Reviewer comments on the paper

A wind-farm wake-turbulence parameterization for the WRF model (EWP v2.0)

General comments

In this paper, a new parameterization for the unaccounted production of turbulence by the turbine wake is proposed. The application is for mesoscale simulations using Wind Farm Parameterization (WFP) to represent the wake effects from wind farms.

The new method is conceptually sound and targets an important and neglected root problem in the literature: accounting for the influence of the subgrid wake on TKE production and thus wake recovery within and especially downstream of the turbine grid cell. In the Fitch parameterization, the TKE is injected at once within the turbine grid cell and decreases too fast over the downstream grid cells. This fast decrease of TKE occurs because the mesoscale simulations lack shear production of TKE downstream owing to their coarse spatial resolution. With this method, fine subgrid-scale processes are accounted for while using a coarse mesoscale grid. The superior performance of the new LKE model against the well-known Fitch WFP and PBL schemes is demonstrated. Thus, I am satisfied with the vision and development of the LKE method. Nonetheless, I would like to offer some comments and suggestions to improve the manuscript.

More discussions with the literature are needed. When presenting results, there is no reference to the literature to indicate whether the results are supported by it. Also, some of the latest developments and discussions on WFPs are not mentioned. You can find the suggested References at the end of the document.

Turbine power is a necessary piece of research with WFPs because it depends on and influences the wind speed deficit via momentum extraction. These results will enrich the paper with a deeper discussion of performance across different modeling setups. To summarize, these elements that influence the streamwise evolution of wind speed should be described in the manuscript: turbine power, momentum extraction by the turbines, TKE production, and wake recovery. As is, the paper only mentions TKE production and wake recovery.

Lastly, how is the literature supposed to behave in the face of this new model and its many parameters and possibilities? Is the model ready for real wind farm simulations? Should they use the recommendations proposed here or perhaps tune the model with their own LES cases? Can the recommendations provided here be a starting point? What about the application to cluster wake effects? The Discussions and/or Conclusions would benefit from this clarification.

Specific comments

1) Are you aware of the axial induction correction proposed by Vollmer et al. (2024) to avoid a negative wind speed bias in the turbine grid cell (a single turbine causing itself a deficit)? Although it is unlikely its usage would change the results, it is worth mentioning this improvement for the Fitch WFP.

2) "In this sense, it plays a role analogous to shear production, which transfers energy from the mean flow to turbulent fluctuations." Perhaps it can be stated that the mechanism through which the wake generates TKE IS shear production. Otherwise, "a role analogous" gives the impression of a similar, but not quite the same, process.

3) Turbine added TKE varies with stability, but the Fitch WFP does not account for that (Porté-Agel et al., 2015). The LKE model is sensitive to stability, so this positive feature should be highlighted.

4) Du et al. (2025; 2026) propose an improved prediction of subgrid wake effects, momentum extraction, which improves the wind speed deficit at the wind farm exit.

5) Recent results (e.g., Radünz et al., 2025) suggest that improving TKE alone does not fully resolve the wake recovery problem owing to unresolved momentum gradients. A discussion of these findings would strengthen the manuscript.

<u>Methods</u>

6) "circumventing the complexities involved in validating TKE". Please specify what complexities those are.

7) Figure 2. Explicitly mention the wind farm dimensions in km.

<u>Results</u>

8) Figure 3. Delta TKE equal zero should be white, not cream. Otherwise, it may look like the wind farm induces a change in TKE even 5–10 km away from the wind farm sides.

9) Figure 4.

    a) Explain what 'L' stands for.

    b) Include Fitch-100-adv. Many studies point out that the standard value of 25% may be too low for some cases.

10) Figure 5.

a) In 5a, include more Fitch-XX-adv cases to understand how the TKE levels at the wind farm exit influence its evolution behind the wind farm. Perhaps 25% and 50%.

b) In Figure 5b, the normalized wind speed is not the best metric to assess the streamwise rate of change. There is also a large spread in wind speed deficit at the wind farm exit. Consider showing the streamwise gradient of wind speed or another metric that better isolates wake recovery rates (Figure 5c).

11) Important to discuss in the paper the role of wake losses and momentum extraction on the streamwise evolution of the wind speed, alongside wake recovery. For instance, in Figure 5b the wind speed at the wind farm exit is much lower in the mesoscale simulations in comparison with the LES.

12) Figure 6.

a) Consider including the profiles at one more streamwise coordinate, perhaps at x = Lwf/2 or even 0, to assess the streamwise evolution of the profiles.

b) Why is Pw maximum at hub height?

13) I would reconsider the value of using Fitch cases without advection (especially in Figure 7). In the literature, this is considered a bug that bypassed the researchers' knowledge of its implementation in WRF (Archer et al., 2020), and is not a serious modeling setup. I understand that the reason for including the Fitch cases without advection highlights the "passive security" of the new model. However, the reader must be aware Fitch-ctrl is there to demonstrate one specific aspect of the new model, and not as a plausible modeling setup.

14) Figure 7. The wind speeds in all mesoscale simulations diverge from those of the LES behind the wind farm. It is important to discuss what process creates that divergence, the role of TKE in it, and if some PBL setups are better at representing the streamwise rate of change of wind speed. The expectation is that improved TKE may lead to improvements in wake recovery relative to the LES. Furthermore, it is important to discuss the impact of different wind speeds at the wind farm exit (owing to different momentum extraction within the farm) on wake recovery rates.

15) Figures 5 and 7. Is the comparison of wind speeds within and behind the wind farm fair, considering the negative bias in wind speeds for the mesoscale simulations? If the wind speeds upstream matched, what would be the outcome? This clarification is important as there could be a shift in which simulation better matches the LES.

16) Consider horizontalizing subplots in Figures 5 and 7 for improved layout. Alternatively, stretch them horizontally for a better visualization.

<u>Discussion</u>

17) "circumventing the complexities involved in validating TKE". Please specify what complexities those are.

18) “We consider that a 20-meter horizontal resolution in our LES was adequate to evaluate TKE from wind farm parameterizations”: Please include the vertical resolution here. Refer to the literature that a 20 m grid is sufficient for a near-neutral boundary layer.

19) “satisfactory results away from the turbine locations, as indicated by previous studies”. There is no reference associated with this claim. Furthermore, be more precise about the “satisfactory results”: wind speed or TKE? Within the wind farm, at the exit or further downstream?

20) “gradual and measured release of TKE”. Perhaps “release” could be replaced by “shear production”.

21) “(and possibly the real world)”. I would omit this.

22) “Alternatively, one could adopt the approach of Fitch et al. (2012), which considers the difference between thrust and power coefficients for the LKE source.” I would either further explain or remove this statement. This approach to computing the available LKE is insensitive to atmospheric stability, and thus should not be presented as an alternative approach to yours. If you do want to present this approach, include the limitations related to atmospheric stability (which is a strength of your new WFP).

23) “An interesting possible improvement should account for sub-grid wake interactions (as in the MAV parameterization; Ma et al., 2022), providing a more precise estimation of the inflow wind speed than the standard averaged-grid-cell speed approach.” Again, this discussion could include the promising approach proposed by Du et al. (2025, 2026) for representing subgrid wake effects.

24) “the present form (Eq. 11) is direct and practical”. Please be more precise about what direct and practical stand for.

25) Please mention that PBL schemes should not be used without advection in the last paragraph. Otherwise, the reader may think it is a viable alternative route.

26) Could the LKE method be used with the subgrid wake model by Du et al. (2025; 2026) to produce even better results? This is an interesting discussion, as their subgrid wake model seems superior to Fitch, MAV, etc.

Conclusions

27) “Introducing TKE from turbines”: Perhaps: “Introducing TKE from turbine **wakes**”

28) “circumventing the complexities involved in validating TKE”. Please specify what complexities those are.

29) “allowing TKE to be released progressively”: Perhaps “allowing TKE to be **produced** progressively”, since shear production is the physical mechanism. “Release” refers to the LKE reservoir, which is not a physical mechanism. If you want to keep “release”,

please tie it up conceptually with the LKE model and mention the actual physical process of shear production.

30) I would include the sensitivity to different atmospheric stability as the third item in lines 528–533. Also, please include line numbers more frequently, every one or two lines in the revised manuscript.

Technical corrections

1) “The comparison of the wind speed deficits in Fig. 7b reveals that MYJ simulations exhibit similar deficits when using the MYNN—greater deficits than those observed in LES-AD”: This sentence needs rewriting. Specify where the deficits are greater than the LES, as they match at some locations. Also, I would remind the reader that the LKE-opt uses MYNN. For instance: “exhibit similar deficits when using the MYNN scheme (LKE-opt)”. Last, there are several MYJ cases and some do not match LKE-opt at all. Please state you (probably) are comparing MYJ-LKE-opt and LKE-opt.
2) “Wind speed deficit patterns in the Fitch simulations under MYJ resemble those of MYNN, without TKE advection (Fig. 4), but with different TKE levels.” Please be more precise (with what and where) with what you mean by “wind speed deficit patterns”. Furthermore, you discuss wind speed deficits but refer to Figure 4, which shows NDTKE.

References

1) Abkar, M. and Porté-Agel, F.: Influence of atmospheric stability on wind-turbine wakes: A large-eddy simulation study, Phys. Fluids, 27, https://doi.org/10.1063/1.4913695, 2015
2) Vollmer et al. (2024): Brief communication: A simple axial induction modification to the Weather Research and Forecasting Fitch wind farm parameterization, Wind Energ. Sci., 9, 1689–1693, https://doi.org/10.5194/wes-9-1689-2024.
3) Radunz et al. (2025) Under-resolved gradients: slow wake recovery and fast turbulence decay with mesoscale Wind Farm Parameterizations. Wind Energ. Sci. Dis. https://doi.org/10.5194/wes-2025-147
4) Du et al. (2025): A Meso-Microscale Coupled Wind Farm Parameterization, Boundary-Layer Meteorology, 191, 35, https://doi.org/10.1007/s10546-025-00928-7, 2025.
5) Du et al. (2026) Gaussian-based multicolumn spatial distribution method for wind farm parameterizations, Physical Review Fluids, 11, 023 801, https://doi.org/10.1103/5lsj-m746, 2026.
6) Khanjari et al. (2025): An analytical formulation for turbulent kinetic energy added by wind turbines based on large-eddy simulation, Wind Energ. Sci., 10, 887–905, https://doi.org/10.5194/wes-10-887-2025.