Response to Reviewer

We thank the reviewer for the careful assessment of the manuscript and for their constructive comments and recommendations. The original reviewer comments are presented in black, our responses and text added to the manuscript are presented in blue, and deleted text is presented in ~~red strike-through~~.

Reviewer 1

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, 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.

We thank the reviewer for these insightful comments. Following this recommendation, we have expanded both the Introduction and Discussion to better connect our results with recent developments in wind farm parameterizations, including the studies suggested by the reviewer. We now discuss how improvements in turbine inflow, momentum extraction, turbine-added TKE, and wake recovery represent complementary aspects of the wind-farm-wake problem. We also clarify the scope of the present work. The LKE model is intended as a proof-of-concept development focused on improving the representation and downstream evolution of turbine-induced TKE. While turbine power and momentum extraction are important components of wake development, a consistent evaluation of these quantities would require additional treatment of turbine inflow conditions and unresolved intra-farm wake interactions, which are beyond the scope of the current study. We therefore discuss their role in the interpretation of the results but do not present a dedicated power analysis. Finally, we have expanded the Discussion and Conclusions to clarify the recommended use of the model, the interpretation of the proposed parameter values, and the limitations that should be considered before applying the approach to realistic wind-farm and cluster-wake simulations.

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.

We thank the reviewer for highlighting this recent development. We are aware of the axial-induction correction proposed by Vollmer et al. (2024), and we have now included it in our discussion of recent developments in wind farm parameterisations. This correction is not implemented in the present study, although we employ a modified expression for the turbine-induced force. Its implementation, together with subgrid-scale wake modelling, may be considered in future model developments.

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.

We agree with the reviewer that the original wording could suggest that these are similar mechanisms. To avoid this ambiguity, we have removed the sentence from the revised manuscript.

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.

We thank the reviewer for this positive observation. The formulation of the transfer rate of the LKE sink to the PBL TKE tendency allows the parameterisation to respond to the atmospheric stability state, which is a potential advantage over other WFPs. We have highlighted this feature in the revised manuscript. However, because the present study is limited to neutral atmospheric conditions, its performance across different stability regimes is not evaluated. We have therefore identified such an evaluation, together with the inclusion of a subgrid-scale wake model, as priorities for future work.

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.

We thank the reviewer for highlighting these important contributions. We agree that the developments proposed by Du et al. (2025, 2026) represent a significant advancement in wind farm parameterization modelling, particularly regarding the representation of subgrid wake effects. We have now included and discussed them in the Introduction together with other recent developments in wind farm parameterizations. We also highlight their potential relevance for future developments of the LKE framework in the Discussion section.

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.

We agree with the reviewer. Some of the behaviour identified by Radünz et al. (2026) is also seen in our results. Improving the representation of turbine-added TKE alone may not ensure realistic wake recovery. We have extended the discussion to acknowledge this limitation and to emphasise the role of both unresolved momentum gradients and the effect of PBL scheme in wake recovery of the mesoscale model.

Methods

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

By "complexities", we refer to the difficulty of validating wake-generated TKE against field measurements due to (i) the limited availability of TKE observations in and downstream of wind farms, (ii) the difficulty of discern errors associated with the mesoscale model background flow from those of the wind farm parameterization itself. To clarify this point, we have revised the text in the Discussion section.

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

We have now explicitly mentioned this inside the Figure.

Results

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.

We have now incorporated this suggestion.

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.

The label was rendered incorrectly in the submitted version and should read L_{WF}, which denotes the wind farm length. This has been corrected in Figure 4 and in all manuscript's Figures. In addition, we have included the Fitch-100-adv case in Figure 4. To accommodate the additional results, the y-axis limits were adjusted accordingly.

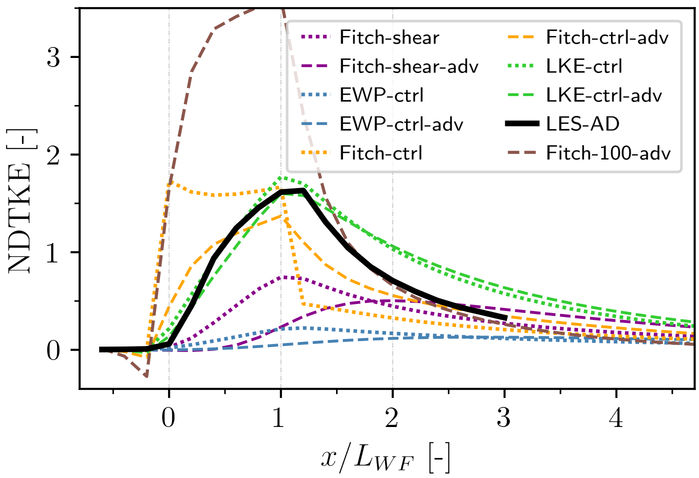

Figure 4: Comparison of the streamwise NDTKE (Eq. 12) using different parameterizations: Fitch with TKE injection (Fitch-ctrl) and only shear contributions (Fitch-shear), LKE (LKE-ctrl) and the original EWP (EWP-ctrl), with LES-AD as the baseline. The impact of TKE advection enabled (dashed lines; suffix "-adv") and disabled (dotted lines) is shown. Vertical grey lines indicate the location of the y-transects referenced in Fig. 2, and Table 2 provides the simulation's details.

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).

Following the recommendation, we have now added the Fitch-ctrl-adv (25% of the TKE source) case to Figure 5. Regarding the wake-recovery metric, we agree that the normalised wind speed does not fully isolate the streamwise recovery rate and that part of the observed spread originates from differences in the velocity deficit at the wind-farm exit. Metrics based on the streamwise gradient of the wind speed, similar to those used in recent studies , could provide additional insight into the recovery process. However, the main objective of Figure 5 is to illustrate the general behaviour of the proposed parameterisation and the influence of the new turbine-induced TKE, rather than to provide a detailed analysis of wake-recovery dynamics. For this reason, we have retained the current presentation while expanding the discussion on this matter.

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.

We agree that the downstream evolution of the wind speed is controlled both by the momentum extraction within the wind farm and by the subsequent wake recovery process. As the reviewer points out, the larger velocity deficit at the wind-farm exit in the mesoscale simulations indicates that part of the discrepancy with the LES is already established inside the farm where the intra-farm wake losses are not adequately capture by the WFPs. To

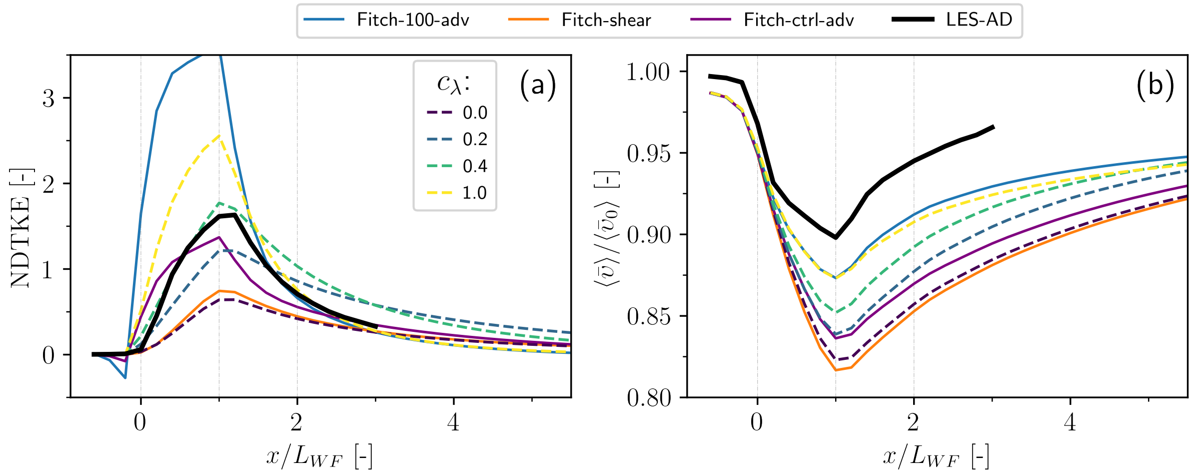

Figure 5: Effect of varying c_λ values on the (a) NDTKE and the (b) hub-height wind speed deficit in the LKE-range simulations with a constant momentum sink distribution ($\sigma_0 = 0.6\, r_0$). The LES-AD, Fitch-100-adv, Fitch-ctrl-adv and Fitch-shear results are shown as a reference (see Table 2 for details). Vertical grey lines indicate y-transects referenced in Fig. 2.

capture the intra-farm wake losses more accurate, a subgrid wake model is needed or another way to ensure that the momentum extracted by the whole wind farm equals the reference LES. Although the main focus of the present study is on the role of turbine-induced turbulence on the wind speed, we have now expanded the discussion to explicitly acknowledge the contribution of wake losses and momentum extraction to the velocity deficit.

12. Figure 6.

a Consider including the profiles at one more streamwise coordinate, perhaps at $x = L_{wf}/2$ or even 0, to assess the streamwise evolution of the profiles.

b Why is P_w maximum at hub height?

Following the reviewer's recommendation, we have now added the profiles at $x = L_{\mathrm{WF}}/2$, which are shown in panels (d), (e), and (f) of the revised Figure 6. Regarding the maximum of P_w at hub height, this behaviour is expected because the source Q from the LKE (tracer equation) is proportional to $\langle \bar{f} \rangle \langle \bar{v}_h \rangle a_x$, which reaches its largest values near the turbine hub height where the turbine forcing is concentrated. For consistency with the updated manuscript, we have also renamed P_w in the figure to $\mathrm{PBL}^{\mathrm{LKE}}$, reflecting that this term represents the source contribution from the LKE model to the PBL TKE tendency.

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.

The reviewer is right, the purpose of those runs was to illustrate the robustness of our new proposed model. We agree that their presence could be misinterpreted. Following the reviewer's recommendation, we have removed these cases from Figure 7 and revised the associated discussion accordingly.

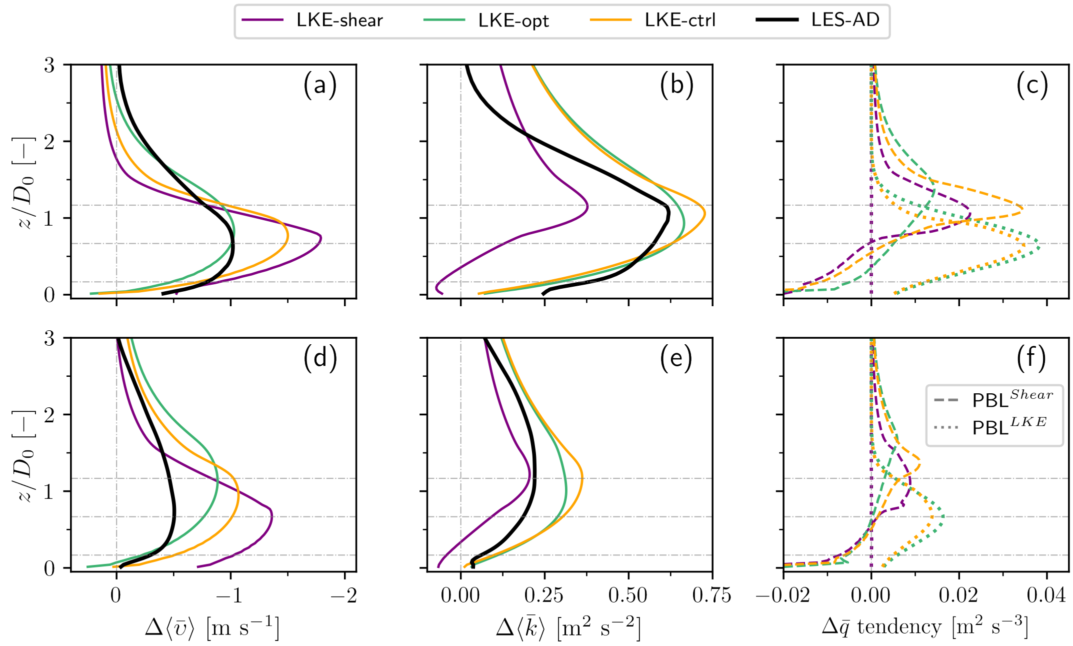

Figure 6: Vertical profiles at the (a–c) last turbine row and at (d–f) $x = L_{\mathrm{WF}}/2$ (y-transect 1 and 2 in Fig. 2, respectively) showing differences (simulations without minus with wind farms) in (a, d) wind speed, (b, e) TKE, and (c, f) Twice TKE production ($\bar{q}$) by shear (PBL$^{\mathrm{Shear}}$; dashed lines) and from the LKE parameterisation (PBL$^{\mathrm{LKE}}$; dotted lines) for LKE-shear, LKE-ctrl and LKE-opt simulations (refer to Table 2 for details). Wind speed and TKE results from the LES-AD simulations are included for reference. Horizontal grey lines mark the rotor's top, bottom, and hub heights.

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.

Since the wind-farm parameterisation (For example Fitch-shear in the figure) are operating with the same configuration (parameters and grid spacing), differences in wake recovery are expected to be influenced by how each PBL scheme represents turbulent mixing. In this sense, the representation of TKE and the resulting eddy diffusivities can affect the streamwise rate of wake recovery, with larger eddy diffusivities generally leading to stronger momentum mixing and faster recovery. However, the wake evolution is also influenced by the wind-speed deficit present at the wind-farm exit. Consequently, differences observed further downstream cannot be attributed solely to the turbine-induced TKE as also noted in . We have now expanded the discussion around Figure 7 to clarify the combined roles of TKE, PBL mixing, and the momentum deficit. A more comprehensive assessment of the relative importance of these processes would require a dedicated study focusing on the interaction between PBL schemes and wind farm parameterisations, which is beyond the scope of the present work.

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

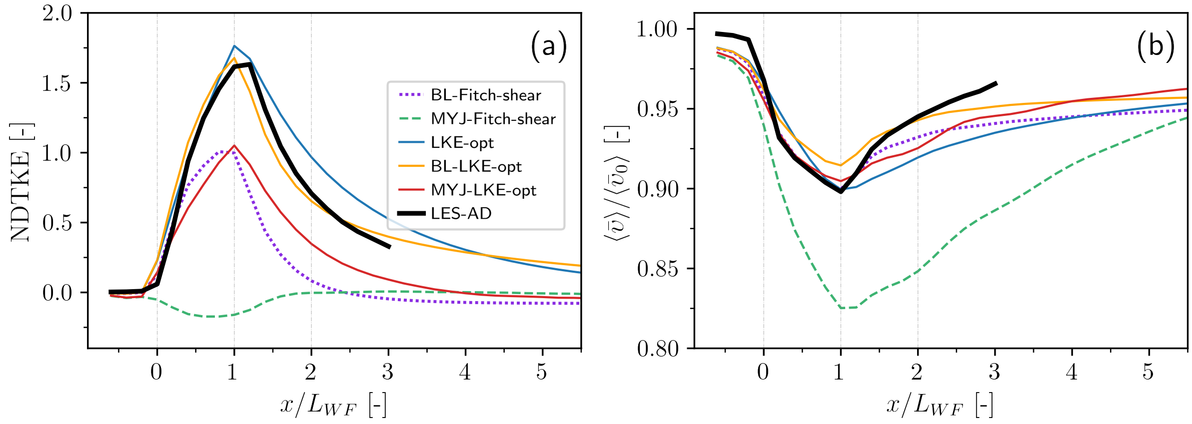

Figure 7: Similar to Fig. 5, but for the LKE and Fitch parameterizations using the MYJ (dashed lines) and BouLac ("BL" dotted lines) PBL schemes. Refer to Table 2 for details of each simulation.

there could be a shift in which simulation better matches the LES.

We agree that differences in the upstream wind speed influence the comparison both within and downstream of the wind farm. In the present study, we chose to analyse the results directly as produced by the mesoscale simulations rather than applying a correction or normalisation to the inflow conditions. As discussed in the manuscript, the upstream wind-speed bias may be related to the combined effects of the WRF horizontal diffusion and the momentum extraction associated with the wind-farm parameterisation. We acknowledge that a closer agreement with the LES upstream of the wind farm could modify the perceived differences in the velocity deficit at the farm exit and downstream. This point has now been clarified in the revised results Section.

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

Following the reviewer's recommendation, we have adopted a horizontal layout for Figures 5 and 7.

Discussion

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

Addressed in point 6.

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.

We thank the reviewer for this suggestion. We have revised the statement to include the vertical resolution of the LES and have added references supporting the adequacy of the selected grid spacing for the simulation of a near-neutral atmospheric boundary layer. To avoid duplicating the resolution analysis in the main text, we now refer more explicitly to Appendix D, where the adequacy of the LES resolution is evaluated in detail. This assessment shows that more than 85% of the total TKE is resolved within and above the rotor area, satisfying the criterion proposed by Pope (2004), and that the resolved TKE differs by approximately 2.2% from the corresponding 10m-resolution simulation of García-Santiago et al. (2024) using the same turbine type.

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?

We thank the reviewer for pointing this out. The wording "satisfactory result" was imprecise and lacked references. Our intention was to refer to the downstream distribution of turbine-induced TKE in the Fitch parameterization. We have revised the sentence , now it reads the following in the updated manuscript:

> This leads to immediate injection of TKE at the turbine location, which requires advection of TKE from the PBL to transport TKE further downwind
>
> ~~in an attempt to achieve satisfactory results away from the turbine locations, as indicated by previous studies.~~
>
> and avoid excessive TKE concentrations within turbine-containing grid cells (Archer et al., 2020).

20. "gradual and measured release of TKE". Perhaps "release" could be replaced by "shear production".

We agree with the reviewer. We have now changed the text to 'production of TKE'

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

It is now omitted.

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).

We thank the reviewer for this comment. We agree that the wording was ambiguous and could lead to confusion regarding the meaning of the term "LKE source". In this paragraph, the term refers to the turbine-induced production term Q in the prognostic equation for LKE. The statement regarding the approach of Fitch et al. (2012) was intended to refer only to an alternative formulation for parameterising this turbine-induced source term, rather than to the transfer of LKE to the PBL TKE tendency. The latter process is the component through which the parameterisation can respond to atmospheric stability and therefore should not be conflated with the definition of Q. To avoid this misunderstanding, we have now revised the corresponding text.

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.

We agree with the reviewer. The discussion has been expanded to include the recent developments of Du et al. (2025, 2026), which provide promising approaches for representing subgrid wake effects. In addition, we have broadened the discussion to include other recent developments addressing subgrid wake interactions and induction effects. These approaches are highly relevant because they improve the estimation of turbine inflow conditions and therefore may enhance the representation of both momentum extraction and turbulence generation within wind farm parameterizations.

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

Indeed, the wording "direct and practical" was imprecise. Our intention was to indicate that the proposed formulation is relatively simple, relies on readily available model variables, and introduces a single calibration coefficient. We have now revised the text accordingly and now it reads:

> Although alternative parameterizations of λ could be formulated based on other definitions of characteristic velocity and length scales, the present form (Eq. 11) is is relatively simple to implement and introduces a single calibration coefficient ...

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.

Unlike the Fitch parameterization, which depends on the TKE advection of the PBL scheme, EWP-LKE includes advection trough its own prognostic LKE equation. Therefore, EWP-LKE can be coupled with 1.5-order PBL schemes regardless of whether they include TKE advection. We have clarified this point in the manuscript.

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.

We thank the reviewer for this suggestion. We agree that the LKE framework would likely benefit from coupling with a subgrid wake model such as those proposed by Du et al. (2025, 2026). At present, EWP-LKE does not account for turbine-to-turbine wake interactions within a grid cell and therefore does not explicitly represent the dependence of turbine inflow conditions on wind direction and wind farm layout. Since the TKE source term of the LKE model is linked to turbine thrust, a more realistic representation of subgrid wake effects would be expected to improve the estimation of both momentum extraction and wake-generated turbulence. We therefore consider the incorporation of such approaches to be a promising direction for future development. Several aspects of Du et al. (2025) are conceptually aligned with our previous work on wind farm parameterizations (García-Santiago, 2024; García-Santiago et al., 2026), while the methodology proposed by Du et al. (2026), which shares similarities with actuator wind farm (AWF; van der Laan et al. (2023)) model may also provide valuable elements for future implementations.

Conclusions

27. "Introducing TKE from turbines": Perhaps "Introducing TKE from turbine wakes".

Added.

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

We have addressed this comment in point 6

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.

We agree with the reviewer in the use of the 'release' term. We have modified the entry for 'produced' in the Conclusions section.

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.

We thank the reviewer for this suggestion. Indeed, the formulation of the LKE parameterisation has the potential to respond to different atmospheric stability regimes through its coupling to the PBL TKE tendency. However, because the present study only evaluates neutral atmospheric conditions, we prefer not to include this feature among the main conclusions, as its performance under stable and unstable conditions has not yet been assessed. Instead, we have highlighted this capability in the discussion and identified its evaluation across different stability regimes as an important direction for future work. Regarding the line numbering, we used the standard formatting provided by the official Copernicus manuscript template.

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.

We thank the reviewer for the comment. We removed the part of "greater deficits than those observed in LES-AD" and remind the reader that we are referring to 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.

We thank the reviewer for pointing out the ambiguity. We have modified the text for: "The streamwise development of the wind speed deficit in the Fitch simulations under MYJ (MYJ-Fitch-shear) resemble those of MYNN, without TKE advection (Fith-shear in Fig. 5), specifically at the exit of the wind farm.

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