We would like to thank referee #3 for the valuable comments. We address the comments (in italics bold) on a point by point basis.

General Comments

The authors present a new parameterization of wind farms for mesoscale models, in particular for WRF. The explicit model is well described in the formulation as far as I understand, although I admit I havent reproduced the equations myself. There is however little justification as to how the freestream is selected or the individual source terms of the turbines aggregated to the grid-cell values. Adopting a common cell velocity U0 and adding individual thrust forces together to form the grid-cell overall thrust is an ad hoc solution that seems appropriate as first approximation.

As a first approximation, we use the available model information (spatial-averaged variables) to calculate the single turbine thrust forces (similar as in the WRF-WF scheme). As shown in Abkar and Porté-Agel (2015), this approximation holds for wind directions in which the turbines are aligned. However, for more complicated configurations, the local free-stream velocity is expected to deviate from the grid-cell velocity. In Badger et al. (2013), we introduce an offline coupling method, where the total thrust on the l.h.s. of Eq. (8) is determined as a function of wind speed and wind direction (and optionally stability) from high resolution models. This method has been implemented in the WRF model and in the future we would like to add this as an option. We discuss this issue on p. 3502 l. 15 to p. 3503 l. 4.

Then σ_0 is used to calibrate the wake model to the Horns Rev results. The doubt resides on whether this calibration holds for completely different layouts. For example, a layout occupying the same space that Horns Rev and with the same type and number of turbines but very different distribution would produce the same wind farm wake following this methodology. I agree with the authors that further research would try to account for the specific layout of the wind farm to generate grid-cell values without calibration.

The reviewer is right that current wind farm parametrisations do not account for the individual turbine positions within the grid-cell. The method described in Badger et al. (2013) accounts for the turbine positions, since it applies the drag from the micro-scale model. As soon as data from different wind farms (with wind farm wake measurements) becomes available, we will compare the presented approach with that of Badger et al. (2013).

There is very little research so far on mesoscale-simulated wakes so I think

that the paper is worth for publishing and I look forward to further efforts with this model in the future. The following remarks shall be considered to provide further justification to the evaluation methodology of the paper.

Thank you for this positive comment. The reviewer is right that little research has been done in the mesoscale modelling of wakes. In our opinion mesoscale models cannot only be a useful tool to study the impact of wind farms to the local climate, but it could also be used to study wind farm interaction, which is an important issue with the increasing wind farm density in the North-Sea.

Specific Comments

Page.3482 Line.23: I would remove "in Northern Europe and China" to make the statement general to coastal areas

On p. 3482 l.23 we would remove "in Northern Europe and China".

P3491, L.7: It is not so clear why the cell-velocity is representative of a common upstream velocity for the whole wind farm.

Actually, the grid-velocity is only used as a upstream wind speed for the local turbines within that cell.

We propose to rephrase the sentence on p. 3491 l. 7–8:

> We use the grid-cell averaged velocity as the upstream velocity U_0 to the wind turbine.

with:

> We use the grid-cell averaged velocity as the upstream velocity U_0 for all turbines within the same grid-cell.

P.3491, L.13: add Section number.

Thank you. We would replace p. 3491 l. 12–13:

> A practical description of how to use the EWP scheme in the WRF model is given in Sect. .

with:

> A practical description of how to use the EWP scheme in the WRF model is given in the section "Code availability" at the end of the paper.

P.3493, L.21: Can you provide a reference for the roughness length or justify why the value of 2e-4 m has been adopted?

We have used this value from the WMO standard roughness length over water.

We propose to change on p. 3493 l. 20:

> The surface roughness was constant in time and set to $z_0 = 2\times10^{-4}$ m for the entire domain.

to:

> The domain is fully contained over water and it had a constant roughness length of $z_0 = 2 \times 10^{-4}$ m in time, which follows the WMO standards.

P.3494, L.4: Why slightly-stable atmosphere when the validation data is considered neutral?

The slightly stable atmosphere initialised in WRF converged to a completely neutral PBL, since the surface heat flux was set to zero (see 7th specific comment of the 1st reviewer).

P.3494, L.7: What is the lapse rate of the inversion layer? Probably not a big influence on the results but should be documented since it is an input in the idealized simulations.

The lapse rate of the potential temperature is around 6.0 K/km within the inversion layer.

We propose to change on p. 3494 l. 5–7:

> After a four day integration period, the wind converged in the whole domain to a logarithmic neutral profile within a 650 m deep boundary layer and remained independent of height above the inversion layer.

to:

> After a four day integration period, the wind converged in the whole domain to a logarithmic neutral profile within a 650 m deep boundary layer capped by inversion layer with a potential temperature gradient of around 6 K/km. Above the inversion layer the velocity became independent of height.

P.3494, L.16: Was the averaging of the 9 wind direction runs done using a uniform directional distribution or considering the weighted-average following the actual distribution observed in the 30deg sector width? The result can be quite different depending on the averaging procedure

The averaging was done with the uniform direction distribution.

We propose to change on p. 3494 l. 16:

> For the validation against the mast measurements, we averaged the model wind speeds over the 9 wind directions.

to:

> For the validation against the mast measurements, we averaged the model wind speeds over the 9 wind directions with a uniform direction distribution.

P.3494, L.24: It is not clear how the length scale is determined based on the wind speed. Can you clarify this point? Maybe it is just enough to say that the initial length scale should be in first approximation equal to the rotor area. Then you introduce the idea of using the scaling factor to calibrate the model results to the observational data.

Thank you for this comment. The initial length scale is supposed to scale with the rotor area, but it includes the near wake expansion.

We propose to rephrase on p. 3494 l. 24–26:

> To a first approximation the initial length scale is defined to be independent of the upstream conditions and it is therefore the same for all turbines.

to:

> To a first approximation, the initial length scale is defined to be independent of the upstream conditions and it is therefore the same for all turbines. The initial length scale is assumed to scale with the rotor radius and accounts for the near wake expansion.

Figure 9: It would be useful to show the undisturbed upstream velocity profile from both the WRF-WF and EWP models to show that they are actually the same or very similar.

In the attached figure, we show the upstream velocity profiles from the EWP and WRF-WF scheme from the same grid-cell as has been used for Fig. 9. The two curves are indistinguishable from each other. In case the reviewer or editor would like to have this figure included in the manuscript, we would add it. Otherwise, we would mention in the text that the up-stream profiles very visually the same.

We propose to add the here underlined text on p. 3501 l. 18:

> ... farm simulation. <u>The free-stream velocities are visually indistinguishable between the EWP and WRF-WF simulation.</u> We choose ...

References

Abkar, M. and Porté-Agel, F.: A new wind-farm parameterization for large-scale atmospheric models, J. Renewable and Sustainable Energy, 7, 013121, doi:10.1063/1.4907600, 2015.

Badger, J., Volker, P. J. H., Prospathospoulos, J., Sieros, G., Ott, S., Rethore, P.-E., Hahmann, A. N., and Hasager, C. B.: Wake modelling combining mesoscale and microscale modelsm, in: Proceedings of ICOWES, Technical University of Denmark, 17–19 June 2013, Lyngby, p. 182–193, available at: `http://indico.conferences.dtu.dk/getFile.py/access?resId=0&materialId=paper&confId=126`, 2013.

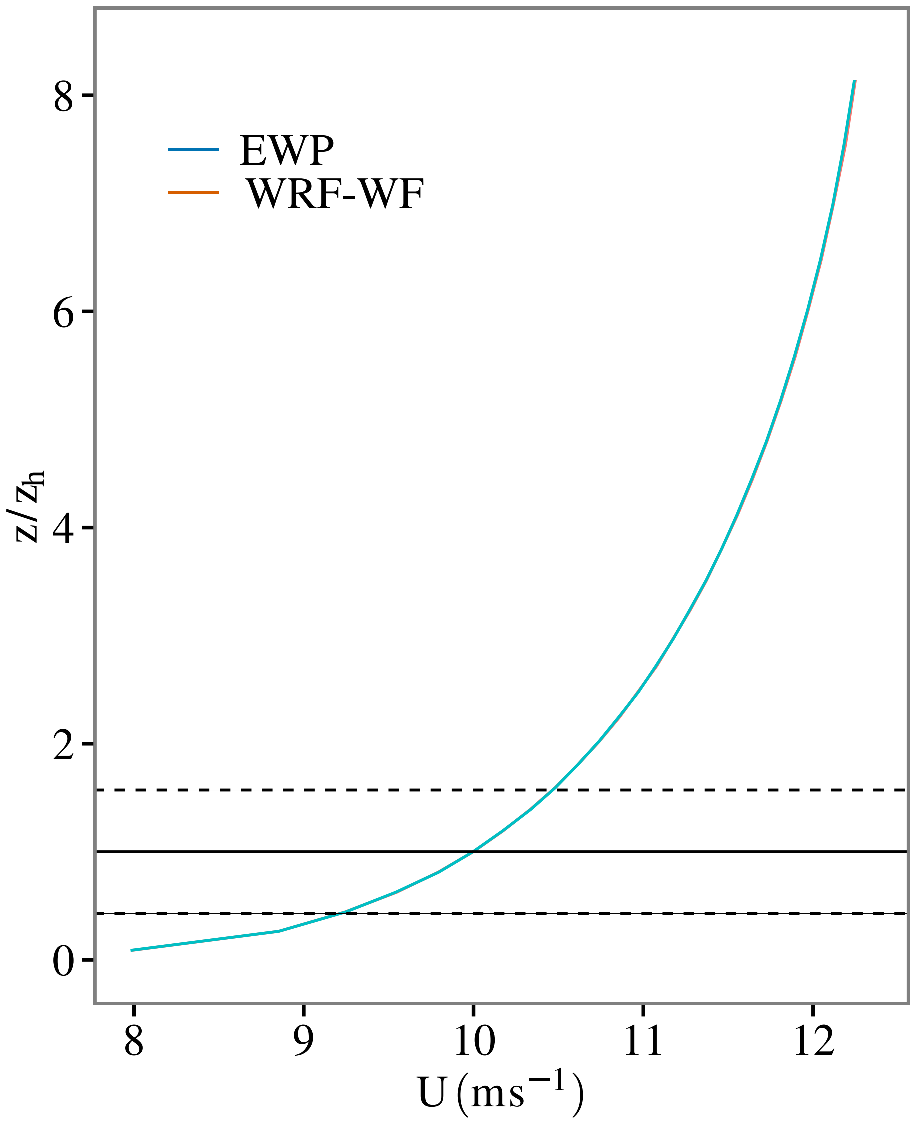

Figure 1: Upstream velocity profile from the EWP and WRF-WF.