We want to thank referee #2 for the valuable comments. We address the comments (in italics bold) on a point by point basis.

1. in Section e 2.2.2, equation 7, the authors assume that the growth rate of the wake is proportional to $x^{1/2}$. This assumption is not correct, as the both numerical and experimental studies of wind turbine wake under the atmospheric conditions show that the wake growth rate has a linear proportionality with x. As a result, the model derivation starts with an assumption that is basically wrong. (It should be noted that this assumption is only valid for the plane wake under laminar inflow condition.)

First, It is important to state here that our focus is on the far wake development as mentioned on p. 3488 l. 13-17 and not on that in the near wake.

Regarding the far wake development, several studies have investigated the downstream wake expansion in the vertical direction, while others examine the downstream decay of the velocity deficit, $\Delta U/U_{\mathrm{re}f}$, where ΔU denotes the velocity deficit and $U_{\mathrm{re}f}$ the reference wind speed. Iungo and Porté-Agel (2014) study the velocity decay with a LiDAR in atmospheric conditions and Zang et al. (2013) study the velocity deficit decay for a neutral boundary layer in a wind tunnel. Iungo and Porté-Agel (2014) find a median velocity deficit decay of $x^{-0.28}$, whereas the fit between 2 and 16 rotor diameters in Zang et al. (2013) leads to a decay of $x^{-0.81}$. Assuming self-similarity (Tennekes and Lumley, p. 114), this would result in a wake expansion of $x^{0.72}$ for Iungo and Porté-Agel (2014) and $x^{0.19}$ for Zang et al. (2013). Both expansion rates are well below $n = 1$. Stallard et al. (2015) study the wake expansion and the velocity deficit decay behind a turbine in a water tank in a neutral, turbulent boundary layer. They find for the transverse profile a wake expansion close to $x^{1/2}$ and a velocity deficit decay of $x^{-1/2}$. Xie and Archer (2014) investigate the wake expansion with a LES model and compare their results to those from a wind tunnel experiment. They find self-similarity in the velocity deficit profile for low friction velocities. In their Fig. 14b, they could fit a non-linear wake expansion between 0 and 20 rotor diameters to a specific linear function. This example shows that the fitting of the wake expansion is (at these short distance ranges) not very sensitive to the expansion rate.

Given the spread in the estimated wake expansion coefficients n found in the literature, we use the RANS equations (Eq. (5) in the manuscript) to describe the far wake expansion. The resulting wake expansion in Eq. (7) is the same as in classical wake theory for turbulent flows (see for example Schlichting (1968) or Tennekes and Lumley (1972)). In contrast to the fits obtained in the experimental studies, which depend on the specific experimental set-up, the expansion as described in Eq. (7) of the manuscript is a function of stability, and therefore general applicable to different atmospheric conditions.

Finally, the comparison to the measurements shows (our Fig. (4)) that a fairly good agreement with the measurements is achieved for this approach.

To clarify that the formulation is used for the far wake, we propose to combine on p. 3487 l. 16–17:

> The sub-grid-scale model describes the unresolved turbulence diffusion process that results from turbulence shear production. In this model . . .

and p. 3488 l. 15–18:

> The turbine wake is normally divided in a near and far wake, where the far wake begins between one rotor diameter D_0 (Vermeer et al., 2003) and 3 D_0 (Crespo and Hernández, 1996).

to:

> The turbine wake is generally divided into a near and far wake, where the far wake begins between 1 and 3 rotor diameters D_0 (Vermeer et al., 2003; Crespo and Hernández, 1996). The parametrisation describes the unresolved far wake expansion, that is caused by turbulence diffusion. We account for the expansion in the near wake in the initial length scale.

Furthermore, we propose to add the here underlined part of the sentence p. 3488 l. 11–13:

> In Eq. (7), σ_0 denotes the initial length scale, which incorporates the near wake expansion. Equation (7) represents the vertical wake extension, resulting from turbulent diffusion of momentum and it is similar to the solution of Eq. (4.29) in Wyngaard (2010) for the dispersion of plumes. . . .

In the discussion section, we would motivate our choice and discuss further investigations. We would add the here underlined paragraph on p. 3502 l. 18:

> . . . remain unresolved.
> In the proposed parametrisation, we use the classical wake theory (Tennekes and Lumley, 1972) to describe the sub-grid-scale wake expansion. Compared to empirical fits from for example Zang et al. (2013) and Xie and Archer (2014), it offers the advantage that the wake is described as a function of stability. In this study, we have validated the approach for different wind speeds. Its performance for atmospheric stability, which requires information of the profiles, will be investigated in future.

2. The authors mention that the definition of TKE in their model is different from the one in Fitch et al. (2012). If this is the case (the definition of TKE is different in different schemes), how the authors compare their results with the ones obtained from WRF-WF scheme?

The TKE definition in the EWP and WRF-WF scheme differ, this affects only the parametrisation of the additional source term P_t. The governing equation is exactly the same (except for the source term P_t). We recall that fluctuations remain unresolved and the TKE is completely parametrised as a function of mean quantities. From a mathematical point of view the two quantities are therefore comparable.

In Sect. 5, we propose to change on p. 3497 l. 8–11:

> In a qualitative validation, we examine the simulated wind farm wake characteristics, as well as the vertical profile of the wake deficits, using results from LES simulations and wind tunnel experiments as a reference.

to:

> In a qualitative validation, we examine the velocity reduction behind the wind farm and the vertical profile of the velocity deficit. Furthermore, we discuss the vertical structure of the modelled TKE field from the discretized Eq. (3), where the additional source term $\langle\overline{p_t}\rangle$ has been parametrised in the WRF-WF scheme and neglected in the EWP scheme. We use results from independent LES simulations and wind tunnel experiments as a reference.

Furthermore, we propose to update the final paragraph of the discussion on p. 3504 l. 2–9:

> . . . of large wind farms. For the WRF-WF and EWP scheme, the PBL scheme adjusts the TKE production through the changed vertical shear in horizontal velocity. The term that represents the local turbine-induced turbulence is neglected in the EWP approach, whereas it is parametrised as an additional source term in the WRF-WF scheme. The total TKE is more than 3 times larger in the WRF-WF scheme than in the EWP scheme. Future measurement campaigns around large wind farms under suitable atmospheric conditions can help to settle this issue.

to:

> . . . of large wind farms. Both wind farm schemes use a PBL scheme that parametrises the TKE equation in terms of grid-cell averaged variables. Therefore, in the wake of the wind turbines TKE is generated by the increased vertical shear in horizontal velocity. Then, the different definition of the unresolved velocity fluctuation in the WRF-WF and EWP scheme leads to a different source term $\langle\,\overline{p_t}\,\rangle$ that is the direct consequence of the presence of a drag force. In the EWP scheme, a velocity fluctuation is defined around the ensemble-averaged velocity. With this definition $\langle\,\overline{p_t}\,\rangle$ is small and can be neglected. Instead, in the WRF-WF scheme velocity fluctuations are defined around the grid-cell averaged velocity and a parametrisation of $\langle\,\overline{p_t}\,\rangle$ is added to the model TKE equation. The simulations have shown that in the WRF-WF scheme $\langle\,\overline{p_t}\,\rangle$ dominates over the shear production and that its total TKE is several times larger than that in the EWP scheme. However, it is unclear how well the actual grid-cell averaged shear production is approximated by the shear production calculated with the PBL scheme in WRF, on which the EWP scheme relies.

Therefore, future measurement campaigns of the actual structure and intensity of the TKE field within and around wind farms under suitable atmospheric conditions can help to settle this issue.

3. In comparison with the field data, the authors only validate their model only for the mean velocity, without any comments for the prediction of the added TKE. The author should comment on this issue that which model (EWP or WRF-WF) could provide better estimation for the added TKE inside a wind farm, since the TKE in the wake is responsible for the wake recovery and has significant effects on the power output from downwind turbines.

The data-set for the Horns Rev I wind farm, contained 10 min averaged measurements and no quantitative validation of the TKE field is possible. Therefore, we compare the structure of the TKE field against that from LES simulations only qualitatively. Furthermore, we compare the internal boundary layer growth, which is related to the additional turbulence. In the discussion, we mentioned on p. 3504 l. 7–9 that future campaigns are needed to measure the structure of the TKE field.

For clarity, we propose to repeat the use of 10-min averaged wind speeds on p. 3492 l. 10:

> ... The 10-min averaged data from the two ...

and on p. 3493 l. 1:

> ... we select the 10-min averaged wind speeds ...

4. In section 2.2, the authors mention that "The expression for Pt can be found by multiplying the NavierStokes equations with the velocity fluctuation and then applying Reynolds averaging" However, in equation 4, a negative sign is missing (F_{Di} has a negative sign). As a result, the equation 4 is always negative and cannot predict the augmentation of turbulence due to the presence of the turbines.

We want to thank the reviewer for pointing this out. Indeed, the term in Eq. (4) should contain a minus sign. This has no implications on the EWP scheme and the results, since the term has been neglected in the parametrisation anyway. The source term we described on p. 3487 l.1–7 would require the simulation of the full blade structure and cannot be described by the drag force in Eq. (1). On the other hand, as pointed out by the reviewer, Eq. (4) represents a sink of TKE due to the momentum sink. This TKE term contributes to the electrical power production and mechanical losses in structures and transmission. For the Vestas V80 turbine, the absolute value of this additional term is around 30 times smaller than the additional term P_t in Eq. (16) from the WRF-WF parametrisation and can, therefore, be neglected. For example, for a wind speed of $10\,\mathrm{m\,s^{-1}}$ we have $c_t = 0.79$, $c_p = 0.43$, and $\langle \overline{u_i' u_i'} \rangle = 0.7\,\mathrm{m^2\,s^{-2}}$ at hub-height. This gives for $\langle \overline{p_{t,\text{WRF-WF}}} \rangle / \langle \overline{p_t} \rangle = 0.5\,(c_t - c_p)\,u^2/(c_t \langle \overline{u_i' u_i'} \rangle) = 32$.

In the manuscript, we propose to

1) add the minus sign to the second and third term in Eq. (4).

2) change on p. 3487 l. 1–7:

> This additional turbulence occurs directly behind the turbine blades. Its turbulence length scale ℓ is expected to scale with the blade's cord length, which is on the order of a few metres and it is therefore much smaller than that of the atmospheric flow. The smaller length scale implies a significant higher dissipation rate $\epsilon \propto (1/\ell)\overline{u_i u_i}^{3/2}$. Therefore, we assume the source term in Eq. (4) to dissipate within a mesoscale model grid-cell and neglect P_t of Eq. (4) on the grid-cell average.

to:

> This term represents a sink of TKE due to the extraction of momentum. The magnitude of this term is much smaller than the additional source term in the WRF-WF scheme (see Sect. 4.1.2). Therefore, the additional term $\langle \overline{p_t} \rangle$ in the EWP approach is neglected.

3) substitute on p. 3496 l. 16–18:

> The TKE source term from the WRF-WF scheme in Eq. (16) is much larger than the source term in Eq. (4), which comes from the different definition of TKE in the two schemes.

with:

> For the considered wind speeds the absolute value of $\langle \overline{p_{t,\text{WRF-WF}}} \rangle$ is around 30 times larger than $\langle \overline{p_t} \rangle$ defined in Eq. (4), which comes from the different definitions of TKE in the two schemes. For example, for $10\,\mathrm{m\,s^{-1}}$ with $c_t = 0.79$, $c_p = 0.43$, and $\langle \overline{u_i' u_i'} \rangle = 0.7\,\mathrm{m^2\,s^{-2}}$ at hub-height, the ratio between $\langle \overline{p_{t,\text{WRF-WF}}} \rangle$ and $\langle \overline{p_t} \rangle$ as it has been defined in Eq. (4) is 32.

It should be noted that, inside a grid cell, the heterogeneity of the flow is not resolved. It means that we cannot resolve the velocity inside the gird as shown in Fig.1a, and what we have is the averaged one over the grid cell (Fig. 1b). Therefore, all the heterogeneity inside the grid cell must be taken into account in the parametrization in order to estimate the added TKE by the turbines. Otherwise, as shown in Eq. (4) (with a negative sign), this formulation only predicts the reduction of turbulence inside the farm, which is not correct.

The definition of the velocity perturbation and the description of the averaging is an important part of the manuscript and we propose to update Sect. 2 to make it as clear as possible. The proposed text can be found at the end of the document. First, we address the individual reviewer's statements below.

It should be noted that, inside a grid cell, the heterogeneity of the flow is not resolved. It means that we cannot resolve the velocity inside the gird as shown in Fig.1a, and what we have is the averaged one over the grid cell (Fig. 1b).

We agree with the reviewer that within the grid-cell the heterogeneity of the flow remains unresolved and that only the grid-cell averaged velocity is resolved.
In Fig. 1, we intended to illustrate the difference in the velocity fluctuation that occurs from the different definition of the average velocity. In the revised text, the illustrative purpose for the definition of the velocity perturbation will be stated more clearly.

Therefore, all the heterogeneity inside the grid cell must be taken into account in the parametrization in order to estimate the added TKE by the turbines.

The main reason for including TKE is that it can be used to parametrise fluxes. These are turbulent fluxes driven by random motion, but not all heterogeneity is random. Therefore, only the random part of the fluctuation should be considered.

Otherwise, as shown in Eq (4) (with a negative sign), this formulation only predicts the reduction of turbulence inside the farm, which is not correct.

It is important to consider the parametrisation in conjunction with the WRF model and not as a standalone model.
In the EWP scheme the additional term P_t in Eq. (4) has been neglected (independent of it being a source or sink term), since this term acts only locally at the turbine and is on a spatial average very small compared to the other terms in the TKE equation (see previous comment). However, the EWP approach relies on the turbulence shear production (P_s) from the PBL scheme. Therefore, the parametrisation embedded in the WRF model predicts, as a consequence of the changed velocity shear, an increased TKE above and a decreased TKE below hub-height compared to the background flow and does not predict a reduction of turbulence inside the wind farm (Fig. 8) as has been stated by the reviewer.

Regarding the averaging, we have proposed several changes (see first specific comment of referee #1) and prepared an appendix that can also be found in the reply to referee #1.

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