Review of "Inclusion of the subgrid wake effect between turbines in the wind farm parameterization of WRF" by Liu, Yang, Chen, Deng, Yu, and Xing, submitted to Geoscientific Model Development

The authors describe a modification to the default Fitch wind farm parameterization (WFP hereafter) in the WRF model to incorporate the sub-grid wakes between turbines that are located in the same grid cell. The WFP is based on the Jensen linear expansion wake model and accounts for (or claims to account for) the wake overlapping and for the angle between the wind direction and the rows and columns of turbines in a wind farm with a regular layout. The WRF model is then run for 3 days with an incredibly huge wind farm consisting of 25600 turbines of 3 MW each (corresponding to an astronomical installed capacity of 76 GW), covering over 100 x 100 km^2 offshore of Hong Kong. Sensitivity to grid spacing and turbine spacing is assessed. Results indicate that the proposed modification to the Fitch scheme produces more wind power and more turbulence than the original Fitch scheme, and therefore, somehow, this means that the proposed modification is desirable. Since there is no comparison of the two against any real data, one cannot conclude that the proposed changes are beneficial in any way. This is a fundamental flaw of the paper. In addition, there are several major issues in the way the parameterization was designed, as described below, and the notation is confusing or possibly wrong. The issues are so severe that I cannot recommend publication of the paper at this point.

Major Remarks

1. If a team wants to demonstrate that their method is better than the default method, then they need to have an observable dataset to compare the results obtained with the old and the new methods. Only by comparison with observations one can possibly conclude that one is better than the other. It is not sufficient to simulate a case with both, as the authors did in this study, because we do not know what the truth is. To make the matter worse, why simulate an impossible farm, one that has more turbines than any built before (25,600) and that has a capacity at least 30 times larger than the largest wind farm existing today and covering an area of approximately 100 km $\times$ 100 km? There is no way to verify any of your findings or confirm that your modification works properly (and I think it does not, see comment 13);

2. The literature review in the Introduction is limited and old, focusing mostly on studies pre-2014. Many more papers have been published since then, including several with the Fitch parameterization. Two relevant studies in particular have been missed and need to be described here because they introduced analytical wake models into WRF to account for the sub-grid wake effects, similar to what this paper is trying to do: Ma et al. (Wind Energy Science, 2022) about the Jensen wake model and Ma et al. (Wind Energy, 2022) about the Gaussian model, Geometric model, among others, and their ensemble.

3. The discussion of Pan and Archer (2018) is incorrect. They used LES results to calibrate the geometric model, which is an analytical model for the wind turbine wakes, and then they inserted it in the WRF model. There is no need to run LES to

use it. As far as I know, it was the first published paper that treated sub-grid wakes in a mesoscale model, WRF in fact. Then Ma et al. (2022a,b) also incorporated sub-grid models in WRF. As such, this paper is not really introducing "a new way, namely, through a simple engineering wake model" because this has actually already been done and documented. The literature review therefore needs to be slightly rewritten to give proper recognition to the three past studies mentioned above: Pan and Archer (2018) and Ma et al. (2022a,b).

4. Also, there was a code bug in WRF that affected the results of the Fitch parameterization, documented by Archer et al. (MWR, 2020). Which version of the WRF was used here? If between 3.6 and 4.2, then the simulations need to be redone to fix the bug.

5. Eqs. 1-3 are incorrect. They are the same as in the original paper by Fitch et al. (2012), but this is not how the scheme is actually implemented in the WRF model. Archer et al. (2020) mentioned above shows the correct version of the equations as they are implemented in WRF since v3.6. Noticeably, the thrust and power coefficients are a function of hub-height wind speed only, not of V_{ijk} (there is only one C_p and one C_t for the turbine, not one for each vertical level k). Also, it is not the wind vector squared that is used for the wind speed tendency. Lastly, A_{ijk} is not the swept area, it is the portion of the swept area that is in vertical level k.

6. I am not 100% sure, but the correction coefficients seem to be poorly designed, because they rely on the assumption that the layout of the wind farm is a regular grid with rows and columns not only perpendicular to each other, but also oriented north-south and west-east, respectively. I reached this conclusion because the wind direction of 0^o is from the North and θ is clockwise from the North, following the meteorological convention. Thus Eq. 8 only works if the wind rows are oriented from north to south -and- if the wind is from the North, thus all turbines are fully waked (except the front row of course). In all other cases, there is a partial superposition. The angle correction and the γ terms take into accounts cases when the wind direction is not from the north and therefore only a fraction of the area overlaps. However, it seems to me that the layout of the wind farm is still assumed to be regular, with rows and columns at 90^o from each other. This is not a reasonable assumption. Modern farm may have variable spacing and non-symmetric and non-regular layouts (e.g., Anholt). If true, this a major flaw of the study such that it should not be published because it uses an underlying assumption that is unrealistic and too restrictive.

7. Eq. 10 does not make sense. Previously, n was the total number of upstream turbines, now, I guess, it is the index of the upstream turbines, previously j? Or perhaps n is the index of the last upstream turbine? Also, the thrust coefficient is just a function of wind speed, thus it should not depend on the angle θ or on the correction coefficient. I do not understand how v_{n0} and $v_{n\theta}$ are calculated. Perhaps with Eq. 14? See below.

8. Eq. 14 does not make sense. How can the velocity vector be a tensor?

9. Eq. 15 does not make sense. What is $v(i, i)$? It might help if you could draw some of the $l(i, j)$ in Figure 3. Which cell is used to determine the upstream flow, basically u_0 from Eq. 8? I believe that this is key. There is not only one value of u_0 in large farms because many grid cells are upstream. Which value(s) is (are) picked here?

10. Eqs. 17, 18, 19, and 20 not explained or derived. The partial superposition with the Jensen model has been solved before, see for example the review paper by Archer et al. (Applied Energy, 2018), Eq. 1-4. Is there a difference between R and $R(i,j)$? Why now are the indices in parenthesis, as opposed to subscripts like earlier in the paper?

11. Not enough details are provided about the CFD simulations. What CFD simulator was used? How were the turbines arranged? I think north-south and east-west perpendicular, but I am not 100% sure, see comment 6 above. What is the diameter D? What is the turbine model, was an actuator disk or line used? How many time steps? What was the grid like? Resolution? Run duration? The one figure shown (Fig. 6) appears to be an instantaneous snapshot, not a time-average as we would need in order to tune the parameterization.

12. The discussion at L. 305-309 seems to be related to the incorrect dependency of C_t on the wind speed at each vertical level k mentioned at 5, which is NOT how Fitch works in the recent versions of WRF. This confirms to me that either the wrong equations have been written here or a very old version of WRF has been used, perhaps one with the code bug.

13. The results do not make sense and suggest that something is wrong in the parameterization.

    - Fig. 12 shows the average wind power generated by the farm after 3 days. In the presence northeasterly flow, as the case here (Figure 10), the boundary grid cells along the northern (top) and the eastern (right) sides experience the strongest wind, especially the ones along the right boundary. The turbines in the grid cells along the northeastern boundary all produce maximum power with the default Fitch scheme because there is no sub-grid wake effect in Fitch. By contrast, with the proposed modification, some of the turbines in this "ribbon" experience losses. Therefore I expect the Fitch run to produce more power along the northeastern edge. Instead, the exact opposite happens! Also, I would expect to see large differences in the middle and towards the outer edge of the farm (southwestern end), because the inclusion of wake effects should create more differences further into the farm. Instead, the two simulations are basically identical except for the northeastern band, where the modified Fitch scheme somehow produces more than the original for the most upstream rows! In addition, the new scheme generates an astonishing double power than the original scheme. This is highly suspicious.
    - The total power generated is very small in both runs. With an installed capacity of 3 MW $\times$ 25,600 = 76,800 MW, a production of 11,639 MW and 5703 MW for the modified and original scheme, respectively, correspond to a capacity factor of only 15% and 7%. Why is it so low?
    - To make things worse, the energy left in the farm (Figure 11), a weird concept per se because we would need to know over how many levels and how many hours it was calculated, is larger for the simulation that extracted more power, i.e., the one with the new scheme. I find it hard to conceptualize how higher power extraction also leaves more power behind, everything else being equal. Perhaps the authors need to look over a larger domain than just the wind farm area?

I stopped reading at the Sensitivity experiments due to excessive prior issues.

Minor Remarks

14. L. 12: Please replace "explosive" with a more appropriate adjective.

15. L. 14: There should be no citations in the abstract, remove the citation Fitch et al. (2012).

16. L. 15: The sentence does not read well, rephrase to something like: "shortfalls, e.g., it does not consider the wakes behind wind turbines inside the same grid cell."

17. L. 22: You need to spell out "CFD".

18. L. 31-34: I know this is probably a translation issue, but the first three sentences have a lot of words but really do not say much ... consider removing them or combining them into one, concise, and clear sentence that goes straight to the point.

19. L. 41 (and other parts): Replace "decay" with "deficit" throughout the article, which is the term I have seen used in the literature.

20. L. 41: The sentence does not read well, rephrase to something like: "wind speed deficit in offshore farms could reach 16% and their wakes could extend downstream as far as 60 km."

21. L. 44-47: The findings by Baydia Roy and Traiteur (2010) have been shown to be unphysical and not due to the wind farm in the study by Archer et al. (2019, Journal of Turbulence, `https://www.tandfonline.com/doi/full/10.1080/14685248.2019.1572161`), see their Figure 1. Thus, Baydia Roy and Traiteur (2010) should not be cited as a reference for the impacts of wind farms.

22. L. 50-54: Similarly to Baydia Roy and Traiteur (2010), parameterizing a wind farm as a surface roughness element, which is what Barrie and Kirk-Davidoff (2010) did, has been shown to be unphysical by several studies, including the study by Fitch et al. (JClimate, 2013), and Jacobson and Archer (PNAS, 2012). Thus, Barrie and Kirk-Davidoff (2010) should not be cited as a reference for the impacts of wind farms.

23. L. 49: Another study that found modest to negligible impacts of offshore wind farms on precipitation is that by Al Fahel and Archer (BAST, 2020).

24. L. 62: The correct reference for the Fitch scheme is Fitch et al. (2012), published in Monthly Weather Review, but the list of references does not include it. Fitch et al. (2013) is used here, but Fitch et al. (2013) is an application of the Fitch scheme, was published in Journal of Climate, and is not the correct reference here. You need to add Fitch et al. (2012) to the References and cite it here.

25. L. 102-104: the sentence needs to be rewritten.

26. Eq. 1-3: the notation needs to be consistent here and in the rest of the paper. First, i, j, k is a subscript for all terms, except for N_T^{ijk}. For consistency, call it N_{ijk}^T or just N_{ijk}. Second, later you use i, j as the indices for the turbines in a grid cell (confusingly, by the way, as sometimes i is the upstream turbine, sometimes j is). To address this, I recommend removing i, j from these terms here, you can just state in the text that the equations are valid at each grid cell i, j.

27. Eq. 7: comparing Eq. 5 and Eq. 7, it appears that i is the turbine of interest and j is one of the n turbines upstream (also from the summation index in Eq. 9). Thus I would call this u_{ij} (not u_{ji}), the wind speed at i caused by turbine j, and I would replace x_j with x_{ij}, the along-wind distance between i and j. Later, L. 218, you define γ_{ij} as the shielding factor from turbine i to j, the exact opposite convention.

28. Figure 9: What value is used to normalize? u_0? Or inflow u_i?

29. For Figs. 11-13, I would not use a smooth shaded contour plot, but rather a gridded plot, to see better the individual grid cells. Also, using a color like a greenish blue for the near-zero values makes it hard to tell where the difference is positive and where negative. Use white for near-zero instead. Last, use a finite number of color levels, 8-9, and choose the levels in a smart way (e.g., we do not want a plot to be all blue except for with a few grid cells that are other colors, we want to see a balance of cells with the various colors).