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 #1

General considerations

In this paper, the authors elaborate on the impact of downslope wind storms on wind speed at hub height and power production downwind of a hill or small mountain of some 550 m height. The results are based on two (close) wind parks with a total of 67 turbines in northern Norway. From a mountain wave perspective, this is not entirely new (and also not intended to be by the authors) but from a wind power perspective, this additional aspect for site selection certainly will add added value. The problem with the paper is, that the authors do not have 'good' data (the nacelle wind speed is certainly good for operational purposes, but of course constitutes a perturbed measurement per se (one places the instrument into the perturbation that one wants to observe…). So, basically the analysis has to rely on the modeling, the essential features of which are hard to validate (what really counts is the upwind stability (no observations available), the Scorer parameter as a function of height, the upwind topography (for different flow situations), i.e., the compatibility of the flow configuration with theoretical framework, of mountain waves. So, when relying on the model simulations (or having to rely) it would be desirable to see some more sensitivity analysis rather than demonstration of the occurrence at this particular site.

I have added some suggestions (major comments 1-3) how to possibly enhance the value of the existing simulations and also a major comment on which sensitivities could possibly be explored in more detail (major comment 4). All together, since there are quite numerous detailed comments and one or the other major comment needs to be properly addressed, I call this ‘major revisions required’.

Major comment

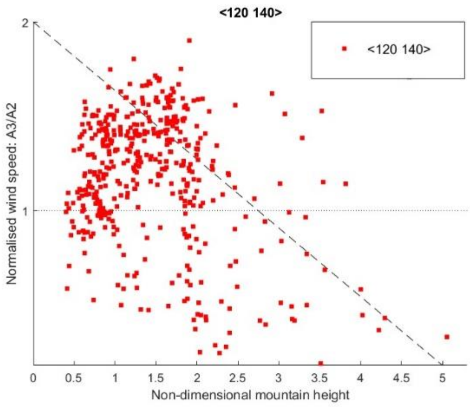
1. **The SE events are selected (which is fine in principle). The WD sector, however, consists of a range (from about 165 to 140 degrees) where indeed the approach flow rises from sea level to the 550 m high ‘hill top’ (so, non-dimensional mountain height is certainly appropriate), while for the WD range 140-120 degrees, the flow is in fact descending from a much higher mountain (cluster of peaks) to zero (only a few km horizontal distance), then rising over an even higher area. From idealized (e.g.doi:10.3390/atmos8010013) (but also real) studies we know that in this situation wave interference may play a crucial role. Did the authors consider a distinction according to wind direction within the SE sector (I perfectly realize that wind direction variability may not allow for such a fine distinction – but maybe a tendency will be visible)?**

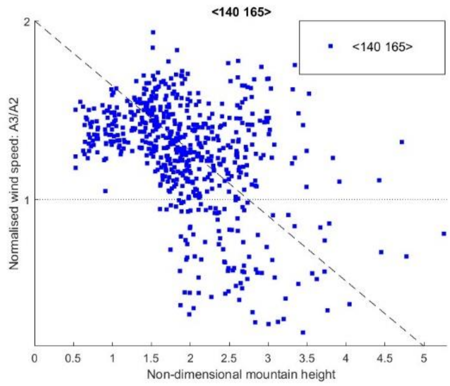
   We greatly appreciate this constructive feedback. We have now distinguished between the two sectors (120-140 degrees and 140-165 degrees) as you suggest, and the results are summed up in the figure below. The figures show a difference between the two sectors, with a better agreement with the theory for the sector 140-165 degrees (blue) than sector 120-140 degrees (red).

   We have added the figure and the following paragraph describing the sensitivity to the selection of wind direction sectors to Sect. 3.1 in our revised manuscript:

   “The sensitivity to upstream geography is tested by dividing the SE-wind sector into two smaller sectors. In the first sector (120 ◦ - 140 ◦) the nearby upstream terrain is characterized by several high mountains (Fig.1). In the second sector (140 ◦-165 ◦), the air flow approaches the wind parks rather undisturbed through a long fjord. The distinction between the two sectors is made based on observed wind directions from one of the A2-turbines. Figure 6a) shows the results for the first sector, and Fig. 6b) for the second sector. Within the first sector, there is a tendency of increasing normalized wind speeds as $\hat{H}$ increases towards one, however, similar to Fig.5, there are some deviating wind speed values for $\hat{H}$ ∼1.Within the second wind sector the relationship between $\hat{H}$ and the normalised A3 wind speed appears to agree better with the theory, with less spread in the normalised wind speeds when $\hat{H}$ ∼1, as well as no normalised wind speed values below unity for $\hat{H}$ < 1.6. Figure 6b) supports our view that one of the reasons why the rather simple theory of the non-dimensional mountain height appears to some extent hold in this real-world case, as opposed to an idealised model, may

be the long and well-defined fjord that channels the wind from location P towards the mountain."





2. **I wonder whether the argument could not be turned around, by also selecting a number of NW flows (the upwind ERA5 grid point is also available, for the Scorer parameter and non-dimensional mountain height. Wouldn't then A3 be the upwind and A1 the downwind site?**

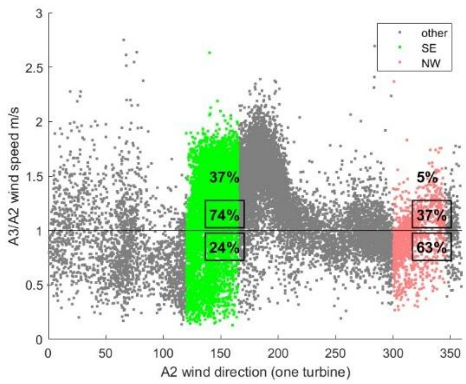
Thank you for this comment. In addition to our answer here, we would also like to refer the reviewer to our answer to the second reviewer (major comment number 10).

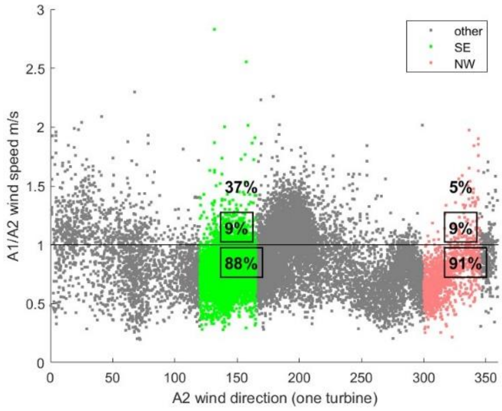
We agree that applying the non-dimensional mountain height framework to NW-winds would be interesting. As you noted, for NW flow, A3 is upwind and A1 downwind of the mountain top. Mountain waves and downslope winds may also occur under NW flows. In this scenario, the A1 wind turbines may encounter higher wind speeds and higher power production than the A3 and A2 wind turbines. However, this wind direction is not considered in this study for two main reasons:

a) The main wind direction is from SE (Solbakken et al. 2021 Figure 1). From a wind power production perspective, the SE wind direction is therefore the most relevant direction. In addition, 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 and the wind is typically weak.

b) Stably stratified flow is a prerequisite for mountain waves to form. During the winter months mountain waves are mainly expected to form when stably stratified air approaches the coast from the east and southeast. Wind approaching the coast from the sea 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, hence mountain waves are less likely to form when the airflow is from NW or these are formed in a stable layer far above the mountain top not influencing the wind pattern at the wind park in a similar manner.

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, while from NW, the wind is typically lower at A3 (63%). The figure to the right is 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.





3. **The case studies. These are presented and discussed in a way to demonstrate their point (which, by the way, is not so clearly worked out, especially for case study two…. Is the intention to demonstrate that 'it is complicated'?). However, case study 1, for example, could possibly be used to elucidate some wind direction sensitivity. At about 2 pm on the 25th (Fig. 8), the observed WD abruptly changes, but does only go to about 150 degrees, while the modelled WD also changes but reaches some 130-140 degrees). Thus, the observed flow seems to clearly come through the fjord, while the modeled flow, at least for some 3 hours, seems to be modified by the high mountains possibly interacting (also with wave activity). Maybe another cross-section (as Fig. 8d) could help to understand some of the flow behavior.**

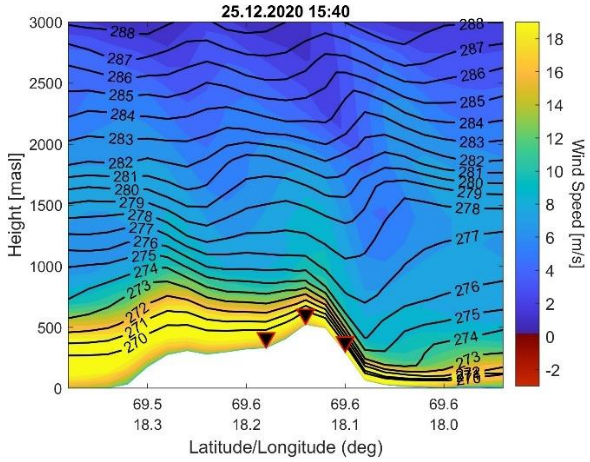
   We appreciate the reviewer's comments and acknowledge that the reasoning behind the two case studies was not sufficiently explained in the manuscript. We have included case 1 to show the typical case with high wind speeds and stronger winds at A3 compared to A2. This represents the main situation at 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 represents 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."

Regarding figure 8 and the abrupt change in wind directions. As the reviewer points out, the observed wind direction at A3 changes abruptly from 160 to about 145 degree at about 2:40 pm. At the same time a similar abrupt change in wind direction at A3 and A2 is seen in the simulations, changing from about 166 to 140. After this the observed wind direction at A3 remains below 150 degrees, while the simulated A2 and A3 wind directions (from about 4.40 pm) increase back to above 160 degrees.

While the observed wind speed remains relatively stable despite the abrupt change in wind direction, there is a large increase in wind speeds from less than 10 m/s to close to 20 m/s at all three turbine clusters. The increase in simulated wind speed coincides with the change in wind direction to 140 degrees, suggesting that the approaching simulated wind is affected by upstream wind conditions, and perhaps upstream wave formation. As expected, the cross section taken at 3.40 pm differ substantially from the cross section in Figure 11 (in the revised manuscript) at 5 am the same day. At 3.40 the upstream wind speed is substantially stronger, however, there are no indications of wave breaking leading to accelerated winds at the leeside.

Although this is an interesting discussion, we do not think the difference between the cross section below, and the one in Fig. 9 in the revised manuscript are that different that it adds any new information to case study 1. A future study, however, should compare accuracy of simulations of events where the wind direction is within the sector 120-140 degree, and the sector 140-165 degree.



4. **Sensitivity studies: more than the question, whether downslope windstorms occur (and hence must have an impact on with energy and power production), it will b of interest, how this can appropriately be modelled with a model like WRF. The authors have made a number of choices: 1) location of P1 (or: determine the background flow characteristics from an ERA5 grid point (and if so, which), or from an average of WRF grid points (and if so, which); 2) levels from which N is diagnosed; 3) depth of what I assume is meant to be the boundary layer; 4) neglection of the non-linear term in determining the Scorer parameter; 5) definition of the variability across the chosen WD sector. It is not that I would doubt the authors' actual choices (they are at least not unreasonable), but there would be considerable added value from an sensitivity analysis (how sensitive are the results on this or the other choice?).**

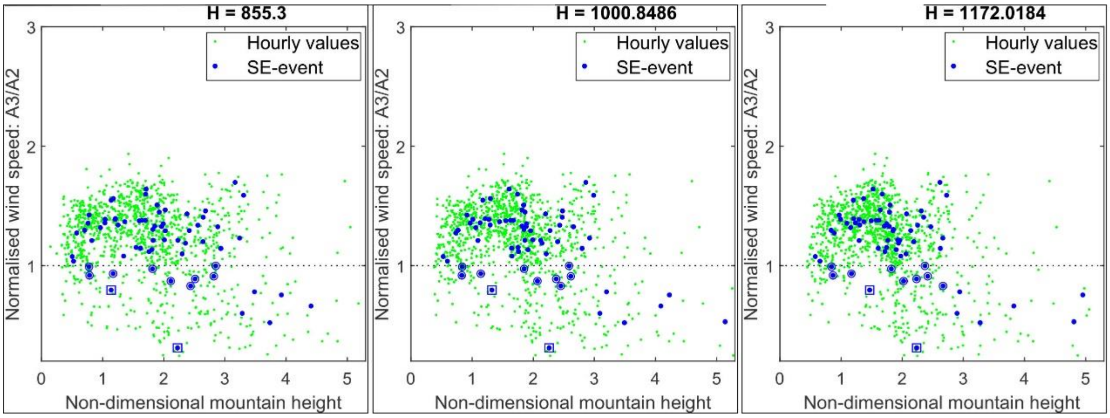
We have revised our manuscript (line 265-272) to make it clear that we have performed some sensitivity tests and that the results appear to be only weakly sensitive to the selection of vertical layers. In addition we have performed a sensitivity test for wind sectors as a response to comment number 1 above.

"To account for the uncertainty in estimating N and U, sensitivity analyses are performed. As described in Sect. 2.2, N is estimated using the bulk method in Eq. 2 between the ERA5 model levels spanning from the surface and up to a height of about 1 km. The sensitivity in the approximation of N is tested by changing the upper level to heights of about 855 m asl, and 1172 m asl. The results (not shown) appear to be only weakly sensitive to the choice of the upper vertical level used to calculate N, with the overall findings remaining consistent. U in Eq. 2 are approximated from the single ERA5 level closest to the mountain peak (about 580 m asl). The sensitivity to the approximation is evaluated by calculating U at

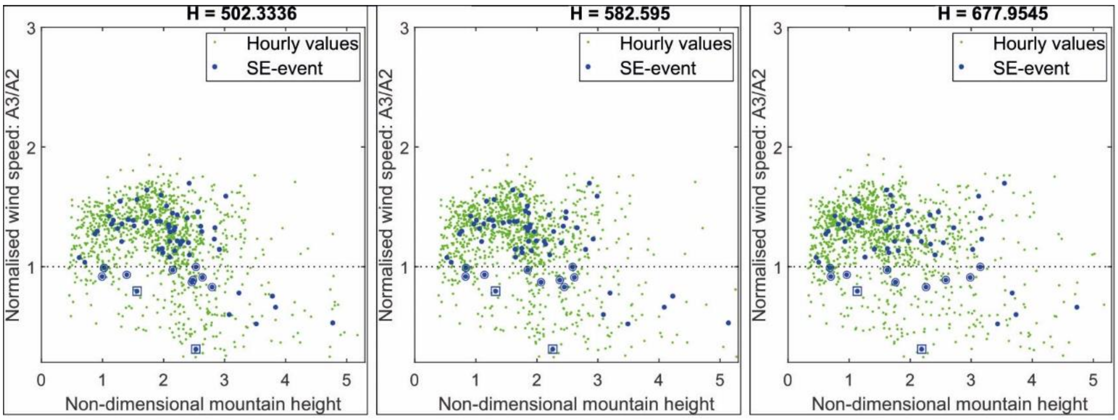
heights slightly below (500 m asl) and above (677 m asl) mountain top level. The results (not shown) are only weakly sensitive to the choice of model level."

In addition to the sensitivity testing of different selection of vertical levels presented below. Changes in h0 (mountain height) results in the $\hat{H}$ *values moving up and down on the x-axis.*

The following figures show how the value of $\hat{H}$ *varies when the upper layer of ΔN varies between the layers of approximately height of 855 m asl, 1000 m asl, and 1172 m asl.*



The following figures show how the value of $\hat{H}$ *varies when the used to calculate the tangential wind speed is varied between layers of approximately height of 502 m asl, 582 m asl, and 677 m asl.*



Detailed comments

l.88 **as follows?**

Fixed

Fig. 1 wouldn't it make sense to indicate which of the two is 'A' and which is 'B'?

Figure 1 is updated with an inserted map in the lower left corner indicating which turbines are A and which are B.

l.141 **I think the Brunt-Väisälä frequency may not be well known to the audience of this journal and should therefore be defined (including its meaning).**

We have included the Brunt-Väisälä frequency in our revised manuscript (Eq. 3).

l.144 **the equal sign should be replaced by 'approximately equal'**

Fixed

l.**155 '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': based on what is this assumption being made? Can the authors elaborate?**

We have assumed that the mountain will have an impact on the stable layer closest to the mountain top and have assumed that this layer has a depth of about 1 km based on the typical stable layer depth in the selected cases. We have updated the revised manuscript with the following text in line 176:

"It is assumed that the mountain will have an impact on the stable layer closest to the mountain top and this layer is taken to have a depth of about 1 km, based on the typical stable layer in the selected cases."

l.156 **In the EAR5 specifications (https://confluence.ecmwf.int/display/UDOC/L137+model+level+definitions) the first model level (which is labelled 137) is at 10 m – and the second (which the authors probably mean) at 31.0 m. The level closest to 1000 m (l. 157) would then be #118 (which is 'number 19' from the surface).**

Thank you for bringing this to our attention. We have corrected the information regarding what model level we have used. These are model level 120 (17) and model level 127 (10). The altitudes for each model level described in the manuscript (m asl) are correct and correspond to the table you refer to when the terrain elevation in location P (about 300 m) is subtracted.

l.158 **according to the same specification from above, the model level closest to about 550 m, would be #123, which is the 14th level from the surface**

Please see the answer above.

Fig.3, caption ‘from sea level (green) to 1500 m asl (white)‘: the figure (and the color bar‘ suggest that the color convention is the other way around….

We have revised the caption to make it clearer for the reader that both the surrounding ocean and the highest altitudes (1500 m asl) are white.

“The WRF domain configuration, D01, D02 and D03, with terrain elevations within each domain, ranging from sea level (green) to 1500 m asl (white). The ocean surrounding the land area is also white.”

l.182 **‚boundary conditions‘: what is the type of boundary conditions? The vertical (if the top level is at 50 hPa) is of particular interest. Also, is there any Rayleigh damping layer invoked, as it is usually found necessary to absorb reflection of gravity waves (e.g., Klemp et al. 2008, https://doi.org/10.1175/2008MWR2596.1)? A damping layer is quite standard the numerical investigation of mountain flows – and if the topic is mountain waves it seems particularly apropriate.**

This is correct, the upper boundary is at 50 hPa which is also stated in line 194 in the manuscript. The model is configured without Rayleigh damping, and we do acknowledge that this may be unfortunate. However, we do not expect a substantial impact on the near-surface simulation results (based on WRF best practice: WRF-dynamics-Klemp.pdf regarding Gravity-wave absorbing layer). Furthermore, we have not experienced any numerical instabilities with this model configuration. We have added this information to line 194.

l.**228 …is the dominant mechanism….**

Fixed.

l.233 ‚….remaining consistent‘: usually we add ‘(not shown‘) when citing such a finding that is not demonstrated.

Fixed

l.272 considerably higher.

Fixed

Fig 6 according to what was stated before, I assume that this is the distribution for only the SE events, right? My ‘doubt’ seems to show that it might be good to explicitly state this again.

This is correct. To clarify this for the readers we have include the following sentence in the introduction of Sect. 3.2 in our revised manuscript line 291:

"*These results include only wind and power production for the SE-events selected as described in Sect. 2.1*"

Tab 2, caption: the statistical measures have to be attributed to WRF simulations. Also Bias and MAE have units (which must be given in the title row). This would also make it clear whether they refer to the capacity factor or to wind speeds.

Thank you for this observation. We have added the units to the table.

l.283 ….'produce 51% and 21% more…': I am not familiar with the capacity factor, but these percentages seem to be based on the respective lower value (a well-known way to make your increase to look bigger). Assuming that the capacity factor is somehow based on a maximum achievable amount, I think a more appropriate way to characterize the production increase would be 25% and 13%, respectively.

We have added the following to the revised manuscript (line 312)

"*The capacity factor is a commonly used parameter to evaluate how well-sited a wind turbine is, and is defined as the ratio of the actually energy produced and the maximum possible energy output over the same period.*"

The percentage is the capacity factor of A3 divided by the capacity factor of A2 (or A1). Which really is just the energy output from A3 divided by the energy output of A2 (A1).

l.404 '….while underestimating….': wouldn't this suggest that WRF is not perfectly reproducing the waves (or the effect of the waves)?

This is correct and discussed in discussed in the paragraph comparing the wind speed distributions in Fig. 7 (revised manuscript). We have also added the following to line 357 in the revised manuscript:

"The larger negative bias at A3 suggests that the model is not fully able to reproduce the accelerated downslope winds."

l.320 at the end of this 'model evaluation paragraph', I am a little disappointed to see only 'mean biases', etc. If the claim is that the velocity differences are due to the formation of downslope windstorm conditions, wouldn't it be interesting to investigate whether the critical level has formed (we can get that from the model….) – maybe even with a distinction of different cases (e.g., wind direction sectors, see above, but also strong vs not so strong underestimation)?

We do absolutely agree that this is interesting. Based on the results we have added in Sect. 3.1 at the reviewers request, we encourage further investigations into different SE-sectors in a subsequent study, as well as the presence of critical levels.

Fig. 9 caption: '….the dotted contours….' should read 'the gray solid contours. Also: '….indicated by the dotted line in the figure c)' should read 'indicated by the full line in panel c)'.

Fixed

l.405 again the dashed grey contour lines…

Fixed

l.407 the units for wind speed are m per seconds, not meters per seconds squared

Fixed

l.413 not a dotted line….

Fixed

l.415/417 again wrong units for wind speed

Fixed

Fig.10, caption : please indicate which hours are displayed in panel a)

Fixed.

l.434 in a similar manner as ….

Fixed

l.454 winds speed units…..also l.459, l.460

Fixed

l.470 **the amplitude of what is growing? And the sentence does not seem to be complete… what is 'and upstream tilting' referring to? The amplitude is tilting upstream?**

Thank you for making us aware of this.

In the revised manuscript this is changed to *"…the amplitude of the wave is growing and the wave is propagating with a tilt upstream up to about 1500 m asl"*