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Comment

Interactive comment on “The Explicit Wake Parametrisation V1.0: a wind farm parametrisation in the mesoscale model WRF” by P. J. H. Volker et al.

Anonymous Referee #1

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The authors present a parameterization to take into account the effects of wind farms on the WRF mesoscale model. I am not aware of a previous implementation of the method described by the authors which confers originality to the work presented. In addition, the manuscript provides valuable information regarding the ability of the proposed parameterization and the one already existing in WRF to reproduce a wind farm wake. The contents of the manuscript should therefore be of interest for GMD readers.

My major concern is that the parameterization has been tuned and validated for the same wind farm (Section 4.1.1). Results herein presented are therefore a maximum limit on the performance of the scheme. Ideally you should calibrate the parameterization on an independent wind farm. If this is not possible you should show results of the

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sensitivity simulations you performed to select the parameter of the parameterization, σ_0 .

Having the scheme tuned for this particular wind farm somewhat hampers the comparison with the WRF-WF parameterization that has not been tuned for this particular wind farm. This should be clarified.

Finally, it seems the authors have available a large observational dataset but have restricted the comparison for westerly winds with different atmospheric conditions. If possible, the authors should extend the validation showing more specific results of the parameterizations performance (e.g. wind farm wakes as a function of the hub height wind speed). I do not see a major reason for not including a more extended comparison having the data and the simulations available.

In summary, I recommend acceptance of the manuscript after addressing these comments and the more specific comments provided below. I think the authors should be able to address the comments in the context of major revisions.

SPECIFIC COMMENTS

1. Page 3485, Line 12: it will be good if you can describe the ensemble-average methodology, its differences with the more standard approach of volume averaged shown in Fig. 1, and why is relevant for the EWP wind farm parameterization.
2. Page 3487, lines 5-10: If I understood correctly, the turbulence induced by the rotor is dissipated within the grid cell and P_t is neglected on Eq. 3. This probably has implications for the maximum horizontal resolution that can be achieved by the parameterization. Is there any theoretical/empirical limit?
3. It is not clear to me what are the final equations that are implemented on the WRF model. I think the parameterization only needs to incorporate equations 14 and 15 in the model. Is this correct?
4. page 3491, line 11: WRF does not have 2nd order PBL closures.

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5. Page 3493, line 14, is there any specific reason to select the horizontal resolution of 1120m? Why not just 1 km?
6. Page 3494, line 11, It is not clear how do you impose the wind speed at the hub height. You mention that you integrate the model for four days and use the resulting wind profile to initialize the wind farm simulations. How do you get the hub height winds that you want to impose?
7. How do you represent the turbulent fluxes at the surface (i.e. sensible heat, latent heat and momentum?) in the idealized experiments?
8. Page 3494, line 17. Are the simulated wind speeds stable enough to use only the instantaneous wind speed in the validation? Usually you average results over a certain temporal period.
9. Page 3495, lines 23-24: Does σ_0 depend on the horizontal resolution?
10. Page 3495, lines 24-36: I do not understand how you reached this conclusion “Therefore, we conclude that for neutral conditions the initial length scale can assumed to be independent of the upstream conditions.” Please, clarify.
11. Page 3497, lines 4-6: You mentioned before that you used different wind speeds at the hub height for validation but here you say that you use 10 m/s. This deserves clarification.
12. Same lines as before. Although you use different wind speeds to select σ_0 and validate the parameterization, you focus on the same wind farm and therefore the parameterization is somewhat tuned for this particular site. This hampers the comparison with the WRF-WF parameterization. If you show sensitivities to the values of σ_0 one can have an idea of how important is the specification of this parameter for the results. See also general comment.
13. Would you recommend using the σ_0 herein presented for other wind farms? Or should σ_0 be adjusted for each wind farm?

14. Page 3499, lines 6-7: The bias in the WRF-WF is not statistically significant. Both the WRF-WF and EWP reproduce the observations within the observational uncertainty.

15. Pages 3500 and 3502, Section 5.2. I like this section. It shows large differences between the TKE from the two schemes. This is probably the largest difference between the 2 schemes. If possible, the authors should show figures from the other works mentioned during the discussion to facilitate the comparison of results from both TKE fields.

16. Section 5.2: Are you advecting the TKE in the WRF-WF runs? I think by default is turned off in WRF but is better to activate it. Activating the TKE advection may change the shape of the TKE field shown on Fig. 8b.

17. Conclusions, page 3501, lines 25-26. The bias is not statistically significant. Maybe is better to say that EWP reproduces the wind farm wake within the observational uncertainty.

18. Page 3506, line 13: Why do you need the power coefficient? It is not mentioned in the description of the EWP parameterization.

Interactive comment on Geosci. Model Dev. Discuss., 8, 3481, 2015.

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