s. I decided that this amount of variation was small enough for this exercise, and I changed the filtering criterion accordingly. Now the analysis includes 6 isolated turbines from simulations of the real terrain. The text in the revised manuscript reflects these changes. Here is the new full paragraph, which is redlined in the manuscript:

It is somewhat less straightforward to assess the effect of terrain on turbine efficiency in this case compared with the idealized cases. This is due to the variability of the terrain in combination with the fact that, unlike the C_P curve in Fig. 2, the C_P curve of a real wind turbine is not constant over a wide range of wind speeds. That said, the maximum variation along nominal C_P curve between 7 m/s and 9 m/s is just 1%. In the following discussion, this operating range is labeled the "nearly flat" part of the nominal C_P curve (again, the nominal C_P curve informs the U_{disk}-based turbine performance look-up tables used in the simulations). We simulated 8 of the 94 turbines in isolation; these turbines, which are spread across the wind farm, are highlighted with light blue dots in Fig. 19a. When simulated on the real terrain, whether in the stable or unstable conditions, 6 of the 8 solitary turbines end up on the nearly flat part of the nominal C_P curve. In stable conditions, the two northernmost isolated turbines are outside the nearly flat range, and in unstable conditions, the northernmost isolated turbine in each of the two rows is outside this range. When simulated on flat terrain, all the turbines operate on the nearly flat part of the nominal C_P curve. If we calculate C_P from the isolated and freestream simulations using Eq. (3) and limit the calculation to turbines operating on the nearly flat part of the nominal C_P curve, the average C_P for the turbines simulated in real terrain is more than 4% lower than the average C_P of the turbines simulated on flat ground – specifically, 4.4% lower in unstable conditions and 4.5% lower in stable conditions. As with wind farm efficiency, the average effect of terrain on turbine efficiency will be much lower when aggregated over the full range of wind speeds.

I did not quite understand the following comment/question: "Also, it seems that for these turbines, only individual power coefficient changes were assessed. Is this because the turbines were randomly apart from each other?**"**

The isolated turbines are distributed across the wind farm, as you can now see in Fig 19a. In case there is any confusion about how the turbines are simulated in isolation in this study, a clarifying clause has been added in Section 2.6.

"… we run a subset of the turbines in isolation, typically four turbines, one simulation per turbine."

This part of the line is new: "one simulation per turbine".

Reviewer 2

This paper presents a large set of RANS simulations of atmospheric flow over one or two rows of onshore wind turbines (actuator disks) located in parallel to a quasi-two-dimensional terrain feature (hill or valley). To identify the terrain effects on both turbine-scale and farm-scale efficiencies of power generation, additional simulations for the same turbines on flat terrain are also presented. The simulation method has been validated in the author's previous publications and no further validation is given in this paper. Overall, these simulations have been designed carefully and the results are very informative, although there are a few points on which the author's discussion seems insufficient or unclear. I would therefore suggest that the paper be revised considering the following points, before it is accepted for publication.

Thank you for this review. It has led to improvements to the paper. I respond point-by-point below.

1. A better explanation and discussion on the mechanisms of (turbine-scale) wake recovery should be given. This is a general comment not only on this paper but also on several other papers in the literature, but I am concerned that the effects of "terrain" and "pressure gradient" seem to have been discussed almost interchangeably. In general, these effects should not be discussed interchangeably, because "pressure gradient" is only one of several things that could be induced by "terrain". More specifically, the recovery of turbine wakes (or any wakes) should be discussed based on the conservation of both "mass" and "momentum", and the effect of "pressure gradient" is only part of momentum conservation (as can be seen from the RANS equations). I think the author's explanation given in the current manuscript would be sufficient if the flow was inviscid, where the acceleration/deceleration of the flow (caused by contraction/expansion of the flow passage due to terrain) would be directly linked to favourable/adverse pressure gradients. But in the real world, where the flow is viscous, this relationship is altered by mixing (or the Reynolds stress due to turbulence). This point needs to be discussed more carefully.

The first reviewer makes a point that overlaps with this one. The paper focuses on inviscid effects largely absent from commonly used engineering models for wind farm flows. Regretfully, in doing so I neglected to discuss terrain-induced effects that could be potentially accounted for by these models, notably those related to turbulence. Significant changes have been made to rectify this omission. These changes include the following (same list as provided to Reviewer 1 above):

- **The Introduction now acknowledges the impact of terrain on turbulence and how this can affect wake recovery. Supporting references are included.**

- **Section 3.1, which discusses the two-row wind farm simulations, now addresses the influence of turbulence on the wake recovery between the first row and the second row. More specifically, we compare terrain-induced turbulence and terrain-induced pressure with respect to their impact on case-to-case wake recovery trends. The discussion in this section is based on the results of control volume analyses of the RANS solutions used in Section 3.1. The control volume analyses are described in detail in the Appendix.**
- **Section 3.2, which focuses on blockage-related loss in single-row wind farms, now includes a sentence reporting the correlation between wind farm efficiency and freestream turbulence intensity, which is affected by terrain. The correlation is weaker than the correlation with pressure coefficient and Froude number.**
- **In part due to the feedback related to turbulence from the reviewers, there was a major change to the end of Section 3.1 where the paper evaluates a hybrid approach to calculating turbine interaction loss (i.e. combine results from freestream simulation of terrain with wind farm simulation without terrain). The evaluation reflects the fact that in practice a hybrid approach may account for terrain-induced turbulence. This is now acknowledged in the revised manuscript. The revision also now includes results from testing a second hybrid approach. The errors from the second hybrid approach are notably smaller on average than the errors from the first. Based on the above, I decided to shorten the write-up on the evaluation of the hybrid approaches and soften the related conclusions.**
- **There are other smaller additions, including a few related to the ability of engineering models to account for terrain-induced turbulence in Section 3.1 (twice) and Section 4.1. The revised manuscript also acknowledges the impact of the much higher background turbulence on wakes in the main valley case (Case J).**

These additions materially increased the length of an already long paper, but I believe they are needed to sufficiently address the reviewers' reasonable concerns regarding this point. Much of the additional content is in the Appendix.

As for the request for a discussion on the mechanisms of wake recovery, the Appendix, which analyzes the various contributors to wake recovery in the two-row simulations, discusses a number of the important factors at play, including mass conservation and the turbulent transport of momentum, along with terrain- and wind-farm-induced pressure gradients.

2. Somewhat related to the above point, I think the author's definition of "blockage" (Lines 64 to 68) is a little unclear. Here the author states that these terms "refer

to the inviscid response of the flow to the obstruction" but I think this "inviscid response" may need to be either rephrased or expanded. My main questions are: (1) Why does this need to be specifically about "inviscid" response? (2) Do we really need to define "blockage" in this way, which seems different from its traditional definition of "blockage" in aerodynamics? I understand that the blockage-related terms have been used in different ways (especially over the last 10 years since the concept of "wind farm blockage" was introduced by the author and other people) and I appreciate that here the author is trying to define how these terms are used in this paper specifically, but traditionally, people who work on turbine aerodynamics (especially those who test a turbine in a wind tunnel) tend to consider "blockage" as the ratio of the turbine's rotor swept area to the cross-sectional area of the flow passage (e.g. wind tunnel). From their point of view, the "blockage" is zero when the flow passage is not constrained laterally, but the turbine still decelerates the flow through its rotor swept area (i.e. "induction" is non-zero even when "blockage" is zero). The author's definition of "blockage" seems to contradict this traditional view, so this point needs to be explained more carefully.

There is some overlap between point #2 and the point below, or least some overlap in how I would like to respond to the two points, so I will respond to point #2 after the next point (#3).

3. Related to the above point, I think the author's definition of "induction" (Lines 68 to 69) may also need some modifications. Here the author states that the induction is "the turbine-blockage-related wind speed reduction upstream of the turbine" but I think "turbine-blockage-related" can be confusing (especially to turbine aerodynamicists, for the reason described above). To avoid this confusion, I think it would be better to say that the induction is "wind speed reduction induced by the thrust force of a turbine (or turbines)". Also, it would be worth noting that "induction" is often defined not only for a single turbine but also for a group of turbines (or a wind farm). My personal opinion is that the turbine-scale and farm-scale "blockage" discussed in this paper (and several other papers in the literature) should be referred to as turbine-scale and farm-scale "induction" instead.

<u>Short response</u>: I have added text to clarify the definitions of blockage and induction in the revised manuscript. The text is redlined in the paragraph spanning lines 71-80.

<u>Longer response</u>:

Before I first started publicly presenting on this subject in 2017, I spent some time mulling over how to label the phenomenon that I eventually came to call wind farm

blockage. The term "blockage" had been used in the wind literature before, but the word "induction" was also used to refer to a similar effect. However, so far as I was aware, "induction" was almost exclusively used in the context of an individual rotor and almost always referred to an effect on wind speed (there are, admittedly, two examples of papers from 2017 referring to an induction zone [1] or induction region [2] of lower wind speeds upstream of a wind farm, but in both cases the wind speed slowdown is attributed to "blockage"). In contrast, there were a number of papers discussing the "blockage" of an arrays of turbines, including [3][4]. Further, the term "blockage" more clearly evokes what I consider to be perhaps the most important aspect of the phenomenon: the fact that the oncoming flow deflects and is forced to go around, at least to a degree, the obstruction/turbine(s). For these reasons, I went with "blockage" over "induction". I have not changed my view. At the same time, I consider the personal preference of the reviewer on this subject to be reasonable – I would not try to change it. I am eager, however, to address the reviewer's specific concerns expressed in points #2 and #3. I do so below and in the manuscript.

The reviewer had some concerns about how "blockage" and "induction" are defined in the original manuscript. I will address these here one at a time, blockage first.

I think the reviewer is right: The definition of "blockage effects" given in the original manuscript ("the inviscid response of the flow to the obstruction") should be improved. The approach I have taken is to add to the definition rather than scrap it. I believe this definition is consistent with how the term is frequently used in the literature. I write "used" because blockage is usually not explicitly defined. It seems to me that the term blockage, as used in the wind energy community, often refers to the flow features missed by the traditional wake models because they do not directly include pressure and buoyancy in their governing equations. I'm not entirely comfortable using that definition, and I also think "inviscid response" is more concise.

The reviewer writes that the definitions of blockage and blockage effects are "a little unclear"; if the critique is suggesting that the definitions would benefit from more specificity, then I have to agree. I have addressed this shortcoming by adding specific examples of inviscid effects, focusing on those most relevant to the manuscript. Here is the current text, much of it new:

"*Blockage* is an obstruction to the otherwise free flow of air. In this paper the obstruction relates to either a single turbine or a group of turbines. *Blockage effects* refer to the inviscid response of the flow to the obstruction. Perhaps the most salient blockage effect is the partial diversion of the flow around the turbine or turbines. The divergence of the streamlines starts upstream of the obstruction and directly connects with an upstream wind speed reduction via continuity. Further, the vertical deflection of the flow can perturb the stably stratified atmosphere in such a way as to significantly modify the blockage-related slowdown upstream of a wind farm (Lanzilao and Meyer, 2024)"

I hope the new text also helps clear up confusion about what is meant by "turbine-blockage-related" in this passage: "… *turbine blockage* refers to blockage from an individual turbine. *Induction* is the turbine-blockage-related wind speed reduction upstream of the turbine**." Just in case, I have added the following to the end of the sentence based on the recommendation from the reviewer: "**alternatively stated, it is the upstream wind speed reduction induced by the thrust force from an individual turbine.**" I have purposely added the word "individual" and left off the recommended "(or turbines)" to keep the definition of "induction" consistent with how it is used in the paper. To be clear, the revised manuscript still includes the original definition. I wish to keep the definition because, among other things, it aligns well with the physical explanation offered for some terrain-related trends in calculated power coefficient. Also, there is precedent in the literature for connecting blockage with induction, including [5][6]. That said, I do not wish to cause confusion, so I have also included the recommended definition.**

Finally, I want to acknowledge the reviewer's concerns about the use of the term "blockage" rather than "induction" in this paper and other papers on this subject. I obviously prefer the term blockage over induction for the reasons already given; however, I admit that its use in the analysis and description of wind farm flows is not without drawbacks. The term "blockage" is historically much more commonly used in internal flows, especially wind tunnel flows, where it usually takes on a specific meaning (ratio of blocked area to cross-sectional area of the test section of the tunnel).

It was in the context of internal flows that I first came across this term. Early in my career I worked in axial compressor aerodynamics and blockage came up often, but it meant more than just a reduction in the effective flow area in the annulus. A primary effect of blockage was to deflect approaching flow around the obstruction (which was typically a hub separation or tip clearance flow) forcing a spanwise redistribution of the flow with significant performance consequences. This use of the term "blockage" – an obstruction diverting flow, causing performance changes – would not be unfamiliar to those using the term in the analysis of wind farm flows.

Separately, there are examples of the term blockage used in external flows. A Google search indicates that the term "orographic blockage" is used in many papers, though I suspect that in some of them, perhaps most of them, "blockage" is actually referring to "blocking", which typically involves specific atmospheric

conditions and a stagnant pool of air in front of the obstruction – not really analogous to what we are referring to when we use the term "wind farm blockage".

The term "blockage" is commonly used in analyses of tidal turbines and tidal turbine arrays. This might be the closest relative to the use of the term blockage in wind farm flows. One might argue that blockage in the context of tidal turbines is more analogous to wind tunnel blockage, with the free surface acting like the top of the wind tunnel, but one could also argue that the free surface is analogous to a capping inversion and stratification above the atmospheric boundary layer, the presence of which is believed to significantly influence wind farm blockage effects. That said, tidal turbines are laterally constrained in channels; wind farms are not.

In the end, I have to concede that "blockage" is an imperfect term, and if I were a wind turbine aerodynamicist specializing in wind tunnel experiments, I would probably be annoyed with how the term is used in the context of wind farm flows. Nevertheless, the term is now widely used in the wind industry and in the wind energy literature – usually in a way that is similar to how it is used in this paper. In the absence of a term that is clearly better, I am not inclined to change it here. The best we can do is define the term and use it in a consistent way throughout the paper.

4. Line 60: "Other research suggests that…" I think a few references should be cited for this.

The references are in the paragraph that starts, "In the LES results reported in Troldborg…" In an early draft, this paragraph came immediately after the text you flagged. However, in a later draft I added, in between, the paragraph with the blockage-related definitions. To address this issue, I have added a couple words to the flagged text:

Other research, discussed below, suggests that…"

5. Equation (1): Please mention what the suffix "WOA" stands for.

"WOA", which stood for Witch of Agnesi, had been replaced with "ground" in Eq. 1 in the revised manuscript.

6. Figure 4 (and other similar figures): Please explain more explicitly what "at hub height" means. I guess these contour plots of wind speed reduction are on the surface that is 115 m (hub height) above the ground everywhere, i.e., this surface is also curved if the ground is curved, but this is not very clear from the figure caption or the main text.

The other reviewer flagged up the same issue. There is a paragraph at the beginning of the results section that specifically defines what is meant by "wind speed" and

“pressure” in the section. In the revised manuscript, this paragraph also includes the definition of “height”:

This section also includes many references to *height*, which here means the vertical distance above the local ground level. For example, a hub-height surface is one where all points on the surface are 115 m directly above the local terrain.

I also added a couple mentions of hub-height surfaces later in the section to clarify what is meant by plotting the horizontal variation of a quantity “at hub height”.

7. Table 1: The power coefficient CP presented here is a “turbine-scale” CP defined based on U_inf,disk (Equation 3). But I think it would be informative to also present a “global” or “overall” power coefficient (say, CP,global) defined based on a single reference wind speed (e.g. 9 m/s) that is the same for all cases, as a metric for the overall farm power.

I have added a column with wind farm power coefficient. $C_{P,WF}$ is the sum of the power of the turbines divided by the sum of 0.5*rho*A*U_{disk}^3 for all the turbine locations. I have mixed feelings about including this column. The quantity is equivalent to $\eta * C_P$, both of which are already in the table. One could argue then that $C_{P,WF}$ does not convey additional information. That said, one could also argue that including $C_{P,WF}$ highlights the point that the overall energy conversion efficiency of the wind farm includes the efficiency of the turbines and the turbine interaction efficiency of the array, and if terrain affects both turbine and array efficiency, the multiplicative effect is different than one might appreciate if they looked at only one of the efficiencies. I only make one reference to $C_{P,WF}$ in the revised manuscript, at the end of the second paragraph in Section 4.1.

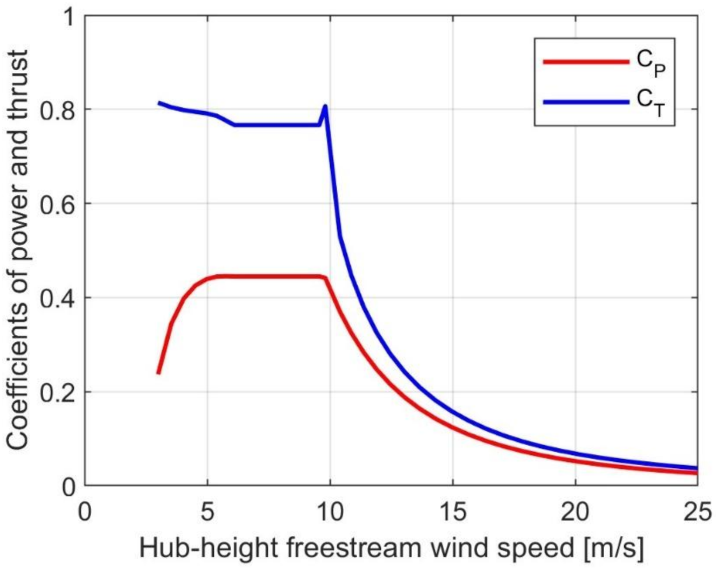
8. Line 426: Here it is noted for the first time that the main difference between Cases J and L is the prescribed geostrophic wind speed. I think this information (geostrophic wind speed) should be included earlier in Table 1.

This has been added to the table.

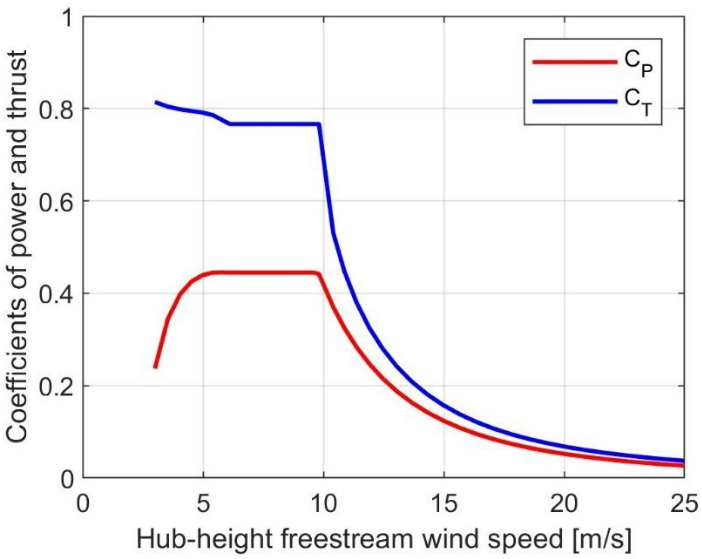
9. Line 449: “the lower wind speed case exhibiting hill-induced gravity waves with longer wavelengths” Is this correct? It looks like Case K has a “shorter” wavelength.

Well spotted. I have corrected the error in the revised manuscript.

Final note: A colleague while reviewing the paper flagged the odd spike in the C_T curve in Figure 2 at close to 9.8 m/s:



The above in the original IEA-3.4-130 curve ([https://github.com/NREL/turbine-models/blob/main/turbine_models/data/Onshore/IEA_Reference_3.4MW_130.csv) reference](https://github.com/NREL/turbine-models/blob/main/turbine_models/data/Onshore/IEA_Reference_3.4MW_130.csv) in the paper; before I used it in this study, however, I removed the spike, which involved editing just one entry in the table behind the curve. Figure 2 now reflects the C_T curve that was actually used in the study:



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