Reply to comments and changes made in the paper

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The authors would like to thank the reviewers for their efforts and valuable comments. They are very much appreciated and incorporated into the revised paper.
In the present document the comments given by the reviewers are addressed consecutively. The following formatting is chosen:

- The reviewer comments are marked in blue and italic.
- The reply by the authors is in black color
- A marked-up manuscript is added. Changed sections with regard to the comments by reviewer 1 (Sarah Barber) are marked in green. Changed sections with regard to the comments by reviewer 2 (Mark Kelly) are marked in orange. The passages of the jointly requested changes in the paper have been highlighted in grey. The text passages in the paper related to the reviewer comments are marked by abbreviations and are highlighted in the marked manuscript.

Reviewer#1 comments

1. "*General comments - Scientific relevance: This is a very interesting an important topic and the level of detail is impressive. - Scientific quality: It is generally of high quality, and sometimes perhaps goes into too much detail. However, I think that the importance of the topic and the lack of similar studies warrants this level of detail. As an additional, I would just like to see an analysis or at least a discussion on how the results could actually be used by wind farm planners in the future. - Presentation quality: Sometimes the paper is difficult to follow. I hope my suggestions help improve the presentation and arguments.*"

Thank you Dr. Sarah Barber for your extremely detailed and helpful comments and especially for the time you invested in them. We have looked at all the hints and suggestions for improvement and have incorporated everything. Below you find the individual responses to the comments, each of which also led to changes in the manuscript and have been colour-coded and numbered in it.

2. "*Line 4: It's not clear what you mean by "The terrain site". Do you just mean "The site"? Please clarify this.*"

Done. See **R1: Comment 1**.

3. "*Lines 7-8 "In the first part, high-resolution CFD simulations are performed to separately investigate the effects of the forested escarpment and of thermal stratification on the flow field and on the wind turbine accordingly". I'm not sure what the word "accordingly is doing there. I suggest removing it.*"

Thank you for this hint. We removed it. Please see $\boxed{\textbf{R1: Comment 2}}$.

4. "*Line 7: Specify and quantify what you mean by "high-resolution CFD simulations" and include the CFD type, e.g. RANS, LES, etc.?*"

We have replaced the vague term "cfd" with our applied method of "Delayed Detached Eddy simulations". See $\boxed{\textbf{R1: Comment 3}}$.

5. "*Line 8: Why did you investigate thermal stratification? Is the site characterised by strongly seasonal differences? Or by unstable condiitons? Low-level jets? This should be mentioned in the sentence "The site is characterised by ...." starting on line 4.*"

According to our measurements and simulations, thermal stratification at this test site on the Swabian Alb is not particularly more significant than at other sites. However, since thermal stratification actually always occurs during the course of the day, one of the aims of this work was to reproduce it well numerically in order to be able to make predictions for all possible flow situations in the test field. We have added this succinctly. See $\boxed{\textbf{R1: Comment 4}}$.

6. "*Line 9: When you write "all the examined effects" do you mean just the two mentioned in the previous sentence. If so, just write "both these effects" instead.*"

Yes, you are right. Done, see $\boxed{\textbf{R1: Comment 5}}$.

7. "*Line 12: please quantify the influence of the effects on the flow field, i.e. the average wind speed is affected by up to x%, which could affect the AEP by x% (if you didn't estimate this in the paper, you should...see later)*"

Very good advice, which we have followed. See $\boxed{\textbf{R1: Comment 6}}$.

8. "*Line 17: "....which is important because ????"*"

We added an explanation, why this is important. See $\boxed{\textbf{R1: Comment 7}}$.

9. "*Please split this section up into sub-sections. It is difficult to follow. - Lines 22-23: This statement is too broad. One paper showed that "the time averaged speed-up and the mean inclination can in general be predicted decently", but what does that mean for wind energy in general? You cannot conclude that therefore RANS can predict wind fields well for all wind energy applications. Please discuss this.*
Line 23: define "speed-up"
Lines 24-25: you need a reference for this. And what does "sufficiently complex" mean? You might want to read and refer to https://wes.copernicus.org/preprints/wes-2021-158/. And why can't these conditions be adequetly modelled by RANS? What does "adequate" mean? Please expand on this section, because it is fundamental to your paper and why your work is important."

We have subdivided the sections as requested for better understanding.
We also corrected the broad statement about RANS methods and rewrote the paragraph for better and more in depth explanations. Also thanks for the reference, which I used for the more in depth description. See $\boxed{\textbf{R1: Comment 8}}$.

10. "*Lines 28-30: please include a reference for this statement*"

We added a reference for LES simulations in complex terrain. See $\boxed{\textbf{R1: Comment 9}}$.

11. "*Line 30: please include a reference about hybrid RANS/LES methods.*"

We also added a reference for hybrid RANS/LES methods. See $\boxed{\textbf{R1: Comment 10}}$.

12. "*Line 37: "hybrid methods or LES methods usually produce significantly better predictions" implies many studies showing this. Please refer to some of these.*"

Done. See $\boxed{\textbf{R1: Comment 11}}$.

13. "*Line 38: without significantly increasing the computational power? (otherwise why not just use LES?) Please discuss this topic here too. Line 39: please specific what "adequate" means in this context. Also, you should refer to https://wes.copernicus.org/preprints/wes-2021-158/ even though it is still a discussion paper (publication expected very soon) because DDES was actually applied to the same site in this work.*"

We addressed this topic in the manuscript an also added the reference. Please see $\boxed{\textbf{R1: Comment 12}}$.

14. "*Line 43: you mentioned "the escarpement" - which escarpement? You don't say anything about the site in Belcher et al. Is it the same site or a different one? Please clarify.*"

Very good advice. We have removed the potential misunderstandings regarding the escarpment. See $\boxed{\textbf{R1: Comment 13}}$.

15. "*Line 57: "in power generation." should come directly after "5%"*"

Thank you. Done, see $\boxed{\textbf{R1: Comment 14}}$.

16. "*Line 57: quantify what you mean by "correct" and then by "accurate" on the next line.*"

For a better understanding we have rephrased this sentence. See $\boxed{\textbf{R1: Comment 15}}$.

17. "*Line 68: "to predict the effects of complex terrain on wind turbines more accurately" would be correct English. Line 68: specify what you mean by "complex terrain" here.*"

Thank you for the correction. We also specified that we meant "flows over complex terrain" See $\boxed{\textbf{R1: Comment 16}}$.

18. "*Lines 69-70: What do you mean by "Thus, a variety of studies are conducted experimentally and numerically"? Do you mean in general, or in this work? It would be clearer if you wrote "...can be...."*"

This is also a good hint. See $\boxed{\textbf{R1: Comment 17}}$.

19. "*Line 71: coarser than what? Remember that the reader does not yet know anything about your simulation set-up. Please quantify this instead.*"

Yes, that's right. We removed the word coarser because its cunfusing in this context. See $\boxed{\textbf{R1: Comment 18}}$.

20. "*Line 72: please reference this paper too: https://wes.copernicus.org/preprints/wes-2021-158/*"

Yes, this paper fits very well into the context, which is why we have added it at the desired place. See $\boxed{\textbf{R1: Comment 19}}$.

21. "*Line 73: specify what you mean by "highly resolved" (time? spatially? both?) and quantify this.*"

We added the information that the simulations are as well spatially as temporal highly resolved. The quantification follows in section 2. See $\boxed{\textbf{R1: Comment 20}}$.

22. "*Line 74: remove "in complex terrain" - you already said that the test site is complex.*"

Done. See $\boxed{\textbf{R1: Comment 21}}$.

23. "*Line 74: "....and on its wake...."?*"

Yes, thank you for this hint. See **R1: Comment 22**.

24. "*Line 76-81: This part does not belong in the Introduction. Please move it to the set-up. Also, say where these simulations come from. Are they from the results of this study? In which case, it's confusing to show results already at the beginning. Consider removing this figure. I don't see what it's contributing to the paper.*"

Yes we agree that this doesn't really fit into an introduction. That's why we also renamed the last section to "The WindForS Test Site and Preview on the Studies" and moved the Figure there. We opine that the Figure is useful because it shows all the analyses of this paper starting from the inflow of turbulence in the valley, ending with evaluations of the wind turbine wake, and also showing the entire test site. However, we have now also presented the relevance and the informative value of this Figure more clearly in the manuscript. See **R1: Comment 23**.

25. "*Line 92: Specific what you mean by "fully resolved", i.e. how many cells over the rotor? Later on you say 1 m, but how large is the wind turbine?*"

Fully resolved means that the boundary layers of the wind turbine structures (tower, blades, hub, connectors, nacelle) are fully resolved with y^+ respectively $z^+ \approx 1$. This means that the viscous sublayer at the bottom surface is fully resolved. We made this clearer. We have also moved these explanations to the chapter "set-up", see **R1: Comment 24**.

26. "*Line 98: Why do you say the height is 2500 m here, but 2600 on line 94?*"

Oh, thank you. That's obviously a typo. We have corrected that. See **R1: Comment 25**.

27. "*Line 120: define z+ and explain why it is different from the commonly-used y+.*"

We made clearer that y^+ respectively z^+ are important parameters to determine the position of the wall nearest grid cell to ensure a good prediction of the wall bounded turbulent flow. In the initial paper we referred to this parameter as y^+. The reviewer comment was to rename this parameter to z^+ as this fits better with the vertically aligned z-axis, to which we agreed. To avoid misunderstandings, we have added a small explanation in the manuscript. See **R1: Comment 26**.

28. "*Line 126: This isn't very clear - do you mean that you just shifted the z coordinate? Or what else changed in the shift between "turbine position" and "hub position"? Please clarify.*"

Yes, we agree. We made that clearer, see **R1: Comment 27**.

29. "*"2.2 Numerical Set-up": you keep switching tenses - please be consistent (i.e. "is" and "was").*"

We changed the tenses to be consistent, see **R1: Comment 28**.

30. "*Line 130-135: Please include a table summarising the set-up.*"

Good hint. We added a table summarising the set-up. See **R1: Comment 29**.

31. "*Line 164: I find it hard to understand the results of the preliminary study (e.g. I'm not sure what "Thus, this study revealed that cells of the lowest approximately 0.5 m above-the-ground are modelled." really means). I suggest a rewording, and also a figure would help here.*"

We rephrased this sentence, because we definitely understand your concern. See **R1: Comment 30**.

32. "*Line 170: please explain briefly what the "chimera technique" is.*"

Done. See **R1: Comment 31**.

33. "*Line 180: Please specify the accuracy of the anemometers and if their set-up was done according to IEC 61400-12 or not.*"

We added the information about the accuracy. See **R1: Comment 32**.

34. "*"2.4 Forest Setup": it's not clear to me how much of this is in Letzgus et al. 2018 and how much is new (without having looked at that paper). Please make this more obvious.*"

We made that clearer. See **R1: Comment 33**.

35. "*Line 192: It's strange to refer to equation (4) here, which is so far away. I would remove this reference, and mention it later next to equation (4). - "2.5 Inflow Setup": make sure you are consistent with the use of "Mann model" or "Model-model". Also, you define "turbulence intensity" as "Ti" so make sure you use "Ti" instead of "turbulence intensity" in the rest of the paper.*"

Good advice! We have taken this into consideration. See **R1: Comment 34**.

36. "*Line 256: Don't write "his" but "their". First of all, this is gender-neutral. Second of all, it could refer to all the authors rather than just the first author.*"

Yes, that's absolutely right. We corrected that. See **R1: Comment 35**.

37. "*Lines 260-275: It's not clear if you are applying this method here for the first time or whether it has already been done. Please make this clear.*"

We made this clearer. See **R1: Comment 36**.

38. "*Line 279: Please indicate why you chose this temperature range. Probably it is a typical range for wind energy applications, but please say where this comes from.*"

We have made it clear that these are relatively typical unstable conditions. See **R1: Comment 37**.

39. "*"2.7 Governing Equations". It's not clear what this section is for. Is this summarising all the changes you made to FLOWer? Please clarify this.*"

We made that clearer. See **R1: Comment 38**.

40. "*The introduction is confusing. It's not clear if "the first part of the results" refers to section 3 only, and "the second part of the results" refers to section 4, or whether you are referring to two parts of section 3. Please reword this.*"

We added the label references to these sections. Now it is clear about which chapters we talk. See **R1: Comment 39**.

41. "*Figure 5 and Figure 6: please add a length scale to these pictures.*"

Thank you, that's a useful hint! See **R1: Comment 40**.

42. "*Line 308: "In general, trees are 20 to 35 meters tall.". Do you mean in general, or in general at this site? Please clarify. Also, where does this information come from, DSM or somewhere else?*"

We added the information that in general trees are 20 to 35 meters tall "at the test site". The heights of trees and buildings are determined by subtracting the DEM and the DSM model heights. See **R1: Comment 41**.

43. "*Line 323: The units of LAI are missing, please add.*"

Done. See **R1: Comment 42**.

44. *"Line 325: Please say why you made this assumption?"*

We added information about the assumption of these parameters. Please see **R1: Comment 43**.

45. *"Line 361: Explain why you chose to plot SD rather than TI or TKE."*

We chose SD because it indicates the absolute fluctuation in the observation period around the mean value. This representation is useful for the very different mean values of the wind speed on the test field plateau. Near the ground, the speed is very low due to the forest, and above there is an area that is strongly accelerated by the terrain. This is why SD is more illustrative than TI. This is why SD is more illustrative than TI. See **R1: Comment 44**.

46. *"Line 363: Please explain why you chose four minutes."*

For the evaluation, the respective flow fields were averaged over four minutes, which means a suitable averaging period below the micrometerological peak in the spectrum according to van der Hoven (Hoven, 1957). Further, this is a time period that is similar to subsequent evaluations of wind turbine simulations and is still appropriate for the high temporal and spatial resolution. See **R1: Comment 45**.

47. *"Figures 10-13, 16: Could all be bigger."*

We have already initially adapted these Figures for the double-sided WES format. We keep these comments in mind and will adjust the Figure size, if the Figures appear too small in the final format. Thank you for your comment.

48. *"Figure 11: Alter the legend to match the text (i.e. "Labil" should be "unstable")"*

yes, this is an error that occurred during the first review iteration. Thanks for the hint. See **R1: Comment 46**.

49. *"Line 385: Explain why at this position."*

The position was chosen because it is in the valley upstream of the escarpment. See **R1: Comment 47**.

50. *"I suggest a table to summarise the effects."*

Thank you for the advice. For reasons of space in the paper, we initially decided against an additional table. However, we will definitely keep this hint in mind for the final two-column layout of the paper.

51. *"Line 487: You mean "in the second part of the results"?"*

Yes, see **R1: Comment 48**.

52. *"Line 497: Define what you mean by "speed-up""*

Yes, we have already defined "speed-up" See **R1: Comment 8**.

53. *"I would like to see one further discussion point - how could readers use the results to assess the impact of such flow conditions on AEP and load cycles?"*

Very good hint. We added a section in the conclusions regarding this topic. See **R1: Comment 49**.

54. *"Lines 9-10: Don't combine two tenses in one sentence please, i.e. change "shows" to "showed". Also, change "wintertime" to "winter conditions" (although March isn't really winter)."*

Thank you. See, **R1: Comment 50**.

55. *"Line 22: "is" –> "can be""*

Done, see $\boxed{\textbf{R1: Comment 51}}$.

56. *"Line 23: replace "decently" with "well"."*

Done, see $\boxed{\textbf{R1: Comment 52}}$.

57. *"Line 37: remove the space after "Sogachev et al. (2012)""*

Done, see $\boxed{\textbf{R1: Comment 53}}$.

58. *"Line 41: space missing after the full-stop."*

Done, see $\boxed{\textbf{R1: Comment 54}}$.

59. *"Line 75: remove "will""*

Done. See $\boxed{\textbf{R1: Comment 55}}$.

60. *"Section 2: Change "Setup" to "Set-up" (throughout the paper)"*

Done. See $\boxed{\textbf{R1: Comment 56}}$.

61. *"Line 135: please be consistent in your use of "2-dimensional" and "2D"."*

Done. See $\boxed{\textbf{R1: Comment 57}}$.

62. *"Line 206: I don't think you defined LAI."*

Tank you for this hint. Done. See $\boxed{\textbf{R1: Comment 58}}$.

63. *"Line 234: You haven't defined ABL"*

Done. See $\boxed{\textbf{R1: Comment 59}}$.

Reviewer#2 comments

1. *"The paper looks improved, and I hope the revision process has been beneficial for the authors as well. There are several issues yet to be addressed, albeit a shorter list than previously; I list them below. Unfortunately this also includes some incorrect and inconsistent statements which have been introduced in the revision; these need to be addressed.*

Some English usage issues remain (or arose on revision); this reviewer suggests WES at the typesetting stage to also catch these. However, via annotating the v2 PDF-file (attached), I've noted some of these, particularly in the abstract/intro. Additionally, there are still some descriptive shortcomings/errors; in the PDF-file I've suggested changes for clarity/correction. Examples of this are "processes" being replaced by "simulations" on line 30, and "very comprehensive study" on line 47 being replaced by "quantitative observational study"."

Once again, thank you Mr. Kelly for the very elaborate review of our manuscript. The review was already very beneficial for us in the first iteration step and has also further improved our manuscript this time. We thank you very much for that. As in the first iteration, we have responded to the comments below and marked the corresponding revisions in the manuscript in colour. Thanks also for the efforts and the quick feedback when technical problems of the Copernicus homepage occurred. We were finally able to download your PDF file and implemented the language improvements you suggested. See $\boxed{\textbf{R2: Language}}$

"The bibliography has a few upper-case ("ALL CAPS") issues."

Thank you, that is a very good hint, which we have corrected now. See **R2: Comment 2**.

"Line 75: given the mostly qualitative findings, "more precise insights" should be replaced by something like "additional insights"."

Good hint, it sounds more scientific now. See **R2: Comment 3**.

"Line 79: λ_2 has not been defined."

We have rephrased the sentence to define the λ_2 criterion. See **R2: Comment 4**.

"Line 141: "introducing default" should be something like "setting fixed""

Done. **R2: Comment 5**.

"L.149-151 needs a reference(s); also, this implicit LES SGS model has consequences/limitations, especially in stable conditions..."

We added a reference. Yes, we are aware that the SGS model is limited for applications in stable conditions. But for the applied fifth order Weno scheme, switching-off the turbulence model has almost no effect on the result, which means that the numerical dissipation still overwhelms the physical one introduced by the SGS model because the Weno scheme already introduce numerical dissipation at high wave numbers due to the Riemann solver. We also added a reference for this statement. See **R2: Comment 6**.

"L.154-156: "vertical velocity distribution" is not correct here, and means something else than what you are trying to indicate: you are referring to the vertical profile of wind speed (or of horizontal components). This sentence needs to be reworked."

Yes, you are right. This description is not accurate. We have also reworded the sentence for better understanding. See **R2: Comment 7**.

"l.226/eq.(3): change "für" to "for""

Thank you. Done. See **R2: Comment 8**.

"l.233: the Mann-model L is not the "largest length scales", but rather related to the wavenumber (scale) at the isotropic spectral peak; see e.g. Kelly (2018) for details."

Thank you for this input. We read the paper and referenced the definitions and explanations given there in our manuscript. See **R2: Comment 9**.

"Comment 18 (now lines 238-9): scaling the dissipation rate (σ_u), i.e. spectral amplitude? How is the scaling done, i.e. can you give the equation you've used?"

The target turbulence intensity TI is used to calculate the standard deviation σ. Then we apply the χ^2-fitting analogous to the procedure described by Mann (1994) according to his equation (4.1), as also done by Chougule et al. (2016). We then verify the output values with various literature data, e.g., Pena et al. (2010) or Sathe et al. (2013). We made this clearer, see **R2: Comment 10**.

"l.241: does "a few length scales" mean 3L, or what?"

The force terms used to introduce turbulence into the field act at a distance of 2-3 length scales downstream of the inflow plane depending on the length scale of the generated turbulence. We added this statement to our manuscript. See **R2: Comment 11**.

"l.244-5: "from experience and literature values" should include references??"

Thanks for the input. We have to admit that this statement was very vaguely formulated. We have now specified this and provided references. See **R2: Comment 12**.

"l.247: how were the "turbulence intensity and the length scales were extracted"? From which height, using which observed quantities?"

Yes, that was a bit imprecisely formulated. We have now described our approach to this in more detail. The procedure is based on evaluations of the unsteady measurements of the met mast (20 Hz), application of the Taylor hypothesis, and verification with literature data such as Peña et al. (2010). The reference height for the analysis was hub height (72 m), since this is also the focus of the wind turbine evaluations. See **R2: Comment 13**.

"l.248: what does "adjusted in analogy to Chougule et al." mean? I.e., "in analogy", instead of using spectral fits plus their estimates (or which methods from Chougule et al)?"

Yes, we mean a spectral fit analogous to the χ^2 fit performed by Chougule et al. (2016) following the procedure described by equation (4.1) of Mann (1994). Subsequently, we verified the results with values given by Peña et al. (2010) We made this statement more precise. See **R2: Comment 14**.

"l.250-259: there appears to perhaps be confusion around Chougule et al.(2016); i.e., they added an extra equation (for temperature) to RDT, which resulted in two extra parameters (5 in total)."

We have referred to the procedure described in the answer of "Comment 14". We have now made this clearer. See **R2: Comment 15**.

"l.273-275: this sentence is not needed."

Ok, we removed this sentence. See **R2: Comment 16**.

"[a note regarding the previous Comment 19— To more specifically precisely (correctly) state my previous statement: as far as the Mann model goes, and the way you used the model, epsilon 'is not affected by stability'. However, overall/in nature it is; e.g. in unstable conditions there is a balance between shear production, buoyant production, turbulent transport, and dissipation (see e.g. Wyngaard's 2010 textbook or e.g. Kelly et al 2014)."

Thank you for the detailed explanations. We have looked at the descriptions in Wyngaard's textbook. These were very helpful and gave far reaching insights. We were familiar with the textbook, but we took a closer look at the relevant sections from this perspective. We appreciate this advice very much!

"l.266: how is the surface heat flux simulated, what is the surface temperature which gives rise to a flux for a given temperature field?"

Initially we define a specific wall temperature and by the profiles of the potential temperature and the vertical gradients $\frac{\partial \Theta}{\partial z}$, respectively, the heat flux is calculated/simulated. We added an explanation. See **R2: Comment 17**.

"l.267: where is it shown that the model predicts M-O profiles in the simulated ASL?"

We have chosen sheared velocity profiles to fit well with measured data (or desired target velocities for generic analyses) in the ABL at a reference altitude, and we also made sure that the profiles fit the M-O similarity theory in the ASL. Therefore we have matched our inflow profiles with shear corrections (α-Corrections) for stability. For example:

$$\alpha = [\phi(z/L)/(ln(z/z_0) - \Psi_{MO}(z - L))] \tag{1}$$

or

$$\alpha = \frac{u_*}{\kappa u} - \frac{gz\overline{w\theta}}{\theta_0 u_*^2 u} - \frac{T}{u_*^2 u/z} \tag{2}$$

Another alternative is the comparison of the sheared profile with log law M-O connections:

$$u(z) = \frac{u_*}{\kappa} ln\left(\frac{z-d}{z_0} - \Psi_{MO}\frac{z}{L_{MO}}\right) \tag{3}$$

For unstable conditions:

$$\Psi_{MO} = 2ln\left(\frac{1+x}{2}\right) + ln\left(\frac{1+x^2}{2}\right) - 2arctan(x) + \frac{\pi}{2} \tag{4}$$

with $x = (1 - 16z/L_{MO})^{\frac{1}{4}}$

For stable conditions:

$$\Psi_{MO} = \begin{cases} az/L_{MO} & \text{for } 0 \leq z/L_{MO} \leq 0,5 \\ Az/L_{MO} + B(z/L_{MO} - C/D) & \\ exp(-Dz/L_{Mo}) + BC/D & \text{for } 0.5 \leq z/L_{MO} \leq 7 \end{cases} \tag{5}$$

With $a = 5$, $A = 1$, $B = 2/3$, $C = 5$ and $D = 0,35$.

In this way, it was possible to generate inflow conditions that reach the desired reference velocities at a certain reference height and that can model the flow field in the ASL according to M-O. This could be confirmed in the comparison with the measured data in Fig. 20 (even if the flow characteristics are strongly influenced by the forest and the escarpment). We made all this clearer. See **R2: Comment 18**.

"*Comment 21 (now lines -): reference to the LAI values?*"

We have clarified that these are estimations of LAI based on the fact that we know the vegetation types of the forest and that we have already carried out several simulation campaigns e.g. Letzgus et al. (2020) and verified them with measurements. Direct measurements (destructive) of the LAI or indirect measurements via the determination of the light transmittance of the vegetation were not available and are planned in follow-up projects. See **R2: Comment 19**.

"*Comment 24 (now lines -): unfortunately your response has the form ""*

We no longer found this superfluous <"> sign. Presumably it has already been fixed. Thank you for pointing it out. We then checked the manuscript again for errors that could have been caused by the marked version.

"*l.387 (was comment 20): inversion strength of 3K per what? (what are the units?)*"

Thanks for the advice. Our specification was not very meaningful and referred to an unnamed reference height, which was not helpful. We have improved this and followed Kelly et al. (2019) by specifying the inversion magnitude or capping inversion. See **R2: Comment 20**.

"*l.389-390: the statement the absolute value of the reciprocal of the Monin-Obukhov length L-1 is proportional to the probability distribution of the particular thermal condition present. does not make sense. What was the observed L-1 measured for each stability, or did you get it from a flux-gradient relation via the dTheta dz you showed?*"

We have corrected this statement. Yes, we got the information about the Monin-Obukhov length from the flux-gradient relation via $\frac{\partial \Theta}{\partial z}$. See **R2: Comment 21**.

"*l.391-393: the these probabilities do not make sense, unless there is a value of L-1 associated with each condition.*"

yes, we agree absolutely. Without quantitative specification of the M-O length or the reciprocal L^{-1}, these probabilities are useless. We have added to these quantitative values of L^{-1}. See **R2: Comment 22**.

"Fig.11: legend in plot is not English (e.g. "Labil")"

Thanks, the error had occurred during the first revision of this diagram. See **R2: Comment 23**.

"l.399: "pressure driven buoyancy" does not make sense. What are you trying to say?"

yes, that was described in a misleading way. We are talking about a pressure driven ABL, where a buoyancy acts due to the vertical gradients of the potential temperature. We made this clearer. See **R2: Comment 24**.

"l.492: how did you choose 300m for the ABL depth in unstable conditions? Typically unstable ABL's have depths of approximately 1000km."

Yes, we are aware that this altitude of ABL was chosen a bit low for unstable conditions. Due to winter conditions, where unstable atmospheric boundary layers extend lower altitudes compared to summer conditions, we chose this altitude. Nevertheless, for the lowest 150 m of the ABL with the wind turbine as the focus of our analyses, this height will be sufficient. However, we have now added for the readers information that the choice of ABL is somewhat shallow. See **R2: Comment 25**.

"l.503-508: these values do not fit with e.g. Kelly(2018) or Mann's (1994) model. I.e., a Uref=4.8 m s-1 and Ti=15% give σ_u=0.72m/s, which is different that the value from L=42m and $\alpha\epsilon^{2/3} = 0.03m^{4/3}/s^2$."

We have chosen the turbulence to match the measured conditions on the test site in the area of the wind turbine. There, $u_{ref} \approx 7.7$ m s^{-1} prevail at hub height during the observation period and these turbulence preferences shall fit with this reference velocity. Since the turbulence statistics and the mean wind speeds were only available for us on the test site downstream of the escarpment, we transferred the turbulence statistics to the valley. There we used these values to generate synthetic turbulence on the basis of the measurements. On top of that, we adjusted the wind speed of 7.7 s^{-1} at the test site to 4.8 m s^{-1} in the valley. In this way, we match the measured data of turbulence and mean magnitudes on the test field with our simulations (See Fig. 20). This procedure is part of the adjustment of the measured flow field to the flow field in the valley. We have now clarified this approach in more detail. See **R2: Comment 26**.

"l.507: units missing after 0.03"

Thank you for this hint. We added the unit $\alpha\epsilon^{2/3} = 0.03$ m$^{4/3}$ s^{-2}. See **R2: Comment 27**.

"l.668: what is meant by "dispersion"? This is typically used for the spreading of pollution/scalar...are you referring to the wake?"

We wanted to address the dissipation of the wind turbine wake. The term was not correct. We have now removed it, since the faster dissipation of the wake under unstable conditions is dealt with separately in the following section of the conclusions anyway. See **R2: Comment 28**.

"l.685: it would be clarifying/relevant to express $\delta\ \sigma$ as a percentage as well."

Thank you, that is a good hint. See **R2: Comment 29**.