In this second response to the comments of referee #1 I provide more detailed answers to some of the comments, as it was requested by the associate editor. The reviewers comments are marked in black and my answers in blue. The additional, more detailed answers are marked in green.

- The change in the horizontal mean flow direction observed in Fig 2, especially the CCW and the following CW deflections of the mean flow for the large WF needs further verification.

I think these effects are very well described in lines 205-216 and Figure 5 (crosswise tendencies):

"At the inflow all forces sum to zero and the mean flow is in a steady state. Due to the wind speed reduction upstream and inside the wind farms, the Coriolis force is reduced, so that the geostrophic pressure gradient force predominates and tends to deflect the flow counterclockwise. The vertical momentum flux divergence, however, tends to deflect the flow clockwise but this force is weaker, so that the sum of these forces is still positive. Because the wind farms are infinite in the y-direction, the perturbation pressure gradient force is parallel to the x-axis and has thus no effect on the wind direction at first. However, due to the change in wind direction further downstream inside the large wind farm, the perturbation pressure gradient force has a component perpendicular to the streamlines that tends to deflect the flow clockwise. At the end of the large wind farm the sum of all forces becomes negative, so that the flow begins to turn clockwise. Because the wind speed increases to super-geostrophic values in the wake, the Coriolis force becomes greater than the geostrophic pressure gradient force so that the flow is deflected clockwise. The most significant difference between the small and the large wind farm is that the speed deficit in the large wind farm is greater and lasts longer. This results in a greater wind direction change and thus a greater inertial wave amplitude compared to the small wind farm."

It is also interesting that there is no visible deflection of the upstream flow in the first 60-70km range (Figs 2&3).

The deflection inside the wind farm and the wake is caused by the reduction in wind speed which results in a reduction of the Coriolis force, which is linearly proportional to the wind speed. Since there is no wind speed deficit in the first 60 - 70 km of the domain, there is also no deflection.

A simulation without Coriolis force is suggested.

The crosswise tendencies, shown in Fig. 5, clearly prove that the cause for the deflection is the change in Coriolis force. The magnitude of the tendencies (0.0001 $m/s^2$) also corresponds well to the change in the v-component (~1 (m/s)/(100 km)). Performing an extra simulation as an additional proof is not necessary in my opinion.

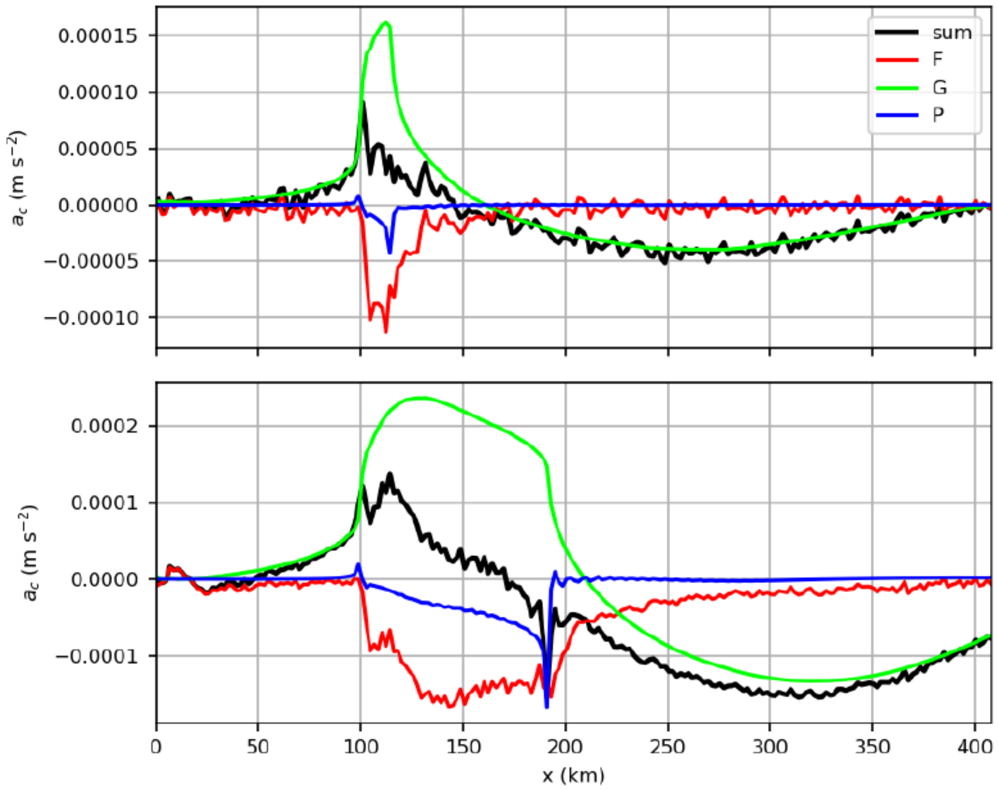


**Figure 5.** Crosswise forces (perpendicular to streamlines) at hub height along $x$, averaged along $y$ for the small wind farm (top) and large wind farm (bottom). Shown are the divergence of the vertical turbulent momentum flux (resolved + SGS) F, the geostrophic forcing G (difference between geostrophic pressure gradient force and Coriolis force) and the perturbation pressure gradient force P.

I would like to add here, that I am of course happy to perform an additional simulation without Coriolis force to underpin the results, if the reviewer insists. However, setting up a simulation without Coriolis force raises some problems and questions that I would like to name and explain shortly here:

- The forcing of the flow, that is otherwise done with the geostrophic wind, has to be done with a streamwise pressure gradient that opposes the friction in the boundary layer. This is possible in PALM but raises some other problems and questions:
- Since there is no friction in the free atmosphere, the pressure gradient has to be zero there, otherwise the flow will not reach a steady state. This raises the question of the right choice of the vertical profile of the pressure gradient, which can be arbitrarily chosen.
- Depending on the above named choice of the pressure gradient profile, the boundary layer height and also other profiles such as wind speed, wind direction and momentum flux profiles may differ significantly from the original simulation (with Coriolis force). E.g. the wind direction will be constant with height (no veer).
- As shown above, many other parameters than only the Coriolis force would be different between the original and the additional simulation. Consequently, making direct comparisons between these simulations is problematic and derived conclusions are of limited validity, in my opinion. E.g. it can not be distinguished whether the wake deflection is caused by wind veer or by the Coriolis force, because **both** are not present in the additional simulation.

- In Fig 3, the reduced wind speed within the WF due to blockage and its correlation with the farm size are expected. However, the increased wind speed by about 12% 150km down in the large WF wake similarly need further verification.

It is not completely clear, what is meant by "further verification". The text (l. 187 - 200) and Fig.~4 clearly state that the speedup is related to the inertial oscillation that is triggered by the wind farm and that the amplitude is larger for the larger wind farm. Please give more specific hints about which information shall be added.

In addition to the hub height, the vertical variation of the wind speed should also be presented.

The vertical variation of wind speed and wind direction is given in Fig. 8 and is described in lines 271-295.

In addition, a simulation without the gravitational force is suggested to identify the cause and to validate the numerical implementation.

Unfortunately this comment is not clear to me. Please clarify what is meant by "without gravitational force". Does it mean without the buoyancy term, i.e. pure neutral stratified? Which cause shall be identified with this additional simulation? The speedup in the wind farm wake is related to the inertial oscillation and not to gravity.
A simulation without gravity/buoyancy is very unrealistic. Additionally, there will be no gravity waves, which is one of the main results of this paper. Thus, I do not see the necessity to do such an extra simulation.

I would like to add some more thoughts on the requested additional simulation without gravity, to explain my last comment in more detail:
Here, I assume that "without gravity" means a simulation without buoyancy forces and thus a pure neutral stratification up to the domain top. First, I agree that performing such a simulation would give some more interesting insights, e.g. it will prove whether the pressure distribution and the blockage effect is caused by the gravity waves or also by other effects.
However, a comparison between this additional simulation and the original simulation is again problematic, because many parameters change at the same time, e.g.:

- The original simulation consists of a convective boundary layer. Without buoyancy forces in the additional simulation the boundary layer will be neutrally stratified. This results in a different boundary layer height and in different profiles of the wind speed, wind direction and turbulent fluxes.
- The above named changes will affect the wake flow and the blockage effect.
- Since a stable stratification and the buoyancy force as restoring force is needed to support the formation of gravity waves, no gravity waves will form in the - now neutrally stratified -

free atmosphere. This will presumably have a significant effect on the pressure and wind speed distribution at hub height.

- In reality the free atmosphere is always stably stratified. I therefore used a stably stratified free atmosphere with a lapse rate corresponding to the international standard atmosphere to obtain a simulation that is as realistic as possible.

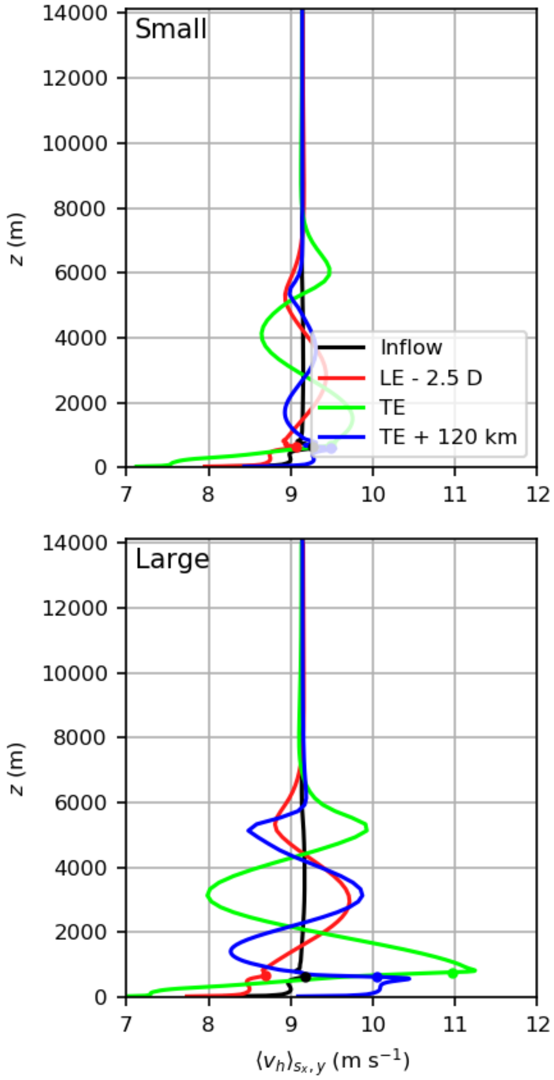
- Similarly, in Fig.8 the BL profile at TE+120km shows that the BL flow is energized significantly up to the BL height. Such an unexpected behavior is attributed to the drop in the perturbation pressure.

This is true for the flow at the BL top. Inside the BL, however, the increase in wind speed is related to the inertial oscillation. I have added this information in line 286:
" In the far wake, one quarter of the inertial wave length (λI /4 = 120 km) downstream of the wind farm TEs, the wind speed in the bulk of the BL is supergeostrophic. At 300 m height the wind speed has increased to 9.2 ms−1 and 10.1 ms−1 for the small and large wind farm, respectively. **This corresponds to a wind speed increase of 0.2 and 1.1 ms−1 relative to the inflow wind speed. These values approximately correspond the amplitude of the inertial wave (see Fig.~4).**"

The velocity profile should be extended further up to see any momentum deficiency and the discussion should be extended to include what causes such a the perturbation pressure drop.

The discussion in section 3.3 (Boundary layer modification) focuses on and is limited to modifications inside the BL. A vertical extension of the profiles is thus not appropriate. The velocity-, pressure and temperature fields in the free atmosphere are determined by gravity waves, which are discussed in detail in section 3.4 (Gravity waves).

Please find below the vertical profiles that extend up to the domain top. As can be seen, the profiles have a sinusoidal shape and are thus related to the gravity waves in the free atmosphere. The amplitudes decay above 5000 m due to the Rayleigh-damping layer. If you insist that these profiles shall be included in the article, then I would suggest to put them into section 3.4 (gravity waves).



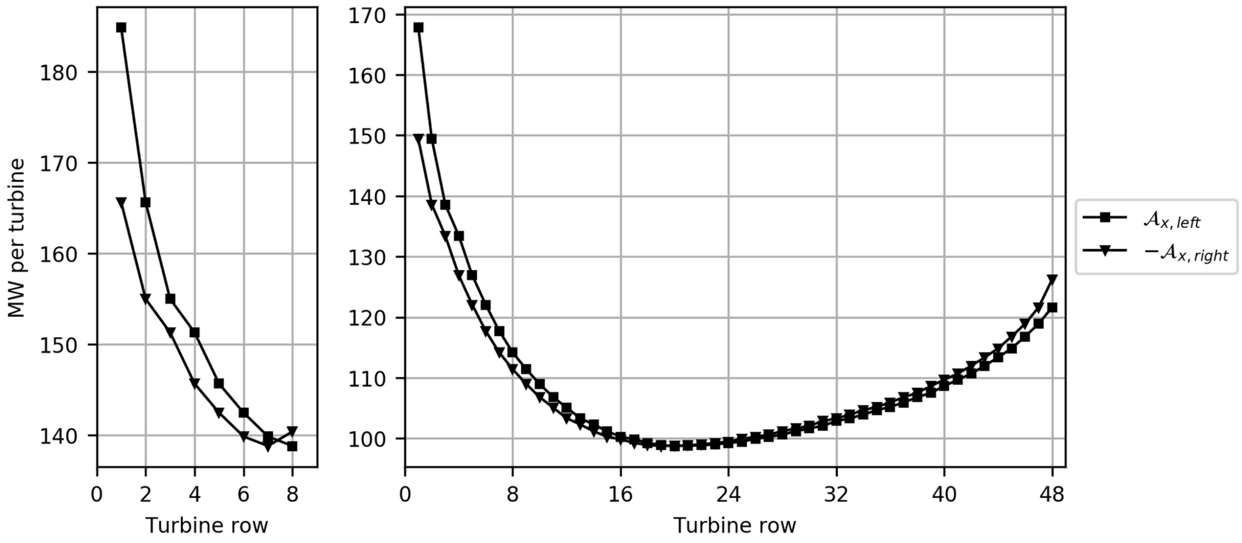
In addition, the momentum deficiency ocaused by WTs should be presented in a plot at the center plane or averaged only across the turbine (not averaged in the full y range)

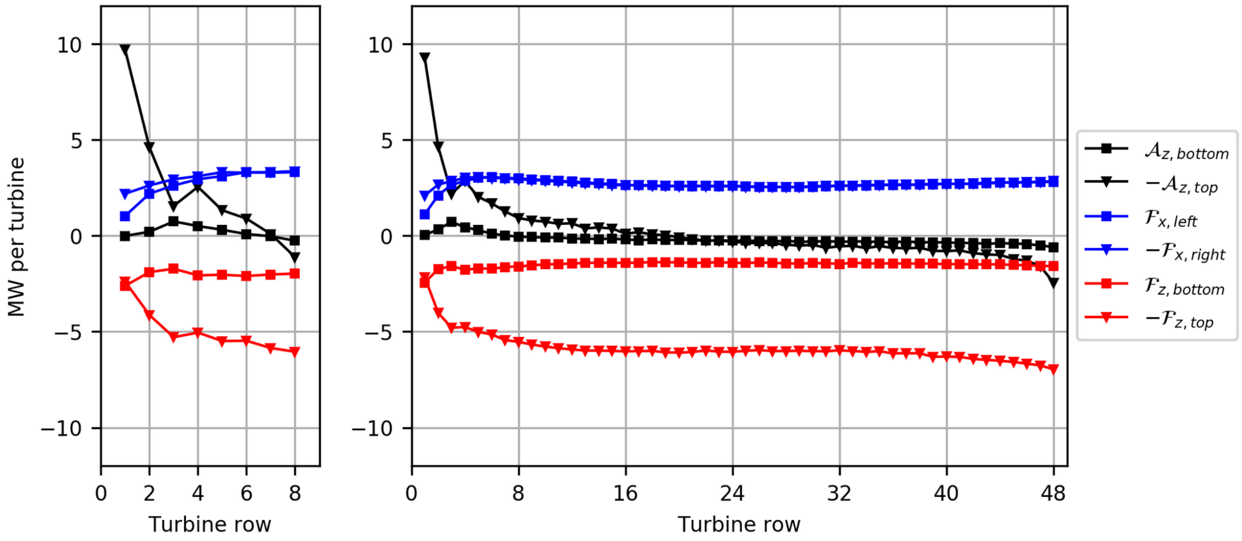
Including such a plot would certainly provide more details of the flow near the wind turbines. However, the aim of this study is to focus on effects that are on the wind farm scale or larger. Thus, adding such a figure is not relevant to the outcome and conclusion and I would like to avoid it.

- The energy budget analysis is performed by integrating the quantities over the control volume of a WT. It is also not clear if the presented values in Fig 11 are averaged over all the CVs. Such an analysis gives the distribution of energy components within the CV, but not the relations between them. For a better understanding, the integrations should be performed at the control surfaces (inflow, top, outflow) of individual CVs and they should be presented as a series for a turbine row. (inflow of a downstream WT would be the outflow of the upstream WT)

The values in Figure 11 are obtained by integrating over the entire wind farm, as it is stated in the caption:
"Figure 11. Energy budgets inside control volume Ωwf , that envelops the entire small/large wind farm. [...]"
The focus of section 3.5 (Energy budget analysis) is on the energy budgets in the control volumes, i.e. the net inflow/outflow or source/sink of energy in a control volume. In- and outflow values (e.g. of the advection term A) can be much larger than their sum (budget), so that presenting them in the same figure is not possible. Thus, such an analysis would require at least two more figures. Since it is only the net energy inflow (i.e. the budget) that can be extracted by the wind turbines, I think it is reasonable to focus only on the budgets. However, the data and the scripts for the requested calculations are freely available in the cited data repository and can be used for this purpose by any interested reader.

Please find below the requested plots of the input-/ output terms of each control volume.





The first figure shows the energy input by advection of kinetic energy through the left and right control volume boundaries. Note that $A_{x,right}$ has a negative sign (in the legend) and is thus a sink. As can be seen, the absolute values are much larger then the differences (the budget). This is mainly

caused by the choice of the control volumes: The turbine only occupies a relatively small portion of the crosswise extend of the control volume and thus most of the flow that leaves the control volume on the right side is not part of the decelerated wake flow.
The second figure shows the vertical advection as well as the horizontal and vertical fluxes. These terms are at least one magnitude smaller and thus require an extra figure. The advection and fluxes through the bottom boundary are much smaller then the respective top values, because they are limited due to the proximity to the surface (bottom boundary is at $z = 60$ m = 3rd grid point). The evolution of the horizontal turbulent fluxes $F_x$ is similar to the evolution of the TKE (see Figure 6 of the manuscript). Since the wind turbines generate additional TKE, more TKE leaves the CV thorugh the right boundary than it enters from the left boundary. After approx. 5 rows the TKE has reached a steady value and the net in/outflow is zero.

Please let me know if you think that these figures are worth to be included in the manuscript.

- It is quite counter intuitive that the pressure force contributes more to the energy production than the kinetic energy of the wind as suggested in Fig 12. It even acts as a sink for downstream turbines. It definitely needs further validation and explanation.

I agree that this is a counter intuitive result. But this behavior has already been observed by Allaerts and Meyers (2017), which validates the result. I added this citation and I have now also stated more clearly that this is caused by the negative perturbation pressure gradient near the end of the farm:

"The flow acceleration at the end of the wind farms is mainly caused by the negative perturbation pressure gradient that has the highest magnitude at the wind farm TE (see Fig. 3). The energy input by the pressure gradient P thus increases towards the TE of the large wind farm and reaches 5 MW per turbine at the TE. The pressure distribution inside the wind farm is determined by wave type two of the gravity waves (see Sec. 3.4 and Fig. 9). The flow acceleration near the TE of the wind farm and the related negative net advection of KE has also been reported by Allaerts and Meyers (2017) for a 15 km long wind farm in a conventionally neutral BL."