Dear reviewer,

We sincerely thank you for your time and effort in reviewing this manuscript. We appreciate your valuable feedback that has helped us improve the clarity and overall quality of the manuscript. Below you will find your original document. We have provided point-by-point responses directly beneath each of your **comments**, covering both major and minor points.

Sincerely,

Kine Solbakke, Eirik Mikal Samuelsen and Yngve Birkelund

Referee #2

Introduction

This manuscript examines hub-height wind speed and power production data from two wind farms located in moderately complex terrain in Norway, and documents the link between lee-side wind acceleration and environmental properties supportive of mountain wave propagation. Some evidence of the match between observations and simplified linear mountain wave theory is presented: for instance, lee-side acceleration is often observed when the dimensionless mountain height NH/U exceeds one. This is hardly a novelty from a mountain meteorology or mesoscale meteorology standpoint, but I might agree that the implications for wind energy harvesting went unnoticed so far. The manuscript also evaluates the ability of state-of-the-art hindcast simulations, performed with the WRF model at 750 m grid spacing, to resolve the lee-side acceleration. The model apparently captures the observed wind variability, but not entirely. For instance, lee-side acceleration in the simulations is less pronounced than in reality. The accuracy of numerical simulations is evaluated over a long hindcast run (about 4 months), and two day-long case studies are also presented.

Recommendation

The study contains a good initial review of literature on mountain waves (lines 27-48), which however contains some imprecise statements and misunderstandings, and lacks some fundamental concepts; accordingly, results based on the dimensionless mountain height NH/U and on the Scorer parameter are sometimes inappropriately discussed (see comments 1-8). The analysis of wind speeds and power production data is convincing, although some aspects could be improved (see comment 9). The global analysis of the accuracy of WRF simulations is thin (Figure 6b, Table 2), but the contents are seemingly solid. The purpose of the two case studies is not explained anywhere; they don’t really add much to the scientific content; most importantly, some statements in the case study discussions are gross misinterpretations (see comments 8-9). Finally, some technical aspects of the simulations are debatable (see comments 11-14). All considered, I would recommend requesting major revisions before considering publication.

Major Comments

1. **NH/U is an important quantity, but in a specific context: it serves as a nonlinearity parameter for linearly stratified flow with uniform wind speed (constant U and N). Yes, the critical value above which upstream blocking and leeside acceleration are expected is of order 1, and somewhat dependent on the geometry of the orography; but this concept only applies to flows that match reasonably well the constant N-constant U assumption. Real-world flows over mountains display extreme horizontal and vertical variability of wind speed, direction and stratification, so NH/U is generally not such a good predictor as one might expect**.

   Thank you for this valuable feedback on this point. Our approach is based on idealized models where N and U are constant with height, and we will add a section to the manuscript to clarify this for the readers. We will also clarify for the readers the method we have used to approximate U and N to a constant value with height. Despite limitations of the concept of the nondimensional mountain height, our result suggests that H may still be a useful indicator of the behavior of a flow encountering a mountain. The following section is added to our revised manuscript (line 168-179):

   The theory of the non-dimensional mountain height was developed primarily for idealized flows, in which U and N are constant with height. Real-world flows will typically display vertical variability in both wind speed, wind direction and stratification, consequently, U and N must be approximated as constant with height(Reinecke and Durran, 2008). Despite the limitations of the concept, $\hat{H}$ is widely used to indicate real world flow behavior (e.g. Overland and Bond (1995); Jiang et al.(2005); Mobbs et al. (2005)). In this study the Brunt-Vaisala frequency is estimated by the following bulk method:

$$N = \sqrt{\frac{g}{\theta}\frac{\Delta\theta}{\Delta z}}$$

   where θ is the potential temperature. It is assumed that the airflow that interacts directly with the mountain is spanning from the surface and up to a height of about 1 km, and $\Delta\theta$ and Δz are therefore calculated between the lowest ERA5 *model level (about 310 m asl) and model level number 120 (varying between 930-1000 m asl). The potential temperature θ in Eq. 3 and the tangential wind speed U in Eq. 2 are calculated using model level number 127. With heights varying between 553-586 m asl, the model level number 127 is the level closest to the real height of the mountain of wind park A. The mountain height h0 is set to be 550 m according to the real terrain within the A2-turbine cluster (Table 1).*

2. **Whenever a low-level inversion is present, concepts based on shallow-water flow are usually a lot more useful than internal gravity wave theory with constant N-constant U. Sometimes, downslope windstorms are caused by the transition from subcritical to supercritical conditions in "hydraulic" flow, capped by a strong inversion which acts as a density discontinuity. Here, sub- and super-critical refer to the shallow-water Froude number, Fr=U/sqrt(g'H). See for instance Durran (1990).**

   We appreciate the reviewer's suggestion regarding the use of shallow-water flow concepts in the presence of a low-level inversion. Shallow-water models and hydraulic theory may indeed provide insight into the relationship between upstream weather conditions and the observed accelerated winds at the A3 wind turbines. For instance Mobbs2005, found a correlation between both, the Froude number (shallow water theory) and nondimensional mountain height (internal gravity wave theory). In our research, metrics based on the internal gravity wave theory have provided encouraging insights, and in future research we would suggest to include metrics based on shallow water theory as well.

   In the introduction of our revised manuscript (line 48-51), we have included the following:

   "A third theory describing the mechanism of downslope windstorms is based on hydraulic theory and shallow water equations (Jackson et al., 2013). Downslope windstorms occur when subcritical upstream flow becomes supercritical over the mountain crest and accelerates down the lee slope. Further downstream, the energy is dissipated in a hydraulic jump where the subcritical conditions is restored."

   We have also added the following to Sect. 3.1 (line 287):

   "Future work should address additional metrics, such as the Froude number, developed based on hydraulic theory."

3. **Although the definition of NH/U seems straightforward, this parameter is not easily evaluated. See Reinecke and Durran (2008).**

Thank you for this constructive comment, which has led to improvements in the manuscript. We have added the following paragraph to Sect. 3.1 in the revised manuscript (line 260-265).

"Although the result of this study indicates a relationship between $\hat{H}$ and the downslope winds at A3, the results should be interpreted with caution. In particular, the approximation of a constant Brunt-Vaisala frequency is not straightforward, as demonstrated by Reinecke and Durran (2008). Reinecke and Durran (2008) applied two common approximations for N, referred to as the averaging method and the bulk method, to multiple cases with vertically nonuniform stability. Neither of the two methods provided a single best method to estimate a constant N. However, compared to the averaging method, the bulk method provided a better prediction of whether a flow will pass over the mountain or around it."

4. **Another relevant aspect totally ignored in this manuscript is that, besides mountain height, mountain width matters too. In analogy to NH/U, it is possible to define a dimensionless mountain width NL/U, where L is the mountain half-width. Mountain wave response is known to be affected by NL/U as much as by NH/U (see for instance Sachsperger et al 2015). NL/U serves as a hydrostaticity parameter. By ignoring NL/U, this study implicitly assumes that waves always propagate nearly hydrostatically. This might not be true in general. Although, indeed, strongly stratified flow over a broad mountain is likely hydrostatic. The reason why NL/U and hydrostaticity are important is that, for sufficiently small NL/U, the maximum wind speed in cross-mountain flow occurs at mountain top, not on the lee slope.**

Thank you for this helpful suggestion. We have now calculated both the vertical aspect ratio and the hydrostatic parameter and added this information to our revised manuscript.

As the results shown in Sachperger et al. 2015, breaking of vertically propagating mountain waves typically occur when NH/U ≈ 1. Sachperger et al. 2015 shows that the formation of breaking mountain waves also depends on the vertical aspect ratio of the mountain. In our case, assuming the mountain of wind park A is about 12 km in SE-direction at the base, gives an L of about 6 km. With a height of 550 meters, the aspect ratio is about 0,09.

The hydrostatic parameter for all of the selected SE-events in our study, with the lowest value being about 4 (mean value about 19). This means that hydrostatic wave formation will be important in all SE-events.

We have added the following to our manuscript:

Method section line 180-184:

"Additional non-dimensional parameters describing mountain wave characteristics are the vertical aspect ratio h0/L, and the hydrostaticity parameter NL/U where L is the half-width of the mountain. These are metrics describing whether the wave developing will be within the hydrostatic or non-hydrostatic regime. Mountain waves only exist when NL/U > 1. For NL/U >> 1 vertically propagating waves dominate with minimal wave motion downwind Jackson et al. (2013)."

The results are presented in Sect. 31 line 245-252:

"Sachsperger et al. (2016) found similar $\hat{H}$ -values for wave breaking in idealised flow over an obstacle. However, at what $\hat{H}$ -value the wave breaking first occurred also depended on the vertical aspect ratio. For aspect ratios of 0.1 and 0.05, wave breaking occurred when $\hat{H}$ = 1. For vertical aspect ratios outside of this range, wave breaking did not occur before $\hat{H}$ = 1.25 (Sachsperger et al., 2016). Along the SE-direction, the mountain of wind park A is approximately 12 km wide at sea level, corresponding to a half width of 6 km and a vertical aspect ratio of about 0.09. The hydrostaticity parameter, given by NL/U, is estimated to be above 4 for all SE-events, and hence within the hydrostatic regime where waves propagate nearly hydrostatically."

Please take note of other changes in Sect. 3.1 in the revised manuscript. In response to the other reviewer's request, we have conducted additional sensitivity analysis. The results and the corresponding discussion have been added to this section.

5. **Similarly to NH/U, the Scorer parameter is an important parameter in a specific context. It is relevant for the description of wave trapping, i.e., horizontal propagation of resonant gravity waves within a wave duct. The Scorer parameter is really useful if one wants to show that variable stratification, wind shear, or wind curvature, lead to lee wave trapping; but trapped lee waves are not discussed anywhere in this manuscript! Herein, the Scorer parameter seems to be used only as an indicator of the presence of a mean-state critical level in the wind profile (U=0, or unbounded Scorer parameter; Fig. 7 and 10). However, wave breaking in the two case studies is visibly not caused by a mean-state critical level. All in all, there seems to be no good reason to use the Scorer parameter in this study. Note also that, if wind shear and curvature are neglected, the Scorer parameter (N/U) and the**

dimensionless mountain height (NH/U) carry essentially the same information.

We thank you for this comment. As the reviewer points out, we do not discuss lee waves, and case study 1 does not suggest wave breaking at a critical layer as the mechanism behind the accelerated downslope winds. We also agree that the Scorer parameter indicates conditions under which these wave phenomena could occur. However, the Scorer parameter also indicates whether upstream conditions occur that favor vertically propagating mountain waves and wave breaking.

When the conditions for decaying waves or trapped nonhydrostatic waves are not met, the waves are able to propagate vertically. For waves with long wavelength this may happen when the Scorer parameter is constant with height. In addition to the decreasing density with height, the vertical variations in the Scorer parameter may also modify the amplitude of the hydrostatic waves (Holton2004). When the cross-mountain wind is decreasing with height, the Scorer parameter increases with height. This allows the wave amplitude to grow and eventually break. In the case where the mean flow goes to zero, the Scorer goes to infinity , and the amplitude enhancement leads to wave breaking. This can happen, as the reviewer describes, at a critical level. But amplitude enhancement and wave breaking can also occur earlier, which is apparent in Case 1. E.g. Rögnvaldsson, et al. 2011 describes wave breaking when there is a reverse vertical windshear, which is also the case in the current study. The enhancement of the Scorer parameter shown in Figure 4 is in line with this. The Scorer parameter discussed in Case 1 and 2, as well as in relation to the mean reversed wind shear presented in Fig. 4,provides additional information about the upstream weather conditions. As the reviewer has already pointed out in comment 1 and 3, the evaluation of the non-dimensional mountain height is not straightforward, due to the fact that U and N must be approximated to constant values, while in reality they vary with height. The Scorer parameter does not require any such approximations as the parameter is developed within a framework that does not require U and N to be constant with height (Jackson2013).

6. **Lines 240-256 are rather speculative, and I would recommend reducing them. I think it is fine to say simply that real-world measurements are likely to deviate (even a lot) from expectations based on linear theory. If the environment is heterogeneous in terms of N and U, there's no reason why linear theory predictions should hold. For instance, a very stable surface layer leeward of the mountain (=strong vertical variability in N), might prevent a downslope windstorm from reaching the foothills.**

Thank you for this constructive comment. Following your suggestions and given the limitations of the non-dimensional mountain height, we have reduced our analysis. Specifically, we have removed Fig. 5b) and the lines 240-256 in the original manuscript.

In addition, we have added the following to our revised manuscript (line 284-286):

"*Although many of the events appear to be consistent with the theory, there are also several events that deviate. This is not unexpected as the concept of the non-dimensional height is developed based on linear flow with approximately uniform U and N. However, the apparent relationship between the non-dimensional mountain height and the A3 wind speeds is encouraging.*"

7. **The authors seem to interpret lee-side wind acceleration and blocking as distinct phenomena; instead, they are closely related. Dynamically, blocking is caused by a large pressure maximum upstream of the mountain, while lee-side acceleration is caused by a large pressure minimum downstream of it. In constant N-constant U flows, large pressure perturbations upstream and downstream of a mountain occur in the same conditions (NH/U>1). See for instance the introductory review by Serafin (2025).**

   Thank you for pointing out this relation. Unfortunately, we have not been able to obtain access to Serafin (2025). Nevertheless, we have addressed this comment by drawing on other relevant literature that covers the topic, and we provide our response on that basis.

   It is correct that these two phenomena are closely related, and they may occur separately or simultaneously under similar value of Nh/U. This is nicely summed up in Baines & Smith (1993). According to theory of uniformly stratified flow past a three-dimensional obstacle, as the non-dimensional mountain height is increasing, two regions of stagnation may occur. One stagnation region forms above and downstream of the mountain and is associated with wave breaking. The other stagnation region that may form is close to the surface on the upstream side of the mountain. The second stagnation region is associated with an adverse pressure gradient and reduced wind speeds. Below this stagnation point, the flow is diverted nearly horizontally around the mountain, resulting in a wake formation on the lee side with low wind speeds and eddy formation due to the upstream blocking. Which of those two stagnation points occur first, depends on the obstacle shape. Baines & Smith (1993) found that for broader obstacles, the stagnation first occurred for nondimensional mountain of 1.05, at a height equal to z = h/2. As Nh/U increases, the stagnation point moves towards the top of the mountain. It is suggested that the upstream stagnation point reduces the effective height of the mountain and hence reduces the amplitude of the vertical wave. Full blocking, i.e. stagnation at a height equal to the height of the obstacle, occurred for very high values of Nh/U. In our results we can see tendencies of reduction in the effective mountain height as the A3/A2 wind speed decreases as Nh/U increases above 1.5. Case study 2 suggests that the upstream stagnation point is still below the mountain top and suggests partial upstream blocking of the flow.

We have gone through the manuscript to make sure our language is more precise. We have also toned down the wording in places, rather than categorical suggesting upstream blocking, we now simply describes it as lower wind speeds at A3 compared to A2. More substantial changes are made in for Case 2, in Sect. 3.4.2 in the revised manuscript.

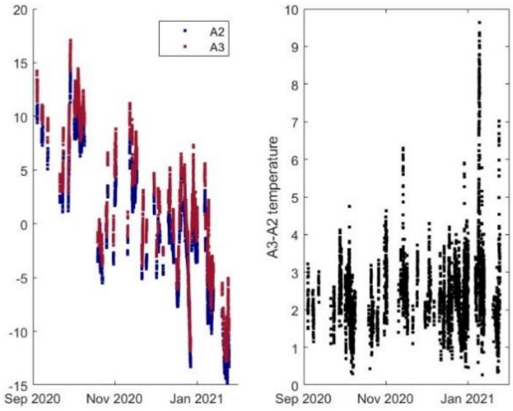
We have also added the following to the introduction line 93- 96

"Combined with pre-development wind measurements, $\hat{H}$ would indicate whether winds are more likely to be diverted around the mountain, resulting in a wake formation on the lee side with low wind speeds and eddy formation due to the upstream blocking, or if the flow is more likely to pass over the mountain (Baines and Smith 1993)."

8. **Line 278 ("low wind speeds at A3 can be due to blocking of the air flow") and the whole discussion in Sec. 3.3.2: Orographic blocking occurs upstream of a mountain, not downstream of it. More likely reasons for low wind speeds on the lee side are the presence of a shallow and very stable surface layer (which would impede the penetration of downslope winds down to the level of A3 turbines); or wake effects such as in atmospheric rotors. Superficially, the spatial distribution of wind speed in Fig 12 is reminiscent of the flow structure often observed in Bora flow, with weak winds leeward of the highest mountains. See for instance Fig 11 in Gohm et al 2008 or Fig 12 in Gohm and Mayr 2005.**

   Thank you for bringing more attention to case 2. We do agree that there might be several reasons that can explain the lower wind speed at A3 compared to A2 in this case. The atmospheric rotors mentioned might be more likely than a very stable surface layer in our cases. The left figure below shows the observed temperature at A3 (red) and A2 (blue) during the study period, and the right figure shows the difference between the temperature at A3 and A2. The figures show that the temperature is always higher at A3 than at A2, indicating that there are no cases with low-level strong inversion at A3. In addition, the ocean is ice-free and typically warmer than the atmosphere in this area during the winter season.

We cannot say for sure which effect is causing the relative low wind speeds at A3, however, the wind speeds are generally low across the mountain in this event, and no enhanced wind speeds are observed at any of the turbines located at the mountain leeside. In addition, the wind turbines extract energy from the flow and may reduce the downslope wind acceleration.



In the revised manuscript we have added the following to the discussion of Fig. 13 (Fig. 12 in previous draft) line 503-509 and line 518-524:

« ......*a large blue area with wind speeds close to zero. The weak winds in this area are interpreted to be a result of partial upstream blocking. Partial upstream blocking occur when a stagnation point develops on the windward side of the mountain and is described in Baines and Smith (1993). Below this point, the wind is diverted horizontally around the mountain, resulting in a lee-side wake with eddy formation. The air above the stagnation point will pass over the mountain, however, the efficient height of the mountain will be reduced. An alternative explanation is that the large blue area of weak winds results from atmospheric rotor formation downstream of a downslope windstorm (Mobbs et al., 2005). However, the absence of accelerated winds at the leeward turbines suggest otherwise..*"

« .... *However, the contour lines are only slightly compressed on the lee side of the mountain, and the A3 winds are weaker than the winds at A2. As noted above, one possible explanation of the weak winds at A3 may be partial upstream blocking, another less likely reason is the presence of atmospheric rotors. Another explanation may be that a shallow and very stable surface layer prevents the downslope winds from reaching A3. However, the observed temperatures at the A3 turbines are consistently higher than at A2 throughout the entire study period (not shown), suggesting the absence of low-level inversions. In addition, a low-level inversion is also unlikely given the ice-free oceans are typically warmer than the atmosphere in the winter season. Furthermore, energy extracted from* *the airflow by the turbines may reduce any potential accelerations of the downslope winds. »*

9. **Please describe the rationale for the choice of the case studies. It is really not clear. It looks like the dynamics are understandable based on simple theory in case 1, while they are not in case 2.**

   We appreciate the reviewer's comments and acknowledge that the reasoning behind the two case studies was not sufficiently explained in the manuscript. We have included case 1 to show the typical case with high wind speeds and stronger winds at A3 compared to A2. This represents the main situation at the wind park during SE events. In addition, we have included case 2 where we have the less frequent opposite situation with lower wind speeds in general and a lower wind at A3 compared to A2. We have added the following to the manuscript (line 420-424):

   "The following analysis investigates two different events: Case 1 represents a typical SE-event, with stronger winds at A3 compared to A2. Case 2 represent the less frequent event, with weaker wind at A3 compared to A2. The analysis is motivated by the observations showing that under comparable wind directions, the wind speeds and power production at A3 relative to A2, can vary substantially."

10. **Figure 5 represents only data points from SE wind events. One might still argue that wind speeds at A3 could be higher than at A2 or A1 for reasons unrelated to leeside acceleration; so the normalized wind speeds A3/A2 and A3/A1 could be >1 even if the wind at P blows from directions other than SE. Does the distribution of normalized wind speed look substantially different for other directions? Could one check similar ratios for NW flow? I think the information could be easily added to this Figure**.

Thank you for this constructive comment. We do agree that it would be interesting to evaluate if there exists a similar relationship when the wind comes from the NW. However, the main wind direction in the wind park is from the SE (Solbakken et al. 2021 Figure 1). The SE wind direction is especially pronounced during the winter months, and because this study covers the period from September to January, the NW wind direction (300° -345°) is observed for less than 5% of the study period.

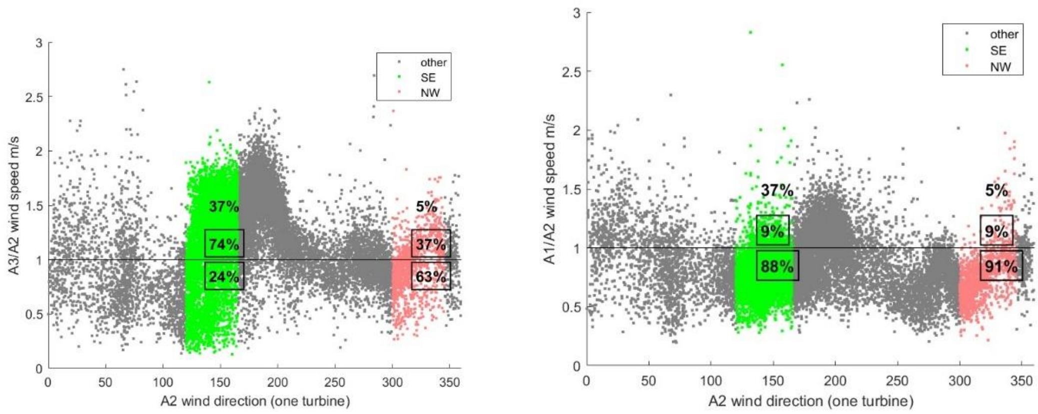
To clarify, all wind speeds presented in Figure 5 are times selected based on the wind direction within the wind park, i.e. every green dot corresponds to a time stamp when the observed wind direction at 40 turbines or more are within the selected SE-sector.

During the selected SE-events, the ERA5 wind direction at location P may in fact not be from the SE. However, and not unexpected, the wind direction at P is typically within the SE-sector during the events studied.

At this high latitude location, the sun is either low or below the horizon during the study period, so thermally driven winds are unlikely to develop. In addition, due to low incoming radiation during the winter months, flow from SE will typically be stably stratified. Air flow from NW during winter are less likely to be stably stratified due to the relative high sea temperature along the coast with relatively colder air above leading to unstable conditions.

The terrain in the wind park is gently undulating with rocks and very low to no vegetation. There are no larger trees. In addition, to reduce the impact on small local terrain effects, we have taken the mean of three turbines within each cluster. We therefore do not think that local friction effects are the reason behind the lower wind speed at A2 than A3.

The left figure below shows the A3/A2-wind speed and the wind direction (at A2). The green dots represent the wind from SE, and the red dots represent the wind from the opposite direction. As can be seen in the figure, during the study period the wind comes substantially more often from SE (37%) than from NW (5%). The wind from SE is typically higher (74%) at A3 than at A2 (24%), while from NW, the wind is typically lower at A3 (63%). The figure to the right is a similar figure, however for the A1/A2 wind speed. As can be seen, when the wind flows from both NW and SE, the wind at A2 is typically higher than at A1.



11. Line 137: ERA5 profiles at point P. Judging from Fig. 3, point P is well within the highest-resolution WRF simulation domain D03. If the purpose is to evaluate the ability of WRF to reproduce downslope windstorms and their impact on power production, I really do not understand the reason why the

properties of the upstream environment are drawn from ERA5 instead of WRF. Leeside flow properties in WRF simulations should be more closely linked to upstream profiles from the same simulations, than to ERA5 upstream profiles (a different model, with much coarser spatitial resolution).

As described in the manuscript, the relationship between the observed A3 wind speed and the upstream weather conditions, presented in Fig. 5, is based on the ERA5 reanalysis. One reason for using the ERA5 data at location P, with a low resolution, as opposed to the high-resolution WRF simulations, is that the ERA5 data provides a mean state of the upstream weather conditions free of local terrain effects. In reality, the complex terrain may modify the weather conditions at location P in such a way that it is not always representative of the upstream weather conditions of the wind park. This will also be the case if location P is positioned closer to the wind park. The high-resolution WRF simulations will be able to capture some of these smaller scales local effects and hence differ from the larger scale mean weather conditions. We could also have used the D01 or D02 WRF simulations, however for the purpose of this study, we do think it is important to minimize any small-scale-terrain induced disturbances that may be present also in these domains. This is addressed in the manuscript line 255-257,

> "One reason the results appear to agree reasonably well with the theory may be that the lower-resolution ERA5 data, as opposed to local observations or high-resolution WRF simulations, provide a mean state of the atmosphere free of local terrain effects at location P."

In addition, this study has three objectives:

> 1) To evaluate the relationship between the upstream weather conditions and the accelerated wind speeds at A3. This is done by use of observations within the wind park, and ERA5 data extracted at location P. (Sect. 3.1)
>
> 2) To document the presence of mountain waves/downslope windstorms and how it impacts the wind parks in the study (Sect. 3.2)
>
> 3) To evaluate the ability of the WRF model to reproduce the observed wind patterns in the wind park during SE-winds. (Sect 3.3)

The evaluation of the relationship between the Ĥ, based on ERA5, and the normalized A3 wind speeds is not meant to contribute to the discussion of the ability of the WRF model to reproduce downslope windstorms.

Rather, the purpose of the evaluation of the relationship between Ĥ and the A3-wind speed is to investigate if Ĥ can be a useful parameter both for planning purposes for a wind park and for power prediction purposes. ERA5 represents a readily available dataset that to a low cost can provide wind park developers with upstream weather information.

To make our objective and motivation clearer to the readers, we have added the following text to the introduction of the revised manuscript (line 92-96):

"This study evaluates whether a similar relationship exists between Ĥ and the accelerated downslope winds observed within the wind park. If such a relationship exists, the non-dimensional metric could be valuable to the wind-energy community. Combined with pre-development wind measurements, Ĥ would indicate whether winds are more likely to be diverted around the mountain, resulting in a wake formation on the lee side with low wind speeds and eddy formation due to the upstream blocking, or if the flow is more likely to pass over the mountain (Baines and Smith (1993)). In the operation phase, Ĥ could indicate whether to expect enhanced power production on the mountain leeside."

12. **Judging from Fig. 1, the area of the two wind parks is about 10 km across. At 750 m resolution, this means about 13 grid points. This is marginally adequate to resolve atmospheric variability within the area, especially considering that the effective resolution of a NWP model is usually at least 7dx. This means that all variability at shorter scales is severely damped by the dynamical core of the model, in order to preserve numerical stability (see e.g. Skamarock 2004). This likely explains why the simulations presented here do not resolve much the of observed variability in the wind.**

We appreciate the observations made by the reviewer and can confirm that the model is configured with a horizontal resolution of 700m, corresponding to an efficient resolution of ~4.9 km. We acknowledge that a higher horizontal resolution could have been beneficial for the results. However, the Fitch wind farm parameterization scheme is recommended to be used in combination with a horizontal resolution no smaller than 5 rotor diameters. In our case the 4.2 MW turbines have a rotor length of 130 m, and hence the grid size should be no smaller than 650 m. We have addressed this point in our revised manuscript, and line 393-400 now includes the following sentences:

"For the purpose of the current study, the horizontal resolution is carefully selected to balance the benefits of higher details with the constraints imposed by the Fitch wind farm parameterization, which recommends a grid size of at least 5 rotor diameters (Fitch et al., 2012). For this particular study, given that the mountains of the wind park are approximately 10 km across, a 700 m grid resolution, corresponding to an effective resolution of $7 \times \Delta x \approx 4.9$ *km, may not adequately resolve the mountain-induced flow perturbations. Perturbations, such as mountain waves, on scales smaller than the effective resolution are damped, and likely contributing to the model not being able to reproduce some of the observed variability in the wind (Skamarock, 2004). Future studies should evaluate the potential benefits of a finer grid resolution, though this would require reconsideration of the turbine representation."*

13. **The WRF orography is drawn from the GMTED digital elevation model. The maximum resolution of this DEM is 7.5 arc seconds (about 230 m), but I think the version available in the WRF pre-processing system is at 30 arc seconds (about 1 km). Again, this is marginally adequate.**

    Thank you for pointing this out. We confirm that the orography is drawn from the GMTED digital elevation model with 30 arc second resolution. We have revised the manuscript (line 390-392) to address this point:

    "In addition, the terrain model used to configure WRF has a resolution of 30-arcseconds and is a part of the default setup of the WRF model. However, higher-resolution terrain models are available and could potentially improve the terrain representation in D03 domain."

14. **Lines 182-184 explain that the hindcast simulations are actually 8 days long, and are connected by some kind of "interpolation". What kind of interpolation is alluded to here? Is it a weighted average between the two simulations, with weights changing over time? Please specify better. I understand that the authors do this in order to ensure that the WRF simulations do not drift away from the ERA5 analysis fields over a 4-month long simulation. However, I have never seen a solution based on interpolation before. Weighted averages of different weather forecasts are notoriously rather unphysical. The state-of-the art technique to prevent drifting is spectral nudging (see e.g. Waldron et al 1996 and Liu et al 2012). I personally find this a severe shortcoming in experiment design, but I**

concede that it only affects about 7% of simulated period (12 hours every week).

After reading your comment we realize that our method is not clearly described in the text. The linearization you refer to in your comment is a part of the post-processing and does not affect the simulations. We have included the following sentences to clarify this in the revised manuscript:

"The ERA5 global reanalysis is used as initial and boundary conditions for the simulations. The simulations are run for 8 days, with the first 12 hours considered spin-up time. The next 12 hours of the simulation period has been interpolated with the last 12 hours in the previous simulation, to allow for smooth overlap of the time series. The simulations cover the period from 4 September 2020 to 24 January 2021. The wind data are retrieved from 85 m agl by vertical interpolation of the model levels, and from the given turbine locations by horizontal bilinear interpolation between the grid points. Similar to the observations, the cluster wind values, and power production are the mean values of each parameter at the turbines within each cluster."

Minor comments

1. **Lines 34-35: Speaking of "fast propagating lee waves" is potentially very misleading. Mountain waves and lee wave fronts propagate opposite to the mean flow, and therefore they are generally stationary. Fast variations in local atmospheric properties near mountain or lee waves are most often not caused by wave "propagation", but by nonlinear wave interactions (Nance and Durran 1998), or by the transient phases (wave onset/demise) in non-stationary flow (Nance and Durran 1997, Grubisic et al 2015).**

   Thank you for this observation, we have removed this sentence from the manuscript.

2. **Line 41: More precisely, locally reversed air flow near the surface is typical of atmospheric rotors. These may or may not be connected with a hydraulic jump (e.g. Hertenstein and Kuettner 2015). Convective overturning and reversed flow connected with a hydraulic jump typically do not occur near the surface.**

   Thank you for the suggestion. We have revised the manuscript to include rotors to our description of downslope windstorms.

"In addition, the accelerated downslope winds can terminate in a hydraulic jump accompanied by rotor development, further reinforcing the turbulence and potentially lead to locally reversed air flow downstream of the lee side of the mountain (Doyle et al., 2000; Doyle and Durran, 2007; Gaberšek and Durran, 2004)."

3. **Line 60: This is strange wording. Static stability is a physical property of the atmosphere, so "various static atmospheric stabilities" sounds just as strange as "various atmospheric pressures".**

   Thank you, and we agree. We have revised the manuscript and changed the text to: *"Radunz2021 studied how a range of static-stability conditions affected power production at two wind farms in Brazil situated on a plateau."*

4. **Line 95: "Straits" instead of "straights"?**

   Thank you, this has now been corrected.

5. **Line 98, and elsewhere: please add a blank space between "m" and "s"; "ms" means milliseconds.**

   Thank you for making us aware of this error. It has been corrected in the revised manuscript.

6. **Line 117: "Winds from SE are considered". I presume this refers to hub-height winds. Wind direction changes with height, and the manuscript makes abundant use of concepts form 2D (x-z) mountain wave theory; so it would be important to make sure that the flow is reasonably 2D, that is, without large directional shear. Is this feasible, with the data at hand?**

   This is correct, and we have added "hub-height" to the sentence you are referring to. Unfortunately, we only have hub height wind and are unable to say anything about the wind shear/directional shear.

7. **Line 166: Horizontal resolutions of 10.5 km, 3.5 km, and 750 m. Is this correct? Nesting ratios in WRF must be integer numbers, preferably odd integers. 10.5/3.5 = 3, which is fine. But 3500/750 is 4.6666, which is not fine. Could you please verify?**

   Thank you for making us aware of this error. The correct grid size for domain DO3 is 700 m. This has now been corrected in the revised manuscript.

8. **Line 204: "conditions in which both mountain waves might grow and break, as well as being dissipated at a critical level". I don't understand this distinction. Mountain waves "grow" and "break" even at a critical level. Dissipation (deposition of the wave kinetic energy into the mean flow) is a consequence of breaking.**

The have revised the wording to make the text more precise:

"*This distinct signature in the upstream large scale wind profile is quite striking and indicates upstream weather conditions in which mountain waves may either grow sufficiently to break and form a so called "self-induced" critical level or propagate vertically until it breaks at the mean-state critical level.*"

9. **Line 272: "considerably" instead of "considerable".**

Fixed.

10. **Line 346: "Several factors may impact the accuracy of the model simulations". I find it awkward to begin this discussion with an impact of microphysics parameterizations. Yes, microphysical schemes might be relevant (especially if the flow is often saturated), but they are certainly not the primary factor that comes to mind. Besides the aforementioned marginally adequate resolution of the numerical grid, I report two more factors that are most likely a lot more relevant than microphysics: 1) Surface friction parameterization, e.g. Richard et al 1989; 2) Predictability; small changes in upstream conditions can cause very large deviations in leeside response, especially in nonlinear flow regimes e.g., Reinecke and Durran 2009.**

Thank you for this feedback. Based on your comment we have made several changes and additions to the discussion regarding the reasons why WRF is not able to more accurately reproduce the accelerated downslope winds. We have changed the wording in the line you refer to (379-395 in revised manuscript). Followed by a discussion regarding horizontal resolution, effective resolution and wind farm parameterization.

"Several factors may impact the accuracy of the model simulations. For instance, Reinecke and Durran (2009) found that small variations in the initial conditions led to substantial differences between forecast ensembles of downslope windstorms, including qualitatively differences in the characteristics of the upper-level wave breaking, as well as in the strength of the downslope winds. The accuracy of numerical simulations of downslope winds are also highly sensitive to the accuracy of the roughness length, land use and surface friction parameterization (Shestakova, 2021; Reinecke and Durran, 2009; Sachsperger et al., 2016)). While Rognvaldsson et al. (2011) highlights the importance of the micro-physical processes in the formation of downslope windstorms."

11. **Lines 404-421: A lot of text in this paragraph describes the graphical elements of Figure 9, and should therefore be reported in the figure caption.**

Thank you for this comment. We have now revised the section of Case 1 and moved the description of graphical elements to the caption of Fig. 9.

12. **Line 420: "This is in accordance with the Scorer parameter in point P". It is really an excellent match with theory, but the description doesn't really explain why and could be more precise. Citing from Serafin 2025: "If N and U are nearly uniform and NH=U is supercritical, wave-breaking tends to occur at altitudes between 1/2 and 3/4 of the vertical wavelength (2piU/N in the hydrostatic limit)". Neglecting the curvature term in the Scorer parameter, I roughly assume N/U=3*10^-3 (Figure 7a). This maps to a vertical wavelength of 2100 m. Therefore, wave-breaking is expected between 1000 and 1500 m, which matches Fig 9d very well.**

    Thank you for this perspective. It is interesting to see that hydraulic theory also matches the height of the wave breaking in case 1. the flow characteristics in case 1. The sentence "This is in accordance with the Scorer parameter in point P" refers to that the height of the breaking coincides with the height ranges in Fig 8a) (in the revised manuscript) where the Scorer parameter is constant and increasing between 1000-2500 m, suggesting conditions in which vertically mountain waves may grow and break.

    We have included the following in our revised manuscript:

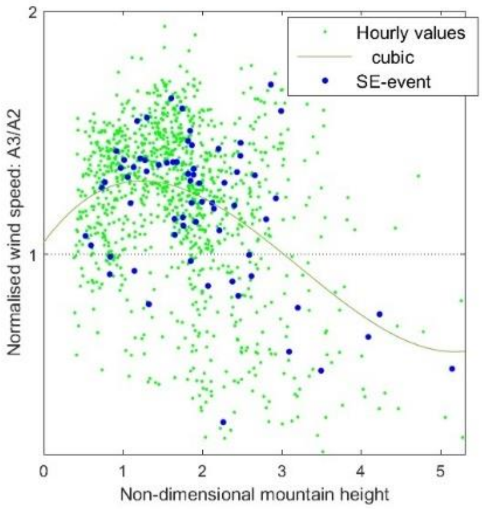
    "This is in accordance with the Scorer parameter in point P. As shown in Fig. 8, the parameter indicates conditions that favor vertically propagating mountain waves and wave breaking between 1000-2500 m asl."

13. **Line 484: "Strong relationship". I presume this statement alludes to Figure 5. It does not look like a "strong relationship". The Pearson correlation coefficient of the relationships between A3/A2, or A3/A1, and NH/U is not displayed, but is likely quite low (for good reasons, as extensively explained above).**

    Thank you for this comment. We acknowledge that "a strong relationship" may be a bit overstated and have therefore reduced the wording to "a relationship". The paragraph in the conclusion (line 537-539) now starts with the following:

    "The non-dimensional mountain height, $\hat{H}$, is a key parameter for describing the development of mountain waves. By comparing $\hat{H}$ calculated from ERA5 data retrieved at an upstream location, and normalised wind speeds at A3, a relationship is found that agrees surprisingly well with the theory of the non-dimensional height."

    Regarding the Pearson correlation coefficients, you are correct, the coefficient would likely be low, as it describes linear relationships. In this case we do not expect to find a linear relationship since for $\hat{H}$ <<1 the normalized wind speed is expected to be close to 1, increasing above 1 for $\hat{H}$ ~1, before decreasing when $\hat{H}$ increases further. We have added a version of Fig. 5 where we have included basic cubic fitting in Matlab to more clearly show this relationship:



14. Figure 1: Please add a latitude-longitude grid, to make it more comparable with Fig. 2 and 3

    Figure 1 is updated with an inserted map in the lower left corner indicating which turbines are A and which are B. In addition scales that indicate the actual distance are included in both the large map, and the inserted map.

15. Figure 2: Please add a bar scale to the map, so that actual distances can be clearly understood. I would also recommend adding a second panel with the resolved model orography.

    We have solved this with updating Figure 1 with bar scales and additional close up map of the wind park inserted into the figure.

16. **Figure 4: I would recommend showing also the variability of the profile, e.g. mean plus-minus standard deviation, or horizontal whiskers between 10th and 90th percentile. I presume the variability will be quite large.**

    We appreciate the reviewer's suggestion. The profile in Fig. 4 is indeed the mean, and as expected, there will be large variations in the profile between each event.

    We have added the following text in line 226: "It is expected that the vertical wind profile will vary between the different events."

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