Reviewer 1:

This work conveys a first attempt at investigating the effect of turbulence intermittency induced by stably-stratified conditions, on atmosphere-windfarm interaction. This involves exploiting an implementation of the DRM model from Katopodes-Chow(2005), permitting the 'backscatter' of turbulence from subgrid scales to larger resolved (though filtered) scales in LES.

[WRF-]LES is run for two cases roughly corresponding to the AWAKEN (CASES99?) campaign, though some details are lacking; it would be difficult for a reader to verify or repeat the study, as things like the explicit filter form are missing. Clarity and references are also lacking in some places, as noted further below.

Idealized simulations for two stable regimes, with a very shallow (rare) SBL having z_i=100m and LLJ below hub height (<80m) in the strongly-stable case, are conducted to study the impact of intermittency. The shallow inversion may be seen to dominate the heat flux, causing 6 times the surface value at z=z_hub; this could use some justification (e.g., many turbines nowadays are taller and larger, approaching or exceeding the inversion height, compared to the one used in the LES here), and\or be more clearly mentioned in the discussion\conclusion (also referring to literature finding such).

Some nice statistics and visualizations of stably-stratified intermittency are shown for the two cases, including compelling evidence of the DRM-enabled backscatter (up-scale transfer of TKE) in Fig.4.

The abstract (and conclusions) should more clearly mention the context, that two flow regimes are prescribed: the more stable case being a rare one where the ABL depth is lower than the upper rotor tip, and associated jet being centered near\below hub height; and a weaker stable case with more commonly-found ABL depth. These 2 ideal cases logically exhibit two different sets of behavior, though all of this depends to some extent on the strength of prescribed free-stream temperature gradient (inversion strength, where there isn't really an inversion).

The intermittency is not found to affect power production, but rather production variability (it could be nice to see what happens in a typical 10-minute period instead of 1 hour).

This reviewer suggests revision, towards publication.

We thank the reviewer for the time spent reviewing our work and for the supportive suggestions and comments. Our replies (including to the above general comments) are inserted below into the reviewer's specific comments.

I attach an annotated PDF that has many small corrections, along with detailed comments; the present text feedback gives the general overview.

Thank you for the annotated PDF. The small corrections have been made to the revised manuscript (please see the diff file). Detailed comments have been inserted towards the end of the responses here.

The introduction is relatively solid, though there are a number of points where references are needed. This is also the case for section 2.1.

We have added several references throughout, as noted in response to other comments below.

Section 2.2 on the DRM model appears to have (or at least cause) some confusion around how the filtering works; some clarification could help, especially since the referenced Chow et al.(2005) paper itself lacks details and points to previous papers by Xue and Chow for the notation.

Please see our responses to the following comments where we have clarified text and added additional references.

One question which arises in eq.(1), for the explicitly filtered Navier-Stokes equations on the LES grid, is the non-commutativity of the discretization and filtering operators which arises for the body force term (see e.g. Finnigan et al papers on this) representing the actuator disks.

Thank you for raising this point. To reduce confusion, we have removed the tilde from the body force term. Note that explicit filtering is not applied to the actuator disk body force term directly. Rather, the body force is a parameterized term that depends only on resolved quantities and is designed to match the thrust curve of a specific turbine. Additionally, the actuator disk model includes Gaussian smoothing to minimize discontinuities between grid cells that intersect the rotor vs. those that do not. The body forcing term also does not include any differentiation. Ultimately, any non-commutativity errors are assumed to be small compared to uncertainties in the parameterization itself.

Or, some mention should be made of how\why the explicit ("smooth") filtering is not applied to the final terms in eq.(1).

For the turbulent flux divergence (2nd to last term), the explicit smooth filter is inside the definition of tau (see equation 2), therefore the overbar does not appear in equation 1 on that term.

Also, why does the resolved stress divergence term have an extra discretization operator applied (perhaps Bardina et al show this)?

There is a tilde (discretization operator) that appears on the turbulent flux divergence term because the divergence operator is discrete. Note that in practice, with a finite difference method, the discretization operators are implicit. This is discussed further in Chow et al. 2005. We have added additional references to Gullbrand and Chow 2003 and Chow et al. 2005 to direct the reader there for further details on the DRM.

What is the actual explicit spatial filter applied, in the horizontal and vertical directions?

The explicit filter is a top-hat filter with a width twice the grid spacing in each direction. This has been added to the text at line 128 of the revised manuscript:

“The dynamic reconstruction model uses a 3D (top-hat) filter applied to the Navier-Stokes equations to separate large and small scales. This filter is twice the grid cell spacing and is applied explicitly in the model; it is thus distinct from the implicit effects of grid discretization.”

The near-wall stress-correction of Brown et al, 2001 (similar to Moeng's) is used, but a "canopy" is mentioned with regard to setting the correction's vertical decay rate via shape parameter and cutoff-scale H_c, lacking some explanation (or removal of 'canopy'). Also in eq.(6) should the square not be on "cos"?

Thank you for bringing this to our attention. We sometimes use the term canopy to describe the near-wall stress-correction, but we have replaced this with “near-wall stress” to avoid any confusion.

The square should indeed be on the cosine and the notation has been fixed.

In §2.4, with the periodic domains, it appears that a WRF domain (and really WRF) is not used, rather a nested LES domain; this should be clarified.

We have rephrased this to clarify that WRF-LES-GAD is run in idealized mode. Please see lines 199-200 in the revised manuscript:

“WRF-LES-GAD is run in idealized mode, with a one-way nested and flat terrain setup to study how a range of SBL conditions interact with a wind farm.”

The choice of $d\theta/dz$=4K/km is a reasonable moderate value (see e.g. Kelly\Cersosimo\Berg2019), but the ABL depth prescribed via inversion height of 100m is quite rare. The latter should be mentioned, beyond "follows common approaches for...idealized SBL's". I.e., with such a choice you are testing cases where downward entrainment into the simulated windfarm will be quite significant.

Thank you for mentioning this. Please see the response to the following comment. Additionally, we have added the following lines at 275-276 and 540-542 in the revised manuscript:

“As such, for this case, downward entrainment into the wind farm is especially significant (see Sec: 3.3)”

“While LLJs below hub-height are uncommon, it is worth noting that as turbines continue to grow (Bilgili and Alphan, 2022), hub-heights will more frequently extend into the free atmosphere and better understanding of this behavior is needed for turbine design (Veers et al., 2023).”

Your chosen values of z_i and the inversion strength will dictate the strength of the jet (Pedersen et al.,2014) and downward heatflux (Kelly\Cersosimo\Berg,2019). Recall the inversion strength is simply the Brunt-Vaisala frequency corresponding to your 0.004K/m and your θ~=300K there.

Thank you for bringing these references to our attention. We acknowledge that an inversion height of 100 m is low; however, the prescribed inversion height only represents the initial condition and the height of the inversion is free to evolve. For the weakly stable regime, the continuous turbulence during the spin up period increases the ABL depth until it is realistic and quasi-steady. Ultimately, as mentioned, the quasi-steady ABL depth will depend on the Brunt-Vaisala frequency in the free atmosphere, the geostrophic forcing, and the surface cooling rate / surface heat flux. For the weakly stable case, this ABL depth is on the order of a few hundred meters, but for the strongly stable case, the ABL depth ends up being close to 100 m because of the weak nature of the turbulence.

We have reworded and added sentences beginning at line 238 in the revised manuscript. We have also included additional references of idealized SBL studies that utilize a similar set as ours.

“The initial profile is not meant to represent observed meteorological conditions but instead follows common practice in idealized SBL LES studies ((Beare et al., 2006; Zhou and Chow, 2011; Peña et al., 2021; Gadde and Stevens, 2021a, b) where relatively shallow initial boundary layer depths and prescribed inversion strengths are used to control the development of the stable boundary layer. In our configuration, the imposed inversion strength and initial inversion height together promote the formation of an LLJ and turbulent entrainment at the capping inversion. As the simulation evolves, turbulence-driven entrainment increases the ABL depth and provides a significant pathway for downward transport of momentum and heat from the free atmosphere into ABL, contributing to wind turbine wake recovery (van der Laan and Sørensen, 2017) as discussed later."

Note that you are using 13 hours of LES "spin-up" time, which might be ok; this needs to be checked, not just justified as "commonly done" as written (e.g. see Pedersen et al, 2014 on LES spinup of inversion-capped ABL).

There is a discernable spinup period of 7 hours, after which, the turbulent statistics such as TKE and heat do not change significantly and are quasi-steady. We ultimately chose hour 13 to 14 to minimize effects due to inertial oscillations.

In the revised manuscript, we have added the following sentences at lines 257-258:

"Additionally, turbulence statistics were found to be quasi-steady for the weakly stable regime (not shown). The fluxes do not reach a quasi-steady state for the strongly stable regime due to intermittency; therefore, we select hour 13 to 14 for consistency."

We acknowledge that the Pedersen reference states that 16 h is needed to reach quasi-steady state (although they state that the boundary layer depth changes little beyond 8 h). However, their study is for neutral ABLs, which typically have much longer spin up periods than convective or stable boundary layers.

In §3.2 mean stability values are calculated inconsistently; one must calculate the mean of 1/L, not L (nor the individual variables within it, due to their skewed distributions), as shown in Kelly&Gryning(2010).

Thank you for bringing this to our attention. We have recalculated the scaling formulation by determining the mean of 1/L at the surface. The value of h/L did not decrease significantly, but is slightly smaller than the previously calculated value (currently 18 compared to 19, previously). Please see lines 327-334 in the revised manuscript:

"The inverse Obukhov length is formulated as: $L^{-1} = -\kappa (g/\theta_{sfc}) H/u_{*,sfc}^3$, where $u_{*,sfc}$ is the surface friction velocity, H is the surface heat flux, κ is the von Kármán constant (taken as 0.4), g is the gravitational constant, and θ_{sfc} is the potential temperature at the surface. The average inverse Obukhov length is then taken over the analysis period. Note that we use this formulation as opposed to averaging the individual quantities that make up L due to the non-linear influence of the surface fluxes in the similarity functions as demonstrated by Kelly and Gryning (2010). The inverse Obukhov length is calculated to be 0.172 m^{-1} for the strongly stable regime resulting in h/L = 18,thus confirming that the strongly stable regime is intermittent as defined in Fig. 2 of Holtslag and Nieuwstadt (1986).”

Please also see the next comment for additional Obukhov length discussion and formulation.

The surface heat flux for the strongly stable regime could be compared to CASES-99 or AWAKEN values, for the reader \ connection to "real world".

There is no published climatology for the surface heat flux itself for the region, but there are figures with distributions of atmospheric stability classified by the Obukhov length at 25 m above the ground for the AWAKEN region by Krishnamurthy et al. 2021.

We recalculated the (inverse) Obukhov length for the strongly stable case at 25 m above ground following the formulation in Kelly and Gryning 2010 and Krishnamurthy et al. 2021 and found an Obukhov length of 36.9 m for the strongly stable case. This value is within the strongly stable range defined by Krishnamurthy et al. 2021 ($0 < L < 50$). The distributions of atmospheric stability are a function of wind direction, time of day, month, and wind speed. Very strongly stable conditions occur approximately 20% of the time, indicating that the simulated strongly stable case is entirely plausible in the AWAKEN region. Please see lines 336-345 in the revised manuscript:

“Furthermore, the Obukhov length L can be defined at a specified height, which is commonly done when characterizing the climatology of field sites. For the AWAKEN region, Krishnamurthy et al. (2021) calculated the Obukhov length at 25 m above the ground and found that very stable conditions ($10 < L < 50$) occurred approximately 20% of the time from 2012-2019 (see Figs. 14 and 15 in Krishnamurthy et al. (2021)). For our study, we can similarly calculate L using the previously mentioned formulation but by replacing H with the resolved heat flux $w'\theta'$ at 25 m where w' and θ' are the fluctuating components of the vertical velocity and potential temperature obtained by removing a running 30 minute mean (the same time window used in Krishnamurthy et al. (2021)). For the strongly stable regime, the average Obukhov length is 36.9 m, which is within the window defined in Krishnamurthy et al. (2021) for strongly stable conditions. This comparison demonstrates that the strongly stable case considered here is entirely plausible in the AWAKEN region.”

Fig. 6 is very nice, though the colors of the lines in plots (a) and (c) are quite similar; they should be made more dissimilar, and use different line styles as suggested by the WES journal.

Thank you, Fig. 6 has been revised to make the colors more distinct.

Some mention of the lack (or narrowness) of the inertial range seen in the spectral plots should be made, as it implies a small Re without discussing the explicit filtering; perhaps the 5/3 line could be shifted to better compare against the z=50 spectrum, and the lack of inertial range at higher z discussed.

> The 5/3 has been shifted to be closer to the z = 50 m line. Additionally, we have added additional sentences discussing the lack of inertial subrange for the larger heights above the surface. Please see lines 382-385 in the revised manuscript:
>
> "In Fig. 6(d), as height increases, the width of the −5/3 inertial subrange decreases. The time series at 80~m is within the nose of the LLJ and the time series at 110~m is above the LLJ. At both of these heights, turbulence is very weak, so the spectra exhibit little to no inertial subrange, which is another reason why the difference in the energy content between quiescent and turbulent periods at these heights is small."

While shallower ABLs and the corresponding inversion-associated jets are often connected to more stable surface-layer conditions, note that you prescribed its strength and height via setting of the capping temperature inversion magnitude (inlet temperature profile) along with surface cooling rate, roughness, and G; one can reference Wyngaard's (2010) textbook, while on the LES side one has e.g. Pedersen et al (2014) and articles by Lanzilao and others.

> Thank you for bringing these references to our attention. We would like to note that there is no inlet temperature profile or temperature forcing. The only forcing mechanisms in our simulations are geostrophic forcing G and surface effects. We prescribe an initial temperature profile but then the simulation is free to evolve in terms of potential temperature given the surface cooling and geostrophic forcing.

Comments from the PDF

Line 11: re-word, here you mean magnitude of surface heat flux. However, this statement might not be fully true, since the capping temperature-inversion magnitude (strength) has the opposite effect, and the prominence of the effect depends on this.

> This sentence has been reworded to explicitly state that the wakes increasing the height of the boundary layer is mainly due to the location of LLJ and the rotor specific to this study, rather than a more general result. Please see lines 10-12 for the revised sentence:
>
> "Wakes increase mixing and deepen the SBL, with a stronger effect under strongly stable conditions, primarily because the SBL is shallower and closer to the top of the wind turbine rotor layer."

Line 12: is this not assuming that the SBL is significantly deeper than the upper rotor tip?

In this statement, we are not assuming that the SBL is significantly deeper than the upper rotor since it is discussing the strongly stable case where the depth of the SBL is similar to the height of the upper rotor tip. To avoid any confusion, we inserted the word “intermittent” into line 13 of the abstract in the revised manuscript.

Line 28: this is not shown yet, no? Also, need citations for the performance impact of stability (via shear & turbulence, e.g. Dimitrov et al 2018) as well as waves from the stable capping inversion (e.g. Lanzilao & Meyers 2024)

Thank you for pointing this out. You are correct in that there are not any studies that have directly tied intermittency to performance, other than the present study and have included the suggested “(possibly)” on line 30 of the revised manuscript.

We included Dimitrov et al 2018, Wharton and Lundquist 2012, Vanderwende and Lundquist 2012, and Sanchez Gomez and Lundquist 2020 regarding the performance impact of stability. For the presence of waves, we’ve included Lanzilao and Meyers 2024, Draxl et al. 2021, and Wise et al. 2025.

Line 41: perhaps you should mention whether you are "locally" considering more than just the surface-induced shear, since many WES readers come from windtunnel\engineering\CFD backgrounds here and not micrometeorology

We have reworded the sentence to be more specific to be more inclusive of readers from other backgrounds. See lines 43-44 of the revised manuscript:

“Conversely, for internal intermittency, shear increases locally within the stable boundary layer (not only at the surface), inducing instability and mixing (Mahrt, 2014; Sun et al., 2015).”

Line 110: it is not a binary 'validity' but more so the decreasing applicability of MOST for z/L beyond Ri_crit; this reviewer suggests re-wording and splitting the sentence. Also, should reference e.g. Kumar+Sharan(2012) and especially Cheng\Parlange\Brutsaert(2005)

Thank you for the suggestion. The sentence has been reworded and the references have been included. Please see lines 111-115 of the revised manuscript:

“MOST is based on the assumption of continuous turbulence; however, as stability increases (and turbulence becomes intermittent), the applicability of MOST decreases (Chenge and Brutsaert, 2005; Kumar and Sharan, 2012). Nevertheless, in the absence of widely accepted alternatives for representing the surface layer and bottom boundary condition, we continue with MOST and seek to extend the model to very stable stratification."

Line 128: but why filter away then reconstruct? A clearer sentence, which also relates to the Chow(2005) paper, would help. Also note that Chow's Fig.1 indicates RSFS at scales greater than the filter scale that you mention here

The filter separates large and small scales, and also smooths to flow fields, thus limiting discretization errors. The inverse filtering operation is needed in order to model the effect of the RSFS stress terms. In the revised manuscript, lines 131-132 now read:

"There is thus a range of scales between the grid scale and the filter scale that can be partially reconstructed in the model using an inverse filtering operation (Chow et al., 2005)."

The RSFS scales are shown in gray in Fig. 1 from Chow et al. 2005; these scales are smaller than the smooth spatial filter and larger than the grid cutoff scale.

Line 134: Following Chow(2005), the tilde indicates discretized—and thus implicitly filtered quantities. Then how could the stress divergence term in (1) have an over-tilde?

We have reworded lines 136-137in the revised manuscript to:

"The overbar operator represents explicit filtering via a smooth spatial filter and the tilde operator is representation of the implicit filter operation."

The tilde over the stress divergence term indicates the added effect of the discrete differentiation, as in the advection terms.

Line 134: but they do not commute, as assumed, for the F_i term

Please see our response to the comment above (the fifth response).

Line 165: canopy? Do you mean to consider a windfarm as a canopy layer?

Thanks for catching this. Please see the response above regarding the near-wall stress-correction as that is what the canopy was referring to (and not the wind farm). The term canopy is no longer used in the manuscript.

Equation (6): should this be cos squared, or cos of the squared argument?

This should be cos squared and has been fixed in the revised manuscript

Line 179: should this not be different for the LES resolutions you are using?

We used a value of ⅓ to be consistent with the set up for Zhou and Chow (2012). We did rerun the simulation using the Prandtl number formulation from Venayagamoorthy and D. Stretch (2010) and found similar levels of intermittency but with some differences in the mean profiles.

Line 182: you should reference Deardorff, Moeng, Sullivan, etc. here

These citations have now been included.

Line 234: you are prescribing the capping inversion strength, as well as an inversion height (ABL depth) of only 100m; such a shallow ABL is quite rare.

Please see our response to comments above regarding the simulation setup. Notably, the inversion height is only an initial condition. The inversion height is free to evolve (as it does in reality through the development of the evening transition and the nocturnal stable layer) and does so for both cases. For the weakly stable case with stronger turbulence, the inversion height increases and becomes quasi-steady at approximately 400 m. For the strongly stable case, the inversion is quasi-steady at 105 m.

Additionally, we have included additional references that utilize the same setup as ours where the initial temperature inversion is located at 100 m. This setup stems from the original GABLS setup but has been used for less extreme SBL studies.

Line 248: spin-up should be mentioned; it is not just "commonly done" but the statistical stationarity\representativity should be checked

Please see our response to the comment above regarding the turbulence spin up period.

Line 310: average Obukhov lengths are not representative nor reliable, due to the character and distribution of L; rather, one calculates statistics of 1/L as shown by Kelly & Gryning (2010). Further, using mean ustar and not ustar^3 also introduces non-representative numbers (see aforementioned article and Kelly+Troen 2016). This reviewer suggests\insists on re-calculating the average 1/L values; these will likely give a different h/L, which might not be so extreme as 19.

Please see our response to the comment above regarding calculating the inverse Obukhov length.

Line 429: this is how wakes recover; similar plots (along with corresponding ideal equations) are shown in van der Laan et al. (2023) on "wake recovery mechanisms"

Thank you for this reference. We have added the following sentence on lines 466-467 of the revised manuscript at the end of the paragraph discussing wake recovery and momentum flux:

“Additionally, see van der Laan et al. (2023) for additional discussion regarding wake recovery mechanisms.”

Figure 16: By "frequency" in the right-hand plots, do you not mean "count"? If so, then replace with count. Further, one cannot see the histogram differences in these plots, due to the overlapping shading; please use the upper-bincount edges to allow the reader to see the differences.

Frequency has been changed to count in the figure. Additionally, the upper-bin count edges have also been added.

Line 492: it did not merely just "form", but was basically prescribed through your inlet temperature profiles along with surface cooling rate, roughness, and G

This sentence has been reworded, please see lines 528-529 of the revised manuscript:

“For both stability conditions, the imposed forcing resulted in a LLJ at the top of the stable layer with the height of the jet relative to the turbine having a major impact on the SBL evolution downwind.”

Line 517: generic; recommend removal or specificity

We have reworded the sentence for added specificity and included a couple of references. Please see lines 554-557 of the revised manuscript:

“Other mechanisms for SBL intermittency that are common in the U.S. Southern Great Plains, the region for the AWAKEN field campaign, include bores, Kelvin-Helmholtz billows, terrain-induced flows (e.g, Radünz et al. (2025) for flow speedup and Zhou and Chow (2014) for terrain-related shear-instability waves), and propagating density currents.”