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Review of wes-2021-88

Mark Kelly (Referee)

Referee comment on "CFD Studies on Wind Turbine Interactions with the Turbulent Local Flow Field Influenced by Complex Topography and Thermal Stratification" by Patrick Letzgus et al., Wind Energ. Sci. Discuss., https://doi.org/10.5194/wes-2021-88-RC2, 2022

General notes ____________________________

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).

specific comments ________________________

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).

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.

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

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

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.

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.

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?

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

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

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

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?

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

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

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).

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

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.

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 $\alpha\varepsilon^{2/3}$ and box size (e.g. for σ_1 per the IEC 61400-1), then this must be stated and elaborated. See Dimitrov et al (2017, doi.org/10.1016/j.renene.2016.10.001) and Kelly(2018, https://doi.org/10.5194/wes-3-533-2018).

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 ($1/L_{\mathrm{Obukhov}}$) and heights for a given value of surface-layer momentum-flux (u_*^2).

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

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?

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.

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

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*.

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

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"?

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 $1/L_O$ found in e.g. Kelly+Gryning(2010), then you can state (how) "likely".

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

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.

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

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?

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

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}.

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

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 $\alpha\varepsilon^{2/3}$ can be set by knowing L and sigma_u; this way one does not arbitrarily set $\alpha\varepsilon^{2/3}$.

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

technical corrections ___________________

Numerous English language/usage elements come up, many of which are further/only noted/corrected in the attached annotated PDF of the draft.

l.54: URANS has not been defined

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.

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

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

l.78: re-order this sentence

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

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

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

l.130: modeled by URANS

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).

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

Please also note the supplement to this comment:
https://wes.copernicus.org/preprints/wes-2021-88/wes-2021-88-RC2-supplement.pdf