Reviewers' comments are colored in black, authors' responses are colored in blue.

General remarks

We would like to thank both reviewers for the time and effort they invested in reviewing and assessing the manuscript. Both reviewers have pointed out that we do not propose a solution to the suboptimal performance of a controller tuned for flat terrain in complex terrain. The contribution is not in developing new models or methods, but in analyzing an existing method (k-ω^2 control) from a new perspective and at a high level of numerical and analytical detail. Since k-ω^2 control is widely used, we believe this work significantly contributes to the wind energy research community's knowledge.

We have made significant changes to the manuscript, with the most relevant listed below. None of the modifications affects the article's conclusions.

- The title has been changed to *"Aerodynamic analysis of standard variable-speed torque control in complex terrain,"* to clarify the scope of the article.
- The identification of the maximum performance at the different turbine locations has been improved by replacing interpolation on a coarse λ-θ grid to find the optimum by a nested grid search where the optimum is found as a discrete value on a grid with spacing $\Delta\lambda = 0.02$, $\Delta\theta = 0.02$ deg. This modification did affect numerical values in the manuscript, but the conclusions stayed unaltered.
- The introduction was rewritten with a greater focus on prior studies and on the specific contributions of this work to the authors' prior publications. Furthermore, the description of the controller has been mostly moved to the methodology section, with only a brief description remaining in the introduction.
- The methodology now features more details on the grid sensitivity (Sect. 2.6), as well as a section on the controller (Sect. 2.2). More explanations of the methodological choices are also provided.
- Flow fields are now included in the manuscript. In this context, also the expected performance biases due to different levels of turbulence intensity are analyzed (Sect. 3.1).
- Throughout the results section, the simulation results for the additionally considered cases (C2, C3.X, C4.X) are now shown as well.

Since the modifications are significant, we kindly ask you to review the entire, revised manuscript.

Reviewer 1

The manuscript in question presents simulation results and analysis regarding the performance of standard k-omega-squared torque turbine torque controllers in non-flat terrain. Although by itself an interesting and relevant topic worthy of publication in WES, I believe the manuscript has some significant shortcomings that would need to be addressed before it can be published here. All comments can be found in the PDF attached, but my main concerns are reiterated below:

- The paper is generally poorly written. Some sentences are hard to understand or do not flow naturally. The structure of the paper does not always make sense and should be reconsidered. The introduction is missing big chunks of information that are relevant for the paper. I strongly suggest having a professional editor or at minimum a native English speaker with writing experience take a look at the manuscript before resubmitting.
  We carefully proofread the entire manuscript and hope that the new submission now satisfies the reviewers' language standards.
- The authors generously self-cite previous work, but fail to point out what specifically this manuscript adds with respect to previous publications. I have not read all the previous publications in full, but I've read enough to know that there is significant overlap between those papers and this

manuscript. To address this, the authors should include a more descriptive paragraph on previous research in the introduction, followed by a summation of contributions unique to this work.
We have added a paragraph at the end of the introduction clarifying the contribution of the present work beyond our previous studies.

- A lot of the methodological choices and modeling simplifications are not or not well explained or justified. This includes but is not necessarily limited to: the choice of the terrain, the choice of the surface roughness, the choice of the turbine location, the choice of the controller used, the choice of the turbine model. Flow results from the RANS simulations, which are the heart of the manuscript, are almost completely omitted, making it very hard for readers to assess what is happening to the flow around the hill, with and without a turbine.
We have extended the description of the methodological choices. Furthermore, flow results with and without the turbine are now included in the manuscript.

- From the introduction, I was made to understand that the main contribution of this manuscript was that it investigates how suboptimal control affects power performance of turbines in non-flat terrain. The authors cite different studies investigating complex terrain, and note that suboptimal control might influence the findings of these studies. However, in the manuscript, they fail to quantify the exact influence of a sub-optimally tuned controller on the total power production of a turbine on or near a hill. Furthermore, they fail to implement potentially simple improvements to the controller, to verify that the power production can actually be increased on non-flat terrain.
We added numerical values of the suboptimal performance of the controller. As stated earlier, the goal of this article is not to propose a new control method. However, as also noted in the article, a simple solution to the suboptimal performance is to retune the torque constant k (and adjust the blade pitch), which would restore optimal operation. The C_P-λ curves in Fig. 6 in the revised manuscript show that performance can indeed be increased by such a retuning. However, we did not perform simulations with a retuned controller because, based on the generated insights that the controller strictly follows its control curve, we can already conclude that this would help.

Despite these shortcomings, I believe this manuscript still has the potential to be published in WES. Apart from improving the presentation quality, this would also require a significant rewrite to emphasize the value of the manuscript with respect to previous work, as well as additional simulations to assess the true influence of sub-optimal or uncalibrated control on wind turbine power production in non-flat terrain. Ideally, the manuscript would also propose a solution to this challenge (perhaps as simple as a recalibration of the optimal TSR), including simulations showing the potential benefit of this solution.

In the following, comments from the manuscript attached by the reviewer are answered. Comments regarding grammar and style are not listed, as their suggestions are directly included in the revised manuscript. Thus, only comments that, from the author's perspective, require a direct, formal response are discussed below.

Line 7 f. These numbers are misleading as they are with respect to the reference wind speed. You should decouple which part of the performance loss is due to the hill, and which part due to the controller.
We improved the phrasing and explicitly state the reduction of the power coefficient due to the suboptimal operation of the controller.

Line 16 In my opinion, the introduction requires a significant rewrite. The order of things is not logical, parts are not well-written, and an introduction to the effect of complex terrain on the flow is missing. I suggest ordering the introduction as follows: 1) introduction to flow in flat terrain, and how controllers are tuned for optimal performance in these conditions 2) introduction to complex terrain and how it influences airflow 3) Overview of literature studying turbine performance in complex terrain, showing the knowledge gap that you are addressing with this paper 4) Introduction to your proposed solution
We have modified the introduction and also have incorporated your suggestions.

Line 22 I disagree with the causality here. In Region 2, the controller aims to extract maximum power, which HAPPENS to coincide with a (relatively) constant CP and CT. Not the other way around. The controller in this region generally regulates torque to keep the TSR constant, so perhaps you meant to say that

From our perspective, it is not a coincidence that C_P and C_T are constant in region 2. When a turbine controller is designed, a C_P-TSR-pitch surface, as shown in Fig. 4 of the revised manuscript, is consolidated to identify the point at which the turbine performs aerodynamically optimally. The TSR and pitch at the optimum C_P are then used to set the blade pitch and the torque constant (Eq. 3 in the revised manuscript), which are subsequently used to calculate the generator torque. Fixing TSR and pitch ensures constant local flow angles at the blade and performance at the C_P these values correspond to, regardless of the wind speed. This can be seen in Eqs. (3) and (7) of the revised manuscript. The entire derivation of a generator torque controller is based on the assumption that there is only one distinct point of optimal operation (TSR, pitch) at which the maximum power performance quantified by the optimal C_P is reached. So yes, the controller attempts to keep TSR constant, but only because at this TSR, the C_P is optimal. See also Johnson et al. (2006) and Bianchi et al. (2007). Thus, it is not a coincidence that C_P is constant in region 2. For C_T, the argument can be made that forces scale with the wind speed squared (at constant TSR and pitch), so the ratio of force to wind speed squared must remain constant. So it is a consequence of the design, and one could potentially argue it is a coincidence. However, because a C_T-TSR-pitch surface uniquely relates a given TSR and pitch to a given C_T, one could also speak of a deliberate decision to operate at this C_T. These arguments, of course, only hold when Reynolds-number independence at the blades can be assumed. We have also improved the description of the torque controller in the revised manuscript and hope it further clarifies it.

Figure 1 I believe this figure is redundant. The different control regimes of a turbine can be considered general knowledge, so unless you use data specifically extracted from this figure later in the paper, this can be omitted and the text describing the regions suffices (with potentially a reference to the original source of this figure)
We have removed Fig. 1 from the original manuscript and instead added Fig. 5 in the revised manuscript, which not only shows the different control regions, but also the impact of the different turbine positions on the power and control curves.

Line 28 What I'm really missing here is an introduction to complex terrain / topology and the effect it has on the wind. Explain that most WTC research focuses on "simplified" situations with flat/uniform terrain, and how the flow generally develops around a turbine in this terrain. Then introduce complex terrain and explain what effect that has on flow development. Then, finally, you can argue why this affects power extraction and might require turbine control redesign
In this work, we focus on a situation that is complex enough to represent a certain terrain-induced effect, namely, flow acceleration and deceleration, but not too complex so that no clear conclusions can be drawn anymore. Therefore, we do not perform an in-depth literature review of the flow in complex terrain. It would touch up on many topics, such as stability, thermal winds, and more, without adding much to the specific problem we are interested in. Nonetheless, we have addressed your concern by including how controllers are usually tuned, considering only streamwise uniform flow and further argue why in complex terrain this tuning might not be sufficient (see l. 29 ff.).

Line 34 I'm not sure I understand this statement. The controller will do what it's made to do, regardless of the flow. Non-uniformity in the flow just acts as a disturbance. I think what you mean to say (or should say) is that the effect of complex terrain on the flow might fall outside the design scope of the controllers implemented in these studies, and as such, controller performance might be suboptimal.
We have clarified this sentence (see l. 60 ff.).

Line 40 This part (lines 40-60) might be to technical for an introduction. Consider moving to Section 2, and limit yourself to explaining the assumptions made in turbine controller design, and why these don't apply in complex terrain.
We have moved most of the technical description of the controller to the methodology (Sect. 2.2) and only kept what we consider as relevant for the reader in the introduction.

Line 71 So no alternative controller tuned for complex terrain will be introduced in this paper?
No alternative controller is introduced as we consider it as out of the scope of this study, which focuses on the physical analysis of the k-ω^2 controller.

Line 77 Why were these three cases chosen specifically? For example, why wasn't a case with the

turbine behind the hill considered? I would expect different performance there too, right?
Indeed, behind the hill, we would also expect a different performance. The cases in this work are considered due to the following reasons: Flat terrain is used as a reference case; the case ahead of the hill is chosen because it features a situation where flow acceleration leads to a higher performance than in flat terrain, while the case on top of the hill leads to lower performance due to the deceleration. So we have one case with higher performance and one with lower performance than expected. We have added this explanation in l. 88 ff. in the revised manuscript.

Line 84 Only the blade geometries, or just the full geometry (tower, nacelle, hub, etc)?
Only blade geometries are considered. In the revised manuscript l. 95 clarifies this now.

Line 86 While I agree that the k-omega-squared controller is the most common way to regulate torque in region 2, it is not the only way. For example, the Reference Open-Source Controller (ROSCO) also offers the option to use TSR tracking based on internal wind speed estimates. I would expect these estimates to go up at the bottom of the hill or go down at the top of the hill, which might result in closer-to-optimal performance for this controller. Comparing results only for the most basic torque controller is in my opinion not a fair way to determine potential power losses due to complex terrain
As pointed out by the reviewer, the k-ω^2 controller is the most common way of wind turbine control in region 2. Therefore, it is in our perspective consequent to also use this controller in our analysis. We can only speculate how a TSR-tracking controller would affect power performance, and a future study (potentially based on transient simulations) should certainly compare different control strategies. The focus of this study is on understanding the k-ω^2 controller in detail.

Line 102 I would like to get a better sense of what the inflow wind in the simulations looks like. Is wind shear considered/implemented here? I can imagine that this also has an effect on the flow of the turbine on the top of the hill, as it will likely experience higher wind speeds than the turbine at the bottom of the hill. Please include (averaged) wind speed and turbulence (and possibly wind direction) plots as a function of height, possibly at different locations in the domain (inflow boundary, location A, B, C). I think this is absolutely vital information to assess the results presented in Section 3.
Figure 3 in the revised manuscript shows the undisturbed vertical velocity and turbulence intensity profiles at the turbine positions. Since the inflow is quasi-two-dimensional, a wind direction plot is not necessary, and the domain inflow profile is given for all cases by Eq. (12) in the resived manuscript.

Line 117 Why was this configuration chosen? Is there any reason/logic to this configuration? How does this compare to other complex terrain studies? How realistic is this configuration with respect to actual hills that might have wind turbines placed on/nearby them?
The reason for this setup is that it allows for easy modification of the flow around the turbine by modifying hill and surface parameters. As mentioned before, the terrain is complex enough to fall outside regular turbine design considerations, but not so complex that it is not possible to draw definite conclusions. Thus, the goal is not to achieve a highly realistic setup. In general, the setup's dimensions are similar to those in other studies (Liu and Stevens, 2020; Mishra et al., 2024; Troldborg et al., 2022). This is also elaborated in Sect. 2.4 of the revised manuscript. How this setup compares to realistic conditions is a very open question, given the variety of complex terrain. There are certainly cases where it compares well, see, for example, the wind turbine located on a landfill in Freimann, Munich, Germany.

Line 129 The ADM is a significant simplification of a real turbine rotor. Have the authors also investigated what effect using such a simplification has on the results? E.g., by comparing results with an actuator line model? Especially in non-uniform conditions, I can imagine that you might get very different results.
We have not compared the results to actuator line models as we are performing RANS simulations. However, we do not expect the main conclusions to differ significantly, as they are supported by theoretical considerations in the discussion and the appendix. In an earlier study by Hodgson et al. (2021), comparing actuator disk and actuator line models coupled with the aeroelastic solver Flex5 (Øye, 1996)(which includes a torque controller), it was found that steady-state performance and loads agreed well between the methods. Nonetheless, unsteady simulations might yield additional insights into unsteady effects, and actuator line modeling is a logical next step in our research.

Line 134 "in this work" implies that these results will be shown later on, which I do not believe is the case. I do appreciate that the authors took the time to execute such a sensitivity analysis, but would like to see the results in a little bit more detail. Perhaps you could show how the cell volume affects the power and induction in a simple plot?
We have added Fig. 2, which shows the sensitivity of reference velocity and C_P on the cell size.

Line 156I understand that RANS simulations can be expensive, but this is a very coarse grid. The differences in the optima found are smaller than the applied grid size in cases A and B. I would say it is prudent to reduce the grid size around the optimum to increase the reliability. At the very least, you should run simulations with the found optima to verify that the CP values found through interpolation hold. Perhaps that is already done, but if so, that is not clear from the text
We have modified the methodology and identified the points of maximum performance now by a nested grid search with the finest grid having a spacing of $\Delta\lambda = 0.02$, $\Delta\theta = 0.02$ deg. This allows for making definite conclusions about differences in power between the considered cases.

Line 158 Please clarify. Furthermore, the same seems to be the case for labda = 9, theta = -5
The comparably high thrust coefficient accompanied by a strong deceleration behind the hill leads to an oscillating solution. See l. 241 ff. in the revised manuscript. The case for $\lambda = 9$ and $\theta = -5$ indeed converges, but in the original figure (Fig. 2 of the original manuscript), the colormap was cut off at a value above the achieved C_P for this case, so it was not directly visible in the figure. The new figure (Fig. 4 revised manuscript) has an improved colormap.

Figure 2I think you should split this figure up into two figures: a first figure showing the topology, including more details on the slope angle, incline, horizontal and vertical distances, etc. And a second figure that shows the CP surfaces for the three different locations.
The figure is split up in Fig. 2 and Fig. 4 in the revised manuscript.

Figure 2 In addition to this figure, I would love to see a figure of the wind speed (both disturbed and undisturbed by the turbines) as a function of horizontal location, either at hub height (so moving along the surface of the ground) or at a specific altitude (not moving along the surface of the ground), to get a better understanding of the influence of the terrain on the flow
Figure 8 in the revised manuscript shows the undisturbed and disturbed wind speed at constant height (hubheight) above ground.

Line 180 I would like to see the difference between performance with the "standard" controller (which I assume will use a pitch of 0 degrees) and the calibrated controller. I assume that the difference in performance is significant, perhaps even bigger than the difference between cases A, B, and C, which shows that your simulation environment and model assumptions have perhaps a bigger influence on the performance of the controller than the terrain does.
We have performed the same set of simulations using the control constants specified by Bak et al. (2013) with $\theta = 0$ deg and $k = 10652234\ \mathrm{Nm}^2$. Note that k is adjusted for the generator efficiency. Results are shown in Fig. 1. In comparison with Fig. 6 of the revised manuscript, the following can be said: In general, for $\theta = 0$, significant differences in maximum performance between the considered locations can be identified. At the hill foot, the maximum performance is approximately 2.2 % higher than in flat terrain and on top it is approximately 17.5 % lower. These values are smaller than what we find for our tuned controller (2.5 %, 18.7 %). However, it is also known that the effect of streamwise flow acceleration on performance increases with increasing C_P, which is higher in the tuned case (Zengler et al., 2024, 2025). Using the original control settings yields non-optimal performance and a different C_P compared to Bak et al. (2013) in flat terrain in our case, because the flow representation in CFD differs from that in BEM (which was used to tune the original controller). This effect is also visible in other studies (Hodgson et al., 2021). On top of the hill, the difference between the performance at which the controller settles and maximum performance seems to be comparably small; however, the control curve reveals that this is because the control curve is not crossing the optimum in flat terrain. If this were the case, differences in performance on top of the hill would be more significant. Also in this case, the controller operates even further away from the optimum on top of the hill.

There is a fundamental problem when adapting control values from BEM to CFD: one can either use the torque constant directly or use the tip-speed ratio to retune the controller and then obtain the torque

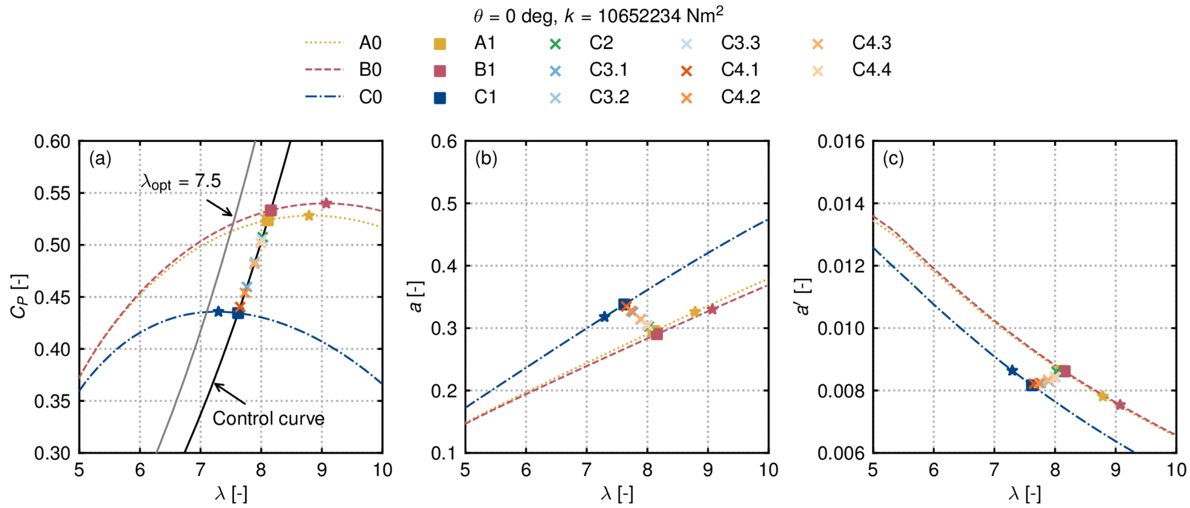

Figure 1: Simulation results for a controller with control constants based on Bak et al. (2013).

constant via an additional simulation that determines C_P at the given tip-speed ratio. We decided to go for the first option as it seems to be the more natural step. When coupling CFD simulations with aeroelastic solvers, the controller in the aeroelastic solver is rarely retuned. In Fig. 1, we also show what the control curve would look like if we had tuned our controller based on an optimal tip-speed ratio of 7.5 for flat terrain as reported by Bak et al. (2013). In this case, the difference to maximum performance in flat terrain would be even more significant, and still, we do not meet maximum performance on top of the hill, because we still do not include the C_P-TSR curve there in our controller tuning.

Overall, it can be seen that our reported differences in performance and the non-optimal operation are not due to our calibration, but rather to the physics constraining the maximum achievable performance and to the k-ω^2 controller being tuneable only to a single optimum in its standard configuration.

Line 218 I'm missing some important discussion points in this section.. It should also include: 1) the role of this specific controller wrt other possible controllers, as mentioned earlier 2) The role of your (lack of) calibration. Unlike what researchers like to believe, wind turbine operators are not idiots. I can't confirm with 100 % certainty, but I think it's safe to assume that operators calibrate each turbine to operate at its optimal CP when it's taken into operation. By simply changing the optimal TSR based, the turbine could likely operate at (close to) its optimum without updating the control law.
We never assumed anything about how operators calibrate turbine controllers, and at this point, we clearly reject any insinuations. We do not consider operators to be "idiots," nor do we see how such a claim could be inferred from the manuscript. We have shown in the introduction that in several research papers, it is not clear whether the utilized controller was tuned for complex terrain/off-design operation conditions, which makes it difficult to interpret performance results, and which motivated this study.

Regarding the other points: 1) including other controllers here would be mostly speculation from our side and we do not want to draw wrong conclusions. Therefore, we do not include them here. For a future study, it is certainly relevant to compare different controllers. 2) Yes, a simple retuning would fix the suboptimal performance. This is addressed in the revised manuscript in l. 286 ff.

Figure 5 What is the value of showing these plots over the radial position mu? From these figures, I would conclude that a is very different for different turbine locations, while a' and alpha are very similar. However, that does not seem to be related to the radial position.
The drawn conclusions that only a is affected by the location when using a torque controller is the intention of this figure. Since a' and specifically α are rather local quantities which depend on the radial position, a plot over the radial position showing their behavior in the different setups seems to be most appropriate from our side. Only claiming they are independent of the position in the text or writing out values for specific positions might not be as convincing. The slightly different results close to the blade root for a' also indicate that our theoretical reasoning in Appendix A is limited to regions of the blade

where drag is negligible.

Line 296 Sure. Or, just calibrate k for an individual turbine location once when putting it in operation, which would likely accomplish a very similar result at much lower complexity
Yes only recalibrating might be an easier solution. However, we explicitly speculate here on why such a recalibration might be difficult due to the small gradients of the quantities of interest. Further, one could speculate that when flow patterns (due to stability and surface coverage) and wind directions change, another recalibration might be possible. An automated algorithm might be beneficial for such scenarios. But at this point we can only speculate and as stated before do not know what is done in practice.

Line 320 The results of these simulations are not shown anywhere else in the paper. You should include these results in your "Results" section before discussing them in your "Discussion" section.
We have included the results of these simulations now also in the results section.

Line 334 f. And how much would that be if better (or any kind of) calibration is performed first?
As we have shown before specifically for the case C0/C1, if we would retune the controller (including adjusting pitch) on top of the hill to maximum performance, the difference to our original tuning would be 2.3 % (20.6 %, losses with controller relative to flat terrain, - 18.3 %, difference between maximum on top of hill and flat terrain). For all other cases, we would also need to simulate the C_P-λ-θ surfaces for a proper retuning, which is computationally expensive. Therefore, we did not do this. However, we expect a similar range of values for these cases.

Line 353 I agree with the limitations discussed here, but I would argue the study has many other limitations too. Just to name a few: 1) simplifying the turbine as and actuator disk 2) Running just one simple type of "baseline" controller 3) The fact that the wind is always flowing perpendicular to the hill
We have added these aspects to the limitations section.

Line 364 a non-calibrated torque-based controller
We have modified the respective sentence (l. 484 in revised manuscript).

Line 366 f. This is a disappointing conclusion, as this was exactly what I had hoped to learn from this paper
In order to improve the expectations for the article, we have adjusted the title so that it reflects the scope of this article more accurately.

Reviewer 2

Review of 'Wind turbine performance and control in complex terrain.'

This article addresses an important topic on optimal control of wind turbines in complex terrain. The authors perform RANS simulations with turbine control to investigate the effect of flow field acceleration on rotor aerodynamic performance. The subject is of interest to the wind energy community. However, there are several aspects that need to be addressed before the article can be published.

1. The results appear to be very terrain-specific. What general lessons can one draw from the study apart from the fact that turbine control is terrain dependent?
Indeed, the results are highly terrain-dependent. However, this study allows for several more general conclusions. First, it yields insight on how exactly a k-ω^2 controller operates in off-design conditions, keeping rotor quantities instead of free-stream quantities constant (Sect. 4.1.1). Further, it is shown, that this controller maintains constant tangential induction and angles of attack (Sect. 4.2.2). Last, it is shown that the usage of speed-up factors in site assessment likely yields overprediction of expected performance (Sect. 4.3).

2. How are the turbine positions chosen? I understand that it is natural to place a turbine at the hilltop, but why at the foot of the hill? If the wind speed changes by 180°, the turbine placed at the foot of the hill will be in its wake. What happens then? I think the choice of turbine locations should be justified.
The location at the foot of the hill is motivated by the intention to not only analyze a situation in

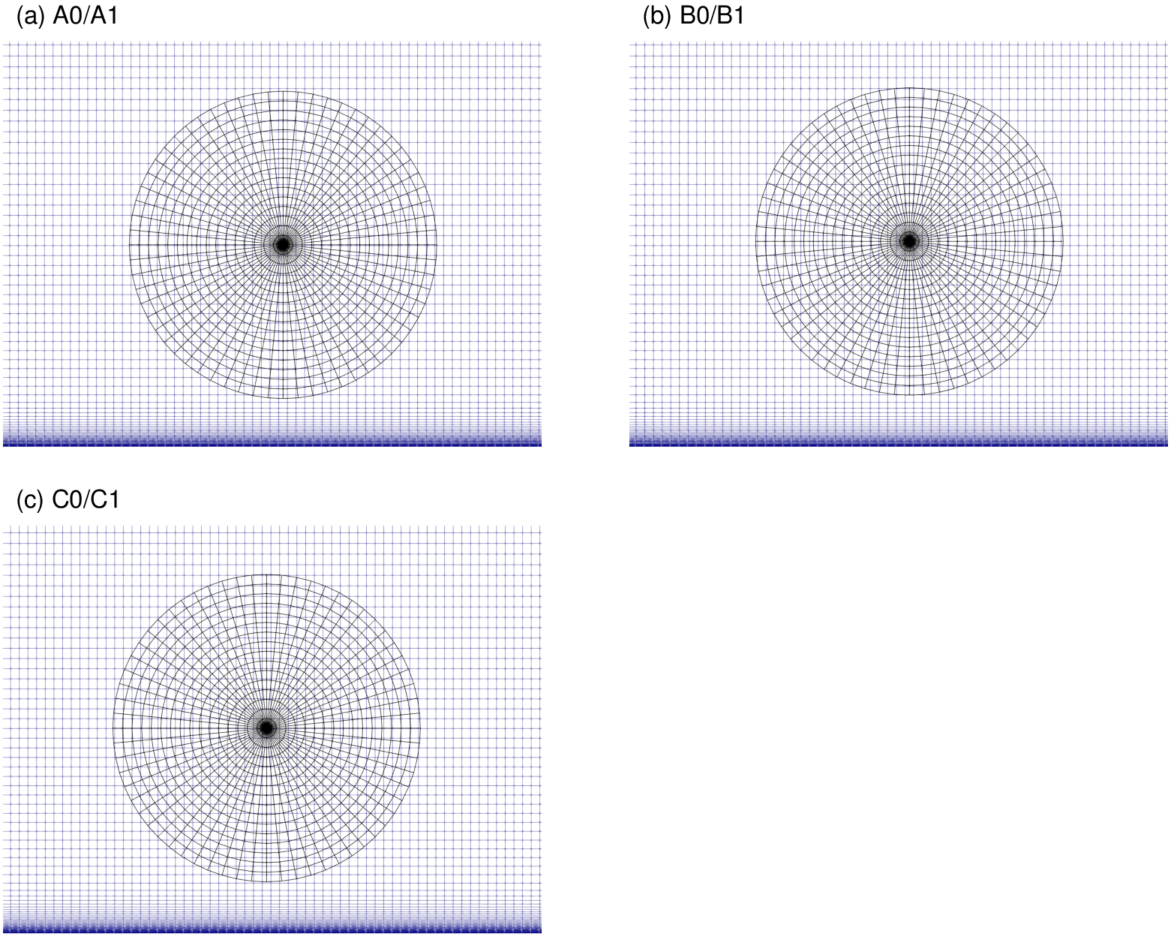

Figure 2: Close-up of the computational grids in the actuator disk region in the y-z plane for **(a)** the flat terrain simulations, **(b)** the simulations of a turbine ahead of the hill, and **(c)** the simulations of the turbine on the hill.

which a wind turbine underperforms, but also a situation in which it overperforms relative to flat terrain. At the foot of the hill, the flow accelerates, leading to the desired overperformance. We admit that this location is unlikely to be chosen in practice because, as stated, when the wind direction changes, the inflow will change significantly, likely leading to lower performance. Thus, it is rather an academic case that solidifies the conclusions drawn in this work. We have added an explanation also to the manuscript (l. 88 f.).

3. While discussing Figure 3(a), the authors mention that the turbine at the foot of the hill produces 'slightly more' power than that in flat terrain. However, visually, I cannot see any difference between the two curves. If indeed there is a very small improvement, is it higher than any uncertainty in the quantification of power from simulations?
The difference in power mentioned in the context of Fig. 3 (a) of the original manuscript is more clearly visible in Fig. 3 (b) of the original manuscript or Fig. 6 (a) of the revised manuscript, where the power coefficient is visualized. Besides the grid-refinement study, we have not conducted an uncertainty assessment. However, in all cases, the computational grids in the actuator disk region are very similar, as seen in Fig. 2. We would therefore argue that any error in the power quantification is a systematic error that should exist to a similar extent in all results and therefore cancels out when performing relative comparisons. This argument is supported by Fig. 3 showing the relative change of the ratio of reference wind speed ahead or on top of the hill to the one in flat terrain in (a) and the respective ratios of power coefficients in (b) when varying the cell size.

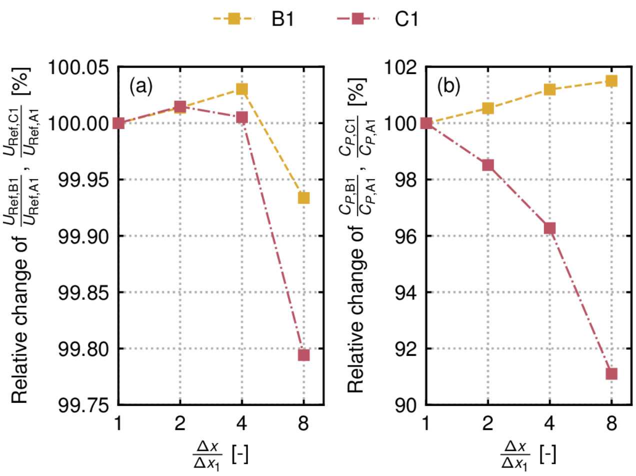

Figure 3: Relative change of the ratio between reference wind speeds ahead of the hill or on top of the hill and the wind speed in flat terrain for varying the cell size in **(a)** and respective relative change in power coefficient in **(b)**. Note that the results for the power coefficient include the controller.

By increasing the cell side length by a factor of two, the reference wind speed ratio changes by less than 0.02 %. The power coefficient ratio varies in this case by around 0.5 % ahead of the hill and by 1.5 % on top of it. It is important to keep in mind here that the simulations also include a controller, which is an additional source of uncertainty. Only the reference wind speed, which is evaluated on the actuator disk grid, is obtained without a controller. Therefore, the reference wind speed shows the actual grid dependency of our method without additional uncertainty sources due to the controller. A simulation without a controller, where for a fixed rotor speed and pitch, the power is evaluated, likely has a similar convergence behavior as the reference wind speed evaluation. Fig. 6 in the revised manuscript further shows that the C_P-λ curve ahead of the hill, obtained without a controller, also exhibits higher performance. The simulation with the controller settles on this curve.

4. The authors discuss in detail what leads to the sub-optimal performance of turbines on a hill. However, they do not demonstrate any control strategy that could lead to a better turbine performance in a non-uniform background flow.
We deliberately decided not to develop a control strategy to improve turbine performance in non-uniform background flow. It is, in our view, outside the scope of this work, which aims to improve understanding of a control paradigm widely used in research (generator torque control in region 2 of the power curve). The work was motivated by the question of how performance results in complex terrain need to be interpreted with respect to the chosen control algorithm, which is addressed here for steady-state simulations. While it would indeed be of interest how one can optimize power capture in such scenarios (control algorithms which seek optimal power performance already exist, see e.g. Creaby et al. (2009)), we believe there is more value in performing such an analysis in a transient environment and therefore leave this question to a future investigation. In the manuscript, we also argue that a simple re-tuning of the control constants would yield a better performance.

5. There are many grammatical and typographical errors, especially in the abstract and introduction. In general, the writing quality can be improved.
We carefully proofread the entire article with emphasis on the abstract and introduction, and corrected potential grammatical and typographical errors. If the quality is insufficient, we kindly ask for specific points that need to be corrected and or improved.

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