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 2 are marked in orange. Paragraphs with corrections regarding the language are marked in blue. Since all comments of reviewer 1 are very similar as those of reviewer 2, the passages of the jointly requested changes in the paper have been highlighted in green. The text passage in the paper related to the reviewer comments are marked by abbreviations and are highlighted in the marked manuscript.

Reviewer#1 comments

1. "*The manuscript is well written and presented. The usefulness of all this work is questionable since all ends in general statements and no quantification.*"

Thank you for these comments. We agree that in the present draft, the shown high fidelity simulations were for the most part only analysed and evaluated qualitatively. Individual effects were examined separately before they were applied and assessed in combination. The quantitative analysis was now conducted and added to the revised document. The specific comments were also answered below. With the quantitative analysis, the paper should contribute more to the knowledge of the community than before. All changes and more quantitative analyses have been added and colour-coded (green) in the revised paper below as requested. See **R1: Comment 1**.

2. "*The escarpment accelerates (HOW MUCH?) the flow field at the altitude of the rotor plane...*"

The quantitative analysis of the speed-up showed that in the investigated terrain on the Swabian Alps, speed-ups of 35 % to approximately 60 % result at hub height on the flat plateau downstream of the escarpment compared to the situation in the valley. The relative speed-up tends to be higher for lower wind speeds in the valley. This demonstrated the benefit of the escarpment for wind energy at the test site. For a reference speed of 7.7 m/s at hub height as it is the case in the simulations of section 4, the speed-up was approximately 60 % compared to the valley situation. For the case with 11 m/s 11 m/s on the test site it was 37,5 % and at 16.5 m/s it was 35 %. Thus, it was possible to demonstrate that the escarpment on this test site can be used to increase the performance of wind turbines. For example, if we take the case of section 3.1 with 8 m/s in the valley and 11 m/s on the test site, the wind power is 2.5 times higher. These analyses and data were also included in the revised manuscript and marked as such. See **R1: Comment 2**.

3. "*At the test site, the forest has a large impact (WHAT IS A LARGE IMPACT?) on the flow field in ground proximity (HOW CLOSE TO THE GROUND?). Highly turbulent fluctuations of low wind speeds in the forest wake strongly mix with the high-velocity flow field above and result in highly complex and turbulent flow situations (HIGHLY, COMPLEX TURBULENT FLOW SITUATIONS?).*"

Large impact means that the forested escarpment leads to a turbulent wake, which due to the inclination of the flow, can reach heights of 60 m and thus spreads into areas which correspond to twice the actual height of the forest. Further downstream, the forest wake can reach up to 70 m in height.
As figure 9 shows, with a reference wind speed at hub height of 11 $^{m}/_{s}$, the standard deviation at the turbine position can be approximately $\sigma = 3$ $^{m}/_{s}$, which corresponds to extremely high turbulence intensities. Analogously to Figure 20, with a reference wind speed at hub height of 8 $^{m}/_{s}$ and lower leaf area densities, the standard deviation can still be $\sigma = 1.5$ $^{m}/_{s}$ to $\sigma = 2$ $^{m}/_{s}$, which extremely dominates the flow near the ground. See **R1: Comment 3**.

4. "*By considering the wind turbine in the flow field, it has been shown that the forested escarpment impacts the inflow of the turbine, as well as the mixing of the forest wake with the turbine wake. This affects wake decay further downstream. Unstable conditions amplify this effect (HOW MUCH), while in stable conditions the wake extends further downstream*"

The turbulent wake structures in Figure 17 and Figure 18 have already given an impression of how different the decay can be under different thermal conditions. Analogous to these illustrations, in unstable conditions 6D downstream of the turbine, the turbulent wake spreads laterally and vertically in approximately 30 % larger areas than for stable conditions. The interaction with the forest wake, which according to Figure 22 already occurs increasingly after 3D-4D downstream of the turbine under unstable conditions, enlarges this rapid decay of the turbine wake.
Considering the effects of the turbulent forest wake under stable conditions, it becomes evident that the interaction of forest wake and turbine wake is usually smaller than unstable conditions. All these effects are, of course, also dependent on turbulence intensity, foliage density, horizontal flow velocity, etc. and therefore cannot be quantified in general terms, but a general insight is given into how dominant the influences of the forest and the thermal conditions are on the inflow and the wake of the turbine. See **R1: Comment 4**.

5. "*Taking all these effects into account when simulating real conditions of a five-minute period on 10 March 2021, decent agreements (WHAT IS A DECENT AGREEMENT?) with the mean velocity profile and the turbulent statistics measured by the met masts were shown.*"

At the position of the met mast, the transient simulations were averaged over time and compared with the measuring mast data. The "decent agreement" in Figure 20 refers to the measured mean velocity profile and the standard deviation of the velocity, which can be reproduced very well in the simulations. The deviation of all measurement points to the simulated results remains in a range of $\pm$ 1 $^{m}/_{s}$. At most measuring positions, however, the deviation is even significantly smaller. A similar statement is also valid for the comparison of the standard deviation. A similar statement is also valid for the comparison of the standard deviation. Especially in the lower area below the hub height, which is strongly influenced by the very turbulent forest wake, there are very good agreements with the measured data. The deviations $\Delta\sigma = |\sigma_{mesured} - \sigma_{simulated}|$ are generally less than $\Delta\sigma \approx 0.3$ m/s. See **R1: Comment 5**.

Reviewer#2 comments

1. "*This draft presents a significant amount of work, done around relevant topics for wind energy. The paper's organization is ok, though writing is a bit convoluted in places, and should be proofread by someone with a native English proficiency level.*
Despite significant effort behind the submission, unfortunately this reviewer must regrettably opine that the paper in its current form does not merit publication in WES, on several grounds. A primary reason is essentially a lack of documentation of what has been done, as seen by the numerous point-by-point (specific) comments below; a reader would not be able to reproduce the results shown, even if they had the same model and data. Another chief reason is that the paper is qualitative, not providing quantitative results nor grounding its arguments and claims in a quantitative fashion; it further includes various claims without references or (quantitative) support. There is a also a general lack of understanding of the atmospheric boundary layer exhibited, including a number of incorrect statements.
The overall (qualitative) conclusions are basically not new nor unexpected, though the visualizations supporting them are quite nice; again some quantitative analysis could help (see specific comments below)."

Thank you for the very detailed comments Mr. Kelly. We have looked at the requests for improvement and have made an effort to respond to all of them. The questions are answered below and the resulting revisions are highlighted in colour in the revised paper. A person with a native English proficiency level has checked our paper, we have endeavoured to show more clearly what exactly was done and we have supported the qualitative results with much more quantitative analyses to increase the scientific usefulness of this work. We have taken all comments seriously and also corrected and supplemented everything requested. The detailed responses and revisions are shown below. The authors would also like to thank you for the attached file and the helpful hints for improving the language. See **R2: Comment 1**, especially for the more in-depth quantitative analyses.

2. "*l.21-22: To avoid confusion/ensure clarity I suggest you change the term over-speed here to "speed-up", since the latter is often used in wind energy while the form can refer to something else in some contexts (e.g. performance of linearized models in complex terrain).*"

The authors agree with this suggestion for improvement and have adapted the respective passages in the revised paper. Please see **R2: Comment 2**.

3. "*l.22-23: what is the purpose/meaning of the statement "can be mapped well"? Linearized flow models (e.g. WAsP's IBZ) can also give maps of speedup... It appears you are trying to say that mean speed-up and inclination angles can be predicted with some accuracy over inhomogeneous terrain using RANS. But note that RANS also encounters difficulty over sufficiently complex terrain.*"

We have explained more precisely in the paper what we want to say: The point is that usually RANS methods can reproduce mean quantities, such as a time-averaged velocity field or the time-averaged inclination angle, decently. Of course, there are also weaknesses of these methods over sufficiently complex terrain, which we have explained better. See **R2: Comment 3**.

4. "*l.23-24: the LB sentence appears out of place, with RANS statements both preceding and after it. I'd suggest moving it.*"

Yes that's right, this sentence is somewhat out of context at this point. the LB sentence has therefore been removed completely. Please see **R2: Comment 4**.

5. *"l.24-26: To help the reader, perhaps introduce DES as a hybrid RANS-LES method; LES should also be introduced separately."*

For a better understanding of the numerical methods, we have added explanations of the LES methods and extended the explanations of the hybrid DES methods. See **R2: Comment 5**.

6. *"l.27-28: This is a bit of a mis-statement, being conditionally untrue. RANS can also account for forest and stability effects, both alone or together (e.g. Sogachev et al, 2012; van der Laan, 2020)—but usually with less accuracy than LES or DES."*

Yes, we agree. The sentence sounds as if it is not at all possible to simulate/model forests or thermal stability with RANS methods. We have now clarified the point we were intending to make about the use of hybrid methods and added the reference of Sogachev et al. (2012). We wanted to show that the very turbulent forest wake and also the turbulent flow field in general can be predicted better with DES methods than it is the case for RANS methods. See **R2: Comment 6**.

7. *"l.37: note the TKE-stability relationship predates Desmond et al (2017) by several decades. Please refer to the earlier papers as well...Ned Patton had several articles for forest and stability in regards to TKE; Sogachev had several papers from 2005-2012 on this using RANS, and in terms of measured data (forest or not) Kelly/Larsen/Dimitrov/Natarajan (2014) shows this from directly measured jPDFs."*

Thank you for this very good input! We have taken a closer look at these papers and have added references and explanations of these papers on effects of forests and thermal stratification on the local flow field in the revised paper. Please see **R2: Comment 7**.

8. *"l.40: 'examined' is not the appropriate word here. Also, how did the 5% discrepancy "therefore" lead to it being "accurately captured by measurements and simulations"? Do you mean that the latter are need to (or should) try to account for such "discrepancy"? What is even meant by "discrepancy" here—do you mean predictions compared to assuming flat neutral conditions, or which?"*

We have reworded this section to avoid misunderstandings and to clarify what the 5% discrepancy refers to and why it is important to consider thermal stratification in simulations. Please see **R2: Comment 8**.

9. *"l.47: do you literally mean "drifts", or "spreads"? This distinction is important."*

That's right. It's a small detail, but one that makes a big difference: we meant "spreads". See **R2: Comment 9**.

"l.56-57: state that it is DDES (since CFD is a non-specific term)."

Done. See **R2: Comment 10**.

"l.84: what is meant by "quickly"? Note physical dimensions, including relation to turbine position (distance to rotor)."

We have corrected these inaccurate statements and provided information about the dimensions and reasons why they were chosen. See **R2: Comment 11**.

"l.94 do you not mean z+ and not y+? How is y+ (or z+) defined? Which boundary layer do you mean–presumably the viscous sublayer? Or do you mean that a log-law will suffice/arise above this?"

Yes, that's right, it should actually be z+, which has also been corrected. In the case of aerodynamic flow around structures, one often speaks of y+, which, however, does not fit the present coordinate system. Accordingly, z^+ ($z^+ = \frac{z u_*}{\nu}$) is calculated from the wall distance z, the friction velocity u_* and the kinematic viscosity ν. In the marked area **R2: Comment 12**, further explanations of this value have also been added to the paper.

"l.105: by "turbulence", do you mean "turbulence field"? Also, is it 2d, 3d, or 4d?"

Yes exactly "turbulence" is too inaccurate, we are talking about 2-dimensional unsteady turbulence fields. An additional explanation has been added. A more detailed explanation follows later in section 2.5 with the explanation of the inflow setup. See **R2: Comment 13**.

"l.107: Regarding "0-order extrapolation", please include reference (and/or explain), as "zero-gradient" is more commonly used/understood in wind energy CFD..."

The authors agree with this statement. The expression "0-order extrapolation" means that the gradients are set to 0 and therefore we have adopted the reviewer's suggestion for a better understanding. Please see **R2: Comment 14**.

"l.112: by "atmospheric boundary layer", it appears you are meaning the profiles and/or solution of pressure, density, and temperature. Please be clear here, and note that atmospheric boundary layer refers to the lowest part of the atmosphere up to the first temperature inversion (e.g. see textbook by Wyngaard, 2010). "

Thank you for the advice. We have clarified this statement to avoid misunderstandings about the atmospheric boundary layer. We clarified that we are talking about the flux calculation, which is performed in the wall nearest 10 m using the JST method and above that using a 5^{th} order WENO method for better turbulence preservation. See **R2: Comment 15**.

"l.115-116: Is the Menter SST model used for just the RANS part? What subgrid turbulence model is used for the LES part?"

Yes, the Menter SST model is used for the RANS region. By replacing the turbulent length scale L_{RANS} outside the boundary layer with the LES filter width, the turbulence model behaves like a Smagorinsky sub grid scale model. Thereby it generates eddy viscosity as a function of the strain rate and the filter size. Please see **R2: Comment 16**.

"l.120: What do you mean by "sheared velocity profile", or why include this statement? Zero shear is very unlikely over a sizeable fraction of the atmospheric boundary layer, except the mixed layer within the CBL."

Thank you for this input. This statement is intended to clarify which position or which height served as the reference position for the specification or calculation of the CFL number. Many aerodynamic studies in wind energy are investigated under uniform inflow conditions. Consequently, the CFL number is constant over the height. Our statement is therefore intended to emphasise that the reference value for a velocity profile that increases with height was set at hub height. We have changed this formulation to eliminate misunderstandings like this one See **R2: Comment 17**.

"l.191-193: the description of Mann-model parameters is not correct; please see annotated PDF file. The Mann-model only has/needs 3 parameters, not including turbulence intensity. If you use TI to scale the turbulence per $\alpha\epsilon^{2/3}$ and box size (e.g. for σ per the IEC 61400-1), then this must be stated and elaborated. See Dimitrov et al. and Kelly."

In the paper, we have now clarified how the general procedure in our institute is to generate turbulence with the Mann model that also corresponds to a desired target turbulence intensity:

The Mann model requires input of L, Γ and $\alpha\epsilon^{2/3}$. The model is based on the spectral tensor of the ABL and, using the Rapid Distortion Theory (RDT), maps a model spectrum which is transformed into a field of velocity fluctuations by an inverse Fourier transformation. The procedure of our turbulence generation model then takes the Mann model parameters and a target turbulence intensity Ti, which the generated field should have at reference height (in our case, the hub height). In a calculation step, the defined Ti is converted into a standard deviation σ_u and set in the field. Finally, the tool generates a three-dimensional field of velocity fluctuations, the so-called Mann box. See **R2: Comment 18**.

*"l.209-210: "As the...similar magnitudes for neutral and unstable" is not correct. epsilon is not affected by stability. Also, the magnitudes of mechanically generated turbulence in neutral conditions is comparable to thermally-generated turbulence in convective conditions, only for certain stabilities (1/LObukhov) and heights for a given value of surface-layer momentum-flux (u*2)."*

We have corrected this incorrect statement and deleted wrong phrases. We have corrected this in the course of the above mentioned corrections to the Mann-model parameters. Therefore, please take a look at **R2: Comment 18**.

"l.228-231 (or all of section 2.6): does your method recover M-O similarity in the atmospheric surface layer? If so, please include reference. If not, this must be mentioned, because then all of the results must be considered only qualitative, and this needs to be stated up-front."

Yes, our model recovers M-O similarity in the surface layer. In the first submission of the paper, the references to this were missing. We have added these to make the theory behind it clearer to the reader and to make later analyses, which for example refer to the propability distribution of different thermal conditions analogous to Kelly and Gyrning (2010), easier to understand. Please see **R2: Comment 19**.

"What is the capping-inversion (depth of the ABL) set to? What is the inversion strength?"

We added information about the depth of the ABL and the inversion strength for the case in the results chapter 3.2.

Since the line number is missing that you are referring to, we assume from the context that the information for the simulations in the observation period from 10.03.2021 is also missing. We have therefore added information about the depth of the ABL. See **R2: Comment 20**.

"l.261-262, 308-309: how did you choose these specific values of LAI? Please include references. l.263 onward: How did you assign the LAI profile, or was it taken to be constant? Or did you get it somehow from the LGL?"

As most of the Swabian Alps, the part of the so called Albtrauf shown here is covered mainly by beech and partly by spruce. It is therefore a mixed forest, but dominated by beech. We have also included a reference to the vegetation types in the Swabian Alps. Through personal visits to the test site, as highlighted in Figure 6, the vegetation could be observed in more detail. In this way, the mean LAI values of the forest were set to 4.5 in summer and to 2 in winter. Measured data of the LGL were only available for the tree heights (see Figure 5) and not for the LAI values. Exact measurements of the foliage density are planned in subsequent projects on the test field. In the following, however, the LAD profiles were not set constant over the height. To model the forest as accurately as possible with the LAI value and the known tree heights, the empirical model of Lahic and Mahilovic was used. With this model it is possible to model LAD profiles when passive (e.g. Lidar drone measurements) or active (destructive) measurements of leaf area density are not available. The equations (2) and (3) and explanations have been added to the paper in order to outline the modelling of the forest and to enable a replication of the simulations shown. See **R2: Comment 21**

"Fig.8: it's nice figure, but it would help to have the velocity color-scale not include any green (say try blue or purple), since the height scale also includes green."

We have changed the colours in the illustration as requested. Please see $\boxed{\textbf{R2: Comment 22}}$

"l.295-296: regarding "The small curvatures of the streamlines and velocity changes are only caused by atmospheric turbulence", you should quantitatively support this."

We have confirmed this statement with quantitative values. See $\boxed{\textbf{R2: Comment 23}}$

"l.321-322: how can the three simulations have "the same inflow conditions and turbulence characteristics", if they have different stratification? Even if you define this in terms of σ_u or turbulence intensity at one particular height, or u_∗ and u[z some height], there are different turbulence length scales involved, and differing variations across z."

Our focus is on the aerodynamic analysis of the wind turbine under various conditions in the test field in complex terrain. The aim of results part 1 (section 3) is to consider different topographical and thermal effects separately in order to be able to correctly interpret a real observation period at the test site. In this way, the topographical and thermal influences acting on the wind turbine in the real observation period can be investigated separately (for example the effects on the phase-averaged power analysed in Figure 25).

For the three simulations shown here, we have deliberately used the same unsteady turbulence generated from the Mann model in order to be able to evaluate the influence of the buoyancy acting differently in all three cases (neutral, stable, unstable). We are aware that different thermal conditions also cause different length scales, different anisotropy, different turbulence intensities and also different shear of the velocity profiles. However, we deliberately neglected this for this purpose for the reasons mentioned above. Thus, with individual studies, we were able to look at how individual effects affect the system and its wake. These effects could then be considered in the real observation period combined with a further simulation in the result part 2 (section 4). However, in order to present all this more clearly, we have adapted the respective passages in the revised paper. See $\boxed{\textbf{R2: Comment 24}}$.

"l.325 TI=8% at what z?"

That is a good hint. We have added this information to the manuscript. Since the wind turbine is the main focus of these analyses, the reference turbulence intensity was set at hub height (z = 72 m) for all performed simulations. See $\boxed{\textbf{R2: Comment 25}}$.

"l.325-326: regarding "assessed at the same time", how can these be compared, if e.g. different spin-up times are needed due to the LES handling most of the domain? Why 4 minutes, and which are averaged versus which are "same time"?"

We have made these statements clearer in the paper in order to avoid confusion regarding the time or time periods. Initially, a Mann turbulence box was created and used for all these simulations. This turbulence was propagated through the field. After the turbulence had passed through the domain once, the analysis period was started. The statement "at the same time" means that instantaneous analyses were evaluated according to the same time step, as in Figure 11, in order to make them comparable with each other. Time-averaged analyses were always carried out over the same time period, such as in Figure 12.

4 minutes are suitable in our analyses because the focus is on the high-fidelity simulations of the fully-resolved wind turbine. Due to our small time steps and our fine resolution of the grid, larger time periods are very expensive. However, because we carry out load and power analyses with time steps of approx. 2° azimuth and also carry out analyses of only a few minutes, this is deemed to be sufficient. See $\boxed{\textbf{R2: Comment 26}}$.

"l.329-330: You cannot say these conditions are "likely to occur in nature" unless you compare to measured statistics. E.g., what is the diagnosed (implied) reciprocal of Obukhov length? If

it fits into the high-probability part of the PDF's of 1/LO found in e.g. Kelly+Gryning(2010), then you can state (how) "likely"."

Thanks for this remark! As suggested, we performed the analyses analogous to Kelly and Gryning (2010) to make a quantitative statement about the probability distribution of the stratification conditions present here. See **R2: Comment 27**.

"l.327/Fig.10: mean potential temperature should be indicated by Theta (θ), by convention/for clarity."

We have now presented the potential temperature in the paper as θ for consistency and for comparability with the literature. In addition to Figure 10, this also applies to Figure 4. Please see **R2: Comment 28**

"l.359-361: this is not really correct. For a given geostrophic wind (pressure gradient), the larger shear in stable conditions (less transfer of momentum up/down due to buoyant suppression of vertical motions and turbulence) means that velocities near the ground are lower—not the other way around. It appears that above the rotor in Fig.13 your simulated dU/dz and U(z) are both the same in stable and unstable conditions; this is likely an artifact of not having a pressure-driven ABL, and not really physical unless some very special situation arose to cause it (perhaps the escarpment helped). Also, typically the ABL depth in stable conditions is a fraction of what it would be in unstable conditions, which will also affect this. "

Thank you for the remark. Here we have made major changes in the revised manuscript. For a qualitative comparison of the different stabilities, we had performed simulations with the same inflow data as described in section 3.2, being aware that different stability conditions also affect shear, wind speed and also turbulence characteristics. As mentioned before, we performed these simulations in order to evaluate step by step the different inflows before the wind turbine was aerodynamically investigated in a real observation period with all these previously evaluated effects combined in section 4.

However, thanks to the remark we realized that here a quantitative evaluation of the simulations with the same inlet turbulence, shear, ABL depth makes less sense than the qualitative evaluation performed above. Because we only consider the pressure-induced buoyancy and neglect the effects mentioned in the previous sentence the criticism here is valid. Quantitative analyses only make sense if, as in Section 4, all effects, such as the correct shear at the respective stratification as well as the correct turbulence characteristics and also ABL depth are also taken into account. For these reasons, we have removed this figure and the associated statements. See **R2: Comment 29**.

"l.366/Fig.13: this is without the turbine present; please label as such."

Due to omission of Fig. 13 in the revised manuscript the labeling is obsolete. For **R2: Comment 30** see **R2: Comment 29**

"l.374: mean flow field? Or temporal standard deviation of just the horizontal velocity component, taken over some time? What is the averaging time, is it e.g. 4min?"

Here, the standard deviation $\sigma = \sqrt{\sigma_u^2 + \sigma_v^2 + \sigma_w^2}$ is analysed, which was evaluated over four minutes. We clarified this. See **R2: Comment 31**.

"l.378: the buoyancy force alone does not "determine the mean flow field", though it can significantly affect the mean velocity profile."

We have shown more clearly that buoyancy has a very strong influence on the flow field in the present case, but the word "determine" was too dominant in this context. We have corrected this statement. See **R2: Comment 32**.

"l.426/Fig.17: to simply add the standard deviations is not correct; the vectorial nature gives magnitude equal to $\sqrt{\sigma_u^2 + \sigma_v^2 + \sigma_w^2}$."

Thank you, we have also realized this mistake directly after the paper was submitted. The analysis was actually carried out for $\sigma = \sqrt{\sigma_u^2 + \sigma_v^2 + \sigma_w^2}$, it was just noted incorrectly in the paper. See **R2: Comment 33**.

"l.445-446: What is meant by "adjusted"? How was this done?"

We have included a detailed explanation of the adjustment. See **R2: Comment 34**.

"l.448-450: Why was Gamma set to 3.9? See e.g. Kelly(2018) and also Chougule et al.'s articles for comparison. Also from Kelly(2018), note that $\alpha\epsilon^{2/3}$ can be set by knowing L and σ_u; this way one does not arbitrarily set $\alpha\epsilon^{2/3}$."

Yes thank you, we noticed this error as well. Fortunately, this is only a typo. The anisotropy was modelled with $\Gamma = 2.9$ analogous to these slightly unstable conditions. We have corrected this. The modelling of the turbulence from the parameters of the Mann model were also adjusted as described in Comment 18. The revised paper was also adapted in this respect. See **R2: Comment 35**.

"l.451: what was the measured veer? What is considered "insignificant"?"

At the heights investigated, the maximum difference from the main wind direction was 5.5° in the averaged time window. Therefore, the wind veer was classified as negligible. See **R2: Comment 36**.

"Numerous English language/usage elements come up, many of which are further/only noted/corrected in the attached annotated PDF of the draft. " all the effort and the suggestions in the attached PDF file. We have improved the revised manuscript and corrected other linguistic deficiencies. These and all other linguistic improvements are marked in blue. See **R2: Language**.

"l.54: URANS has not been defined"

A short explanation for URANS as unsteady Reynolds-Averaged Navier-Stokes was added. Please see **R2: Comment 38** .

"l.60: what is meant by "The fully meshed wind turbine has been integrated in its designated position"? Please be clearer, to help the readers."

This means that the wind turbine was positioned in the numerical domain exactly at the position where the real turbine is currently under construction in the test site. We have clarified this in the revised manuscript. See **R2: Comment 39**

"l.64-66: this is a bit confusing; re-word, see annotated PDF."

We have also changed this in the sections marked in blue as part of the language improvements. See **R2: Language**.

"l.76: "account for" should be "avoid""

Thank you. We corrected this sentence. See **R2: Comment 41**

"l.78: re-order this sentence"

Yes, this sentence sounded a bit weird. We rewrote the sentence. Please see **R2: Comment 42**

"l.82: add reference for "hanging grid nodes", and/or explain what this is."

Hanging grid nodes mean that blocks of the mesh with different resolutions are adjacent to each other and that a resolution jump occurs at the border between them. Figure 1 shows an example of such a sharp transition from a resolution of 1 m to a resolution of 2 m. The transition is realised with the help of auxiliary cells. These auxiliary cells are also called dummy

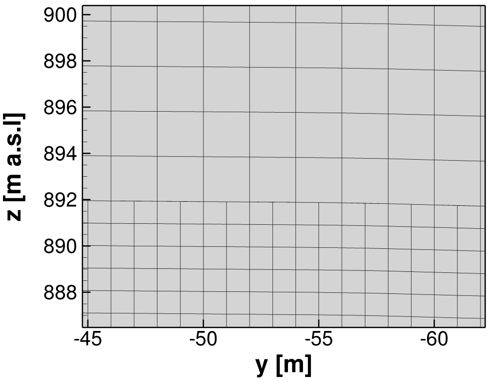


Figure 1: Illustration of Hanging Grid Node transitions in one plane of the grid

layers. The values of the adjacent block are transferred into these cells at the beginning of each Runge-Kutta cycle and serve as the calculation basis for the adjacent physical cells. See **R2: Comment 43**.

"l.82-83, 84-85, perhaps elsewhere: use a non-breaking space between each quantity and its unit."

Thanks for the hint. In the revised manuscript we avoided the line break between number and unit in these cases as well as in all other cases. Please see **R2: Comment 44**.

"l.128: "divided by"? Do you mean decomposed into?"

No we meant that we wanted to quantitatively analyse the resolved areas in the numerical domain $kres$ to identify which areas are resolved and which areas are modelled. We did this by dividing the resolved TKE $kres$ by the total TKE $ktot$. The ratio $\frac{kres}{ktot}$ indicates to which extent turbulence is resolved in certain domains. We concluded that only the wall nearest 0.5 m are modelled by URANS. We have now described this process more clearly and in more detail. See **R2: Comment 45**.

"l.130: modeled by URANS"

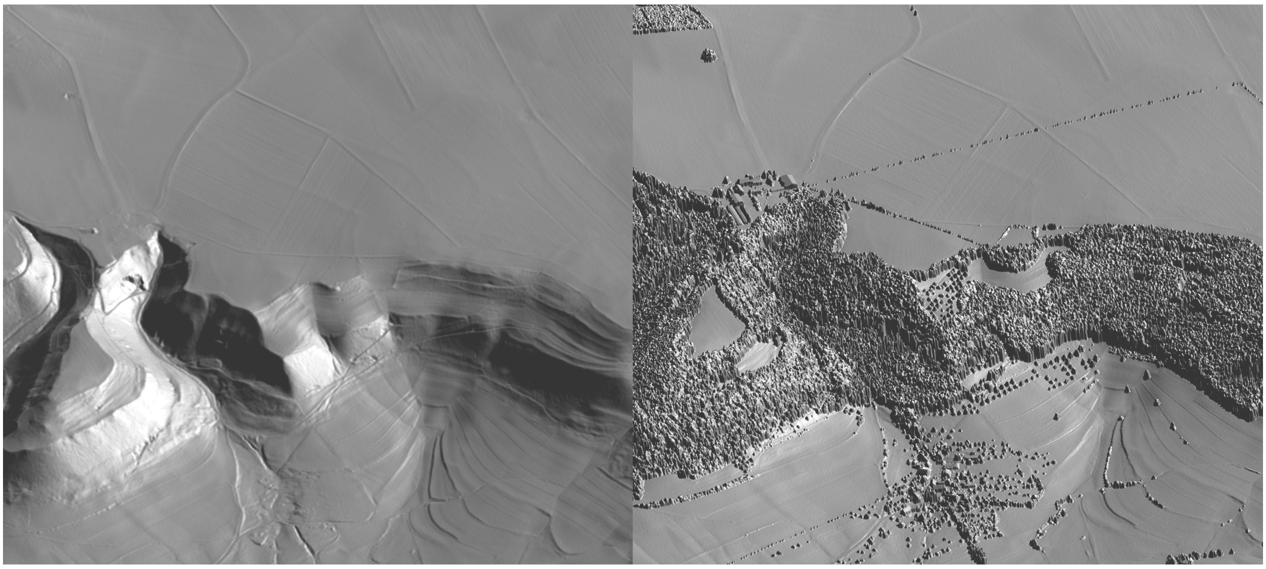
Thank you. This has now been corrected. See **R2: Comment 46**.

"l.212-213: Chougule et al (2016) looked at extending the Mann-model in non-neutral conditions, not just RDT; the latter was considered rather by Hanazaki+Hunt(2004). "

We have corrected this statement and also included the reference from Hanazaki and Hunt as the basis for all these adjustments for the Mann model with non-neutral stratification. Please see **R2: Comment 47**.

"l.247-248: is it possible to include a reference to these datasets/digital models?"

We have added Figure 2to explain the differences between DEM and DSM models. This Figure shows the differences between the two models. The test site of the DEM model is only characterised by the orography, whereas the DSM model also shows vegetation, houses, etc. We have received the data directly from the LGL. There is no specific reference, so we have added Figure 2 in the revised manuscript to ensure reproducibility of the setup and the results. Due to the (height) differences of these models it is possible to determine dimensions of trees etc. See **R2: Comment 48**.



DEM model DSM model

Figure 2: Comparison of the DEM and DSM models of the test site in complex terrain