Response to Reviewer

We thank the reviewers 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 2

The authors describe a novel method to account for the added TKE by wind turbines in a mesoscale model (WRF) via the introduction of a variable called latent kinetic energy (LKE), such that its evolution resembles that of the wake production term in the TKE equation. The formulation is derived via the double-averaging approach of Raupach and Shaw (1982). The LKE formulation is then inserted in the WRF model and the results are compared with those obtained with LES. Favorable comparisons emerge and the sensitivity to a few tuning parameters are presented.

Overall, the study is very interesting, relevant, and original. It has been the most exciting paper that I have reviewed in a long time! It should definitely be published eventually.

The methods are, however, rather cumbersome to explain and at times not convincing. There are so many steps and additional assumptions/simplifications between the original equations, which were rigorously derived, and the final product that is inserted in the WRF (the equation for C) that I am not convinced that the C variable is what we think it is anymore.

This is possibly not a concern when the results look good, as they do for the simple neutral case presented here; but when things get complicated (non-neutral stability, wind direction and speed variability, diabatic effects, irregular layouts, etc.) and the results no longer look good, I am afraid that interpreting the results will be impossible because the connection with the real physical meaning of LKE and TKE may be lost.

I feel that the paper might not be ready for "show time" yet and that a general simplification of the approach is needed (see comment 6).

We sincerely thank the reviewer for the encouraging assessment and constructive criticism. We agree that the original formulation did not clearly separate the exact double-averaged budgets from the reduced closure implemented in WRF. We have therefore reorganised Section 2 to distinguish the physical motivation, closure assumptions, and parameter calibration, while retaining the detailed derivations in the Appendix.

The present study is intended as a proof of concept under a controlled neutral atmospheric stability configuration. We agree that additional evaluation is needed for non-neutral stratification, variable wind direction and speeds, and irregular wind-farm layouts. Testing all of these conditions is beyond the scope of the present work, but we have stated this limitation explicitly. Future developments will include coupling the formulation with a sub-grid wake model so that the layout and wind direction interactions can be represented more directly. The present contribution extends the EWP framework by introducing a prognostic representation of wake-induced turbulence, which was previously absent, and demonstrates that this representation can be used with different 1.5-order PBL schemes rather than being tied to a single PBL scheme. The latter capability is evaluated in the sections following Section 5.2 and constitutes an additional contribution of the study. We hope that the clearer formulation, the more explicit statement of assumptions and limitations, and the extended evaluation in the revised manuscript address the reviewer's principal concern.

Major Remarks

1. The authors use the notation $\overline{(\cdot)}$ for ensemble averages, but in mesoscale models the variables in the Reynolds-averaged Navier–Stokes equations are intended to be time averages, not ensemble averages. Ensemble averages are used in laboratory settings where the same experiment is repeated n times and then ensemble averages are calculated over all experiments; in real world applications for which mesoscale models are employed, this is obviously not possible and that is why the variables are intended to be time averages.

   The paper by Raupach and Shaw (1982), which pioneered the double averaging approach, in fact uses time averages, not ensemble averages. Last but not least, in the weather forecasting community the term "ensemble average" is reserved for averaging over several instances of the same simulation but with small changes from one another, in either the initial conditions, the parameterization choices, or the resolution.

   For the sake of clarity and to avoid confusion, I suggest that all instances of "ensemble averag*" be replaced with "time averag*". This would also make the two separate concepts of time versus space averages even clearer.

   We thank the reviewer for highlighting the potential ambiguity in the term "ensemble average". In this manuscript, the term denotes the statistical expectation over possible realizations of the flow, consistent with the statistical formulation of turbulence closure frameworks used in atmospheric boundary-layer models (Monim and Yaglom, 1971; Mellor and Yamada, 1982). This distinction is relevant because the derivation of the closure equations requires an averaging operator satisfying the Reynolds rules. A finite running-time average does not generally satisfy all of these rules exactly (Monim and Yaglom, 1971), although a sufficiently long time average (larger than a model time step) may approximate the ensemble mean under assumptions of stationarity. We acknowledge that Raupach and Shaw (1982) employ a time average in their double-averaging formulation; in our derivation, the corresponding first operator is instead interpreted as the statistical ensemble average appropriate to the turbulence closure, while the second operator is the spatial average. We have retain the term "ensemble average" and added a note in Appendix stating that, although the double-averaging derivation is based on Raupach and Shaw (1982), we adopt the ensemble-averaging notation consistent with turbulence closure theory.

2. The double-averaging concept for LKE needs further clarifications. As far as I can tell, the idea of splitting the time-average component $\overline{k}$ into a spatial mean $\langle\overline{k}\rangle$ and a fluctuation $\overline{k}''$ comes from the presence of sub-grid features, like wakes in Fig. 1.

   The grid value $\overline{k}_{ijk}$ is the output of a mesoscale model, which is a value representative of the entire grid cell ijk, but there are actually several different sub-grid values $\overline{k}^1_{ijk}$, $\overline{k}^2_{ijk}$, etc.

   It makes sense that, if we want to represent the effects of sub-grid wakes, at grid cell ijk we should look at the spatial average $\langle\overline{k}\rangle$. However, practically speaking, it is not clear how this spatial average should be performed.

   In the case of Fig. 1, there are 4 values of average TKE. Is $\langle\overline{k}\rangle$ equal to the sum of the 4 values divided by 4? Or, is the grid cell ijk further subdivided into smaller (regular or not) grid cells and then $\langle\overline{k}\rangle$ should be the area-weighted average? Should the value $\overline{k}_{ijk}$ be used at all in this average? Or perhaps it does not really matter how exactly the spatial average is calculated?

Regardless, it appears that

$$\langle\overline{k}\rangle \neq \overline{k}.$$

We will get back to this inequality shortly.

We thank the reviewer for highlighting this ambiguity. We agree that the original formulation did not distinguish sufficiently clearly among the different TKE quantities used in the derivation. We have therefore revised Section 2.1 to distinguish three distinct concepts. Firstly, $\overline{k}(\mathbf{x})$ denotes the TKE from the ensemble-average at a particular location. This is the statistical TKE that could, under suitable stationarity assumptions, be estimated from sufficiently long measurements. This spatially varying quantity is not directly available in the mesoscale model. Secondly, the quantity that should represent the TKE of a mesoscale grid cell is its grid-volume average,

$$\left\langle\overline{k}\right\rangle_{ijk} = \frac{1}{V_{ijk}} \int_{V_{ijk}} \overline{k}(\mathbf{x})\,\mathrm{d}V.$$

Evaluating this integral explicitly would require a microscale model capable of resolving the wakes and their variation throughout the grid cell. Thirdly, k_{ijk}^{PBL} is the TKE predicted by the PBL parameterisation and it is the only turbulent kinetic energy related output of the mesoscale model. It is not obtained by explicitly averaging a resolved sub-grid TKE field, but is a parameterised estimate intended to represent the unresolved TKE of the grid cell. Under the conventional assumption of horizontal homogeneity, spatial variations within the grid cell are neglected. In the presence of wind-turbine wakes, however, this assumption is no longer valid and, as the reviewer correctly notes,

$$\left\langle\overline{k}\right\rangle_{ijk} \neq \overline{k}(\mathbf{x})$$

in general. The objective of the LKE parameterisation is therefore not to calculate the individual sub-grid TKE values or to perform the spatial integral explicitly. Instead, it parameterises the aggregate effect of the unresolved wake heterogeneity so that

$$k_{ijk}^{\mathrm{PBL+LKE}} \approx \left\langle\overline{k}\right\rangle_{ijk}.$$

We have revised Section 2.1 to make these three quantities and their relationships explicit, and we have removed the former Eq. (3), which contributed to the confusion.

3. L. 230: "λC represents the sink of LKE and corresponding source of TKE".

This is very important and yet not justified (or not explained clearly). The fundamental assumption here is that the sink of LKE goes entirely into TKE.

How valid is this assumption?

You suggest a link between the two (LKE and TKE) via the $\overline{P}_w$ term but, as I discuss at issue 12 below, this is not entirely correct, or at least not well explained.

The objective of the parameterisation is to improve the representation of the spatially averaged TKE, $\langle\overline{k}\rangle$, in a mesoscale grid cell containing unresolved wake-induced heterogeneity. The relevant budgets are therefore the spatially averaged TKE equation, Eq. (A7), and the LKE ($\frac{1}{2}\langle\bar{u}_i''\bar{u}_i''\rangle$) equation, Eq. (10). The wake-production term $\langle\overline{\mathcal{P}}_{\mathrm{w}}\rangle$ appears with the same magnitude and opposite signs in these two budgets. It is a sink in the LKE budget equation and a source in the budget equation for $\langle\overline{k}\rangle$. The modelling assumption concerns the closure of the wake production term:

$$\lambda C \approx \left\langle\overline{\mathcal{P}}_{\mathrm{w}}\right\rangle.$$

Accordingly, λC is removed from the simplified LKE budget (the tracer equation) and added with the opposite sign to the PBL-scheme TKE tendency. It does not imply that every possible sink of LKE is converted into the spatially averged TKE, but it assumes that the particular LKE loss represented by λC corresponds to energy transfer identified by $\langle \overline{\mathcal{P}}_{\mathrm{w}} \rangle$. We have revised the text around line 230 to clarify this distinction and now state that "λC parameterises the transfer term that acts as a sink of LKE and is added with the opposite sign to the PBL TKE tendency."

4. Fig. 3 and 4: clearly the added TKE is too much (about 50% too high after $2L$) and reaches too far downstream.

We agree that, for the baseline parameter settings (hence, the suffix "-ctrl" for control simulations) used in Figs. 3 and 4, the LKE parameterisation overpredicts the TKE approximately at x = $2L$ downstream. These figures use $\sigma_0 = 0.6r$, where r is the rotor radius, to emulate a vertical distribution of the momentum sink similarly used by the Fitch parameterisation. We also use a baseline value of $c_\lambda = 0.4$, which controls the rate at which LKE is transferred to the PBL TKE. Consequently, the magnitude and downstream extent of the added TKE shown in these figures are specific to this initial parameter configuration and should not be interpreted as a final calibration of the model. Moreover, the PBL-scheme TKE tendency is influenced not only by the LKE transfer, but also by the shear generated by the imposed momentum deficit. The LKE parameterisation can modify the vertical distribution of this momentum sink, similarly to the EWP approach, and therefore indirectly affects the shear production of TKE. We have revised the text to highlight that these are control/baseline simulations.

5. Fig. 5: It is not clear how the wind speed deficit is treated in these runs. Please add a quick reminder of how the EWP and Fitch treat it.

Regardless, from Fig. 5b, the effect on the wind speed deficit is significantly worse for lower values of c_λ. Basically, to get better TKE than with Fitch, the price to pay seems to be an excessive wind speed deficit.

This is a big problem and it should be investigated because of its implications on wind power predictions.

We have corrected the missing labels in Fig. 5. The quantity shown in Fig. 5b is the hub-height velocity ratio between the wind-farm and no-wind-farm simulations. We have also added a short reminder of how the EWP and Fitch schemes represent the momentum deficit. The trend identified by the reviewer is the expected response to varying c_λ. Reducing c_λ slows the conversion of LKE into PBL TKE, thereby extending the LKE wake downstream. Because less TKE is available for turbulent mixing, the momentum deficit also recovers more slowly. Figure 5 is only included to demonstrate this parameter sensitivity, and not for the purpose of any sort of final parameter selection. The calibration presented in the following sections considers c_λ together with σ_0, and shows both good agreement with the aggregated TKE and wind-speed deficit from the LES reference.

6. The proposed method is convoluted and the logics behind it is hard to understand and possibly even harder to explain.

Here is my best attempt at explaining this approach:

(a) The traditional TKE budget (Eq. 1 and A6) in the presence of wind turbines can be modified to account for their added TKE via an extra term S_{wt} (Eq. 3).

(b) To obtain this term, the spatial averaging concept is applied to derive an equation for LKE (Eq. A10).

(c) We can also derive an equation for the spatial average of TKE (Eq. A7). Note that this is NOT TKE.

(d) By comparing A10 and A7, there is a term that appears in both equations with opposite signs, called $\langle \overline{P}_w \rangle$ (but it's not really there in the TKE equation). This is a good candidate for S_{wt}. This term does not depend on any turbine characteristic.

(e) The term $\langle \overline{P}_w \rangle$ behaves very similar to LKE itself in the far wake (Fig. 1b), suggesting that we can parameterize $\langle \overline{P}_w \rangle$ as LKE itself (with proper scaling).

(f) To parameterize $\langle \overline{P}_w \rangle$, the LKE equation A10 is replaced with a much simpler formulation (Eq. 7), that allows LKE to be treated as a scalar variable in WRF (this is a big leap). The loss term is proportional to LKE itself ($-\lambda C$). There is also a source term Q.

(g) Now we need to parameterize λ as a function of c_λ (Section 2.2.1) and the source term Q as a function of the turbine drag force (Section 2.2.2).

(h) The results depend on the value of c_λ and the choice of the PBL scheme, among other tuning parameters (like σ_0).

(i) The S_{wt} term in the TKE equation is then equal to $+\lambda C$.

Needless to say, this is complicated and at times not entirely convincing.

The steps from 1) to 5) are not necessary, really. Eq. 7 does not really need to be justified and, to be honest, the previous 5 steps do not justify the form of Eq. 7 when compared to Eq. A10 anyway.

The manuscript would benefit from a simplification of the approach: start with Eq. 7 and then focus on the parameterizations required for it. You do not need to spend pages of derivations to study the term $\langle \overline{P}_w \rangle$.

I stopped reviewing after Section 5.2.

We thank the reviewer for this summary. It captures the broad sequence of the original presentation and confirms that the distinction between the exact double-averaged budgets and the reduced LKE model was not sufficiently clear. Equation (7) is not intended to be a direct simplification of Eq. (A10). It is a reduced closure model. The role of the double-averaging derivation is to define LKE physically and to identify the transfer between wake-induced spatial variations of the ensemble-mean flow to spatially averaged TKE. This transfer appears with equal magnitude and opposite signs in the corresponding exact energy budgets. The reduced model represents it through λC, while Q parameterises the generation of LKE by turbine forcing. We retain this theoretical basis because beginning directly with Eq. (7) would leave the physical meaning of LKE and its use as the TKE source unjustified. However, we have reorganised and shortened the main text so that the derivation provides a clearer motivation. The mathematical details remain in the Appendix. We have also clarified that $\langle \overline{k} \rangle$ is the spatially averaged TKE. It is distinct from the local field $\overline{k}(\mathbf{x})$ and from the prognostic PBL TKE, which is a closure-model estimate of the unresolved grid-cell TKE. The goal of the LKE parameterisation is to improve the latter in heterogeneous wake flows by approximating to $\langle \overline{k} \rangle$. To avoid conflating these quantities, we have removed the generic S_{wt} notation and revised the definitions throughout Section 2.

Minor Remarks

7. Eq. 2: The term $\langle \overline{T}_w \rangle$ is not in Eq. A7.

Fixed.

8. Eq. 2: Why is there only $\langle \overline{T}_t \rangle$, and not also $\langle \overline{T}_d \rangle$ and $\langle \overline{T}_p \rangle$?

We thank the reviewer for pointing out the inconsistency. We addresed the issue.

9. Eq. 2 and Eq. A7: Why is the term

$$\langle \overline{P}_w \rangle = \left\langle \overline{u_i'' u_j''} \frac{\partial \overline{u_i}''}{\partial x_j} \right\rangle$$

a "wake production" term?

It looks like the shear-production term but for the dispersive components. Why is this term "the primary missing source of turbine-induced TKE"?

The expression quoted by the reviewer is not the wake-production term used in our derivation. The intended term is

$$\left\langle \overline{\mathcal{P}}_{\mathrm{w}} \right\rangle = -\left\langle \left(\overline{u_i' u_j'}\right)'' \frac{\partial \overline{u_i}''}{\partial x_j} \right\rangle .$$

Following the terminology used in double-averaging studies of canopy flows, this term is referred to as "wake production" (Raupach and Shaw, 1982; Finnigan, 2000; Brunet, 2020). We have also revised the statement that this is the "primary missing source". More precisely, after neglecting $\langle \overline{\mathcal{P}}_{\mathrm{f}} \rangle$ and $\langle \overline{\mathcal{T}}_{\mathrm{w}} \rangle$ under the assumptions described in the cited studies, $\langle \overline{\mathcal{P}}_{\mathrm{w}} \rangle$ is the principal additional wake-induced contribution retained in the spatially averaged TKE budget.

10. L. 110: the term $\langle \overline{P}_f \rangle$ is obviously the only term directly associated to the turbine via the drag force f_i and its perturbations f_i'.

How can it possibly be "negligible"?

The term $\langle \overline{\mathcal{P}}_{\mathrm{f}} \rangle$ represents the correlation between turbulent velocity fluctuations and fluctuations of the turbine drag force around the ensemble mean,

$$\left\langle \overline{\mathcal{P}}_{\mathrm{f}} \right\rangle = \left\langle \overline{u_i' f_i'} \right\rangle .$$

Following Volker et al. (2015) (where the double-averaging approach was used) this term is a turbulent kinetic-energy sink which magnitude was found to be substantially smaller than the corresponding turbine-induced TKE term in the Fitch scheme and was therefore neglected in the EWP formulation, which we follow here.

11. Eq. 3 and text above it: please confirm that you are not assuming that

$$\langle \overline{P}_s \rangle = \overline{P}_s$$

etc. It would invalidate Eq. 1.

We confirm that no such equality is assumed. Equation (3), which might have caused this ambiguity, has now been removed in the revised manuscript.

12. L. 135: It is not possible that the term $\langle \overline{P}_w \rangle$ be in both the TKE and the LKE budgets because there is no space averaging in the TKE budget (Eq. A6).

This appears to be a foundational step to explain the exchange of energy between TKE and LKE, but it is not true.

See also comment 3 above.

If we take the space average of Eq. A6 to obtain Eq. A7, then the term $\langle \overline{P}_w \rangle$ appears with a minus sign. But this is not TKE ($\langle \overline{k} \rangle \neq \overline{k}$).

Thus no term appears in both the TKE and LKE budgets with opposite signs.

As clarified in our response to Comment 3, the relevant budgets are not the local TKE equation, Eq. (A6), and the LKE equation. They are the spatially averaged TKE equation, Eq. (A7), and the LKE equation. We agree that

$$\left\langle \overline{k} \right\rangle \neq \overline{k}$$

in a spatially heterogeneous flow; indeed, this distinction motivates the parameterisation. The term $\left\langle \overline{\mathcal{P}}_{\mathrm{w}} \right\rangle$ appears with equal magnitude and opposite signs in the budgets of $\left\langle \overline{k} \right\rangle$ and LKE, and therefore represents an energy transfer between these two spatially averaged reservoirs. The closure

$$\lambda C \approx \left\langle \overline{\mathcal{P}}_{\mathrm{w}} \right\rangle$$

parameterises this transfer. We have revised the manuscript to refer consistently to spatially averaged TKE.

13. Eq. A6: I suggest that you name each term of the time-average budget explicitly, like you did in Eqs. A7 and A10 for the spatial-average budgets.

There seems to be no $\overline{P}_w$ term.

Implemented as suggested.

14. L. 148 and footnote and L. 593: the term $\langle \overline{F}_d \rangle$ is not in RS1982 and cannot be in it because they did not have turbines. Do you mean $\langle \overline{C}_d \rangle$?

We have addressed this inconsistency. Now it reads:

> the budget for canopy flows can simply be reduced to a balance between $\langle \bar{\mathcal{P}}_{\mathrm{w}} \rangle$ and $\langle \bar{\mathcal{C}}_{\mathrm{d}} \rangle$.

The footnote now says:

> Notice that in Raupach and Shaw (1982) the actual $\langle \bar{\mathcal{C}}_{\mathrm{d}} \rangle$ term is represented by $\frac{1}{\rho} \langle \bar{u}_i \rangle \left\langle \frac{\partial \bar{p}''}{\partial x_i} \right\rangle$, a term derived from $\left\langle \frac{\bar{u}_i''}{\rho} \frac{\partial \bar{p}''}{\partial x_i} \right\rangle$.

15. L. 163: the sentence

"suggesting that the depletion of LKE by $\langle \overline{P}_w \rangle$ is inversely proportional to a characteristic time scale"

needs to be expanded and explained.

This is a crucial link between LKE and TKE.

We agree that this modelling step required further explanation. The similar streamwise evolution of LKE and $\langle \overline{\mathcal{P}}_{\mathrm{w}} \rangle$ in Fig. 1b suggests the empirical closure

$$\left\langle \overline{\mathcal{P}}_{\mathrm{w}} \right\rangle \approx \lambda K_{\mathrm{LKE}},$$

where

$$K_{\mathrm{LKE}} = \frac{1}{2}\left\langle \overline{u}_i''\overline{u}_i'' \right\rangle$$

and λ has units of inverse time. Thus, $\tau_\lambda = \lambda^{-1}$ represents the characteristic time scale over which LKE is transferred to spatially averaged TKE. This is a closure assumption supported by the observed similarity of the LES-derived profiles. We have now expanded the text to clarify this point.

16. L. 197: the entire rotor diameter is included for each turbine, or just the fraction of it that belongs to the grid cell? Also, it should be a function of wind direction.

The entire rotor diameter is used as the characteristic length scale for each turbine. We agree that the effective length scale may depend on wind direction and turbine layout. These dependencies are not included in the present formulation and are left for future coupling with a sub-grid wake model capable of representing layout- and direction-dependent wake variations.

Edits and typos

17. L. 132: replace "deals" with "deal".

Replaced.

18. L. 209: replace "neglected" with "neglect" and "leading" with "leaving".

Replaced.

19. L. 216: explain better starting at "We neglect a fixed ...", what are you referring to?

We are referring to the force distribution in the vertical. It is change to retain.

20. L. 219: remove the ")" after r_0.

Removed.

21. L. 221: replace "lef-" with "left-".

Added.

22. L. 233: replace "tracer" with "tracers".

Replaced.

23. Fig. 5b: typo on the y-axis? What parameter is used to normalize the wind speed deficit?

There was a problem with the rendering of the figure on the portal! The Figures are now saved in another format to avoid this problem. The units in axis figures are now correctively display. Regarding this specific Figure, it is normalized by the simulations without the wind farms at the hub height ($\bar{v}_0$)

24. Fig. 6: typos on the x-axes? Perhaps the units are partially missing? What is the first legend item (missing subscript)?

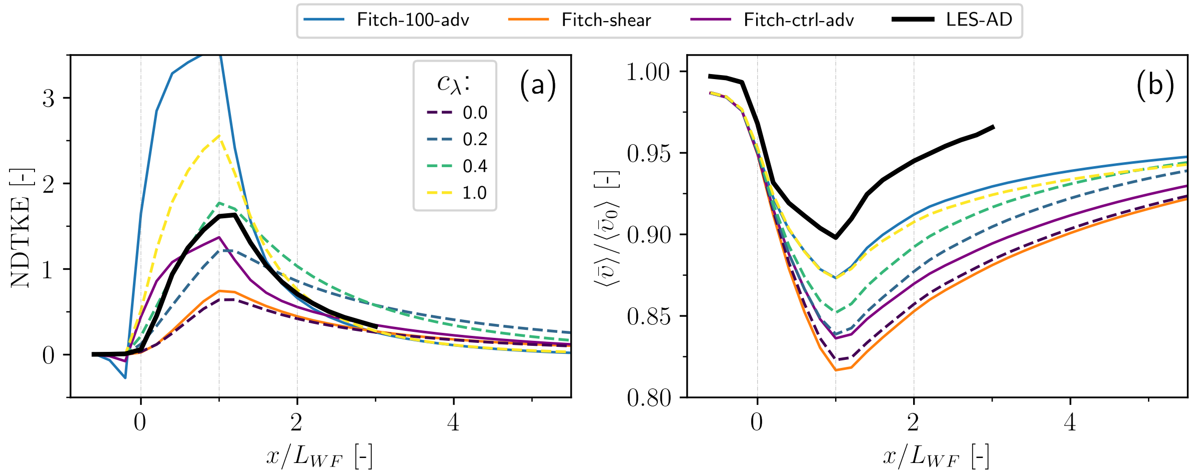

Figure 5: Effect of varying c_λ values on the (a) NDTKE and the (b) hub-height wind speed deficit in the LKE-range simulations with a constant momentum sink distribution ($\sigma_0 = 0.6\, r_0$). The LES-AD, Fitch-100-adv, Fitch-ctrl-adv and Fitch-shear results are shown as a reference (see Table 2 for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

The x-axis in Fig. 6 should be (a) $\Delta\langle\bar{v}\rangle$ [m s^{-1}], (b) $\Delta\langle\bar{k}\rangle$ [m^2 s^{-2}], and (c) $\Delta\langle\bar{q}\rangle$ tendency [m^2 s^{-3}]. The first legend in Fig. 6c (now 6f) should be PBL$^{\text{Shear}}$ and PBL$^{\text{LKE}}$. This also related to the rendering problem. In the updated manuscript units and legends of all Figures are properly rendered.

25. L. 576: subtracting "from", not "to".

Replaced.

26. L. 588: subtracting "from", not "to".

Replaced.

27. L. 580: a bar is missing over k:

$$\bar{k} = \frac{\overline{u_i' u_i'}}{2}.$$

Added.

References

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Finnigan, J.: Turbulence in Plant Canopies, Annual Review of Fluid Mechanics, 32, 519–571, https://doi.org/10.1146/annurev.fluid.32.1.519, 2000.

Mellor, G. L. and Yamada, T.: Development of a Turbulence Closure Model for Geophysical Fluid Problems, Reviews of Geophysics, 20, 851–875, https://doi.org/10.1029/RG020i004p00851, 1982.

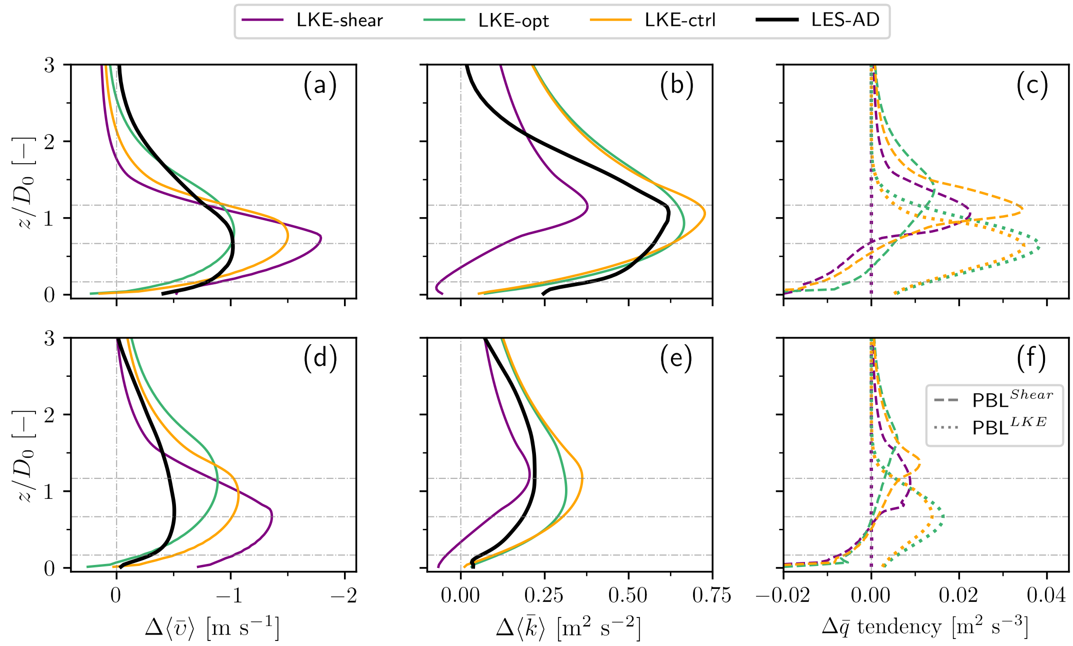

Figure 6: Vertical profiles at the (a–c) last turbine row and at (d–f) $x = L_{\mathrm{WF}}/2$ (y-transect 1 and 2 in Fig. 2, respectively) showing differences (simulations without minus with wind farms) in (a, d) wind speed, (b, e) TKE, and (c, f) Twice TKE production ($\bar{q}$) by shear (PBL$^{\mathrm{Shear}}$; 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.

Monim, A. S. and Yaglom, A. M.: Statistical Fluid Mechanics: Mechanics of Turbulence, vol. 1, The MIT press, 1971.

Raupach, M. R. and Shaw, R. H.: Averaging Procedures for Flow within Vegetation Canopies, Boundary-Layer Meteorology, 22, 79–90, https://doi.org/10.1007/BF00128057, 1982.

Volker, P. J., Badger, J., Hahmann, A. N., and Ott, S.: The Explicit Wake Parametrisation V1.0: A Wind Farm Parametrisation in the Mesoscale Model WRF, Geoscientific Model Development, 8, 3715–3731, https://doi.org/10.5194/gmd-8-3715-2015, 2015.