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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Do large-scale wind farms affect air quality forecast? Modeling evidence in Northern China" *by* Si Li et al.

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Responses to Referee #2

This study quantifies the impact of wind farms in northern China on PM2.5 concentrations in the North China Plain using the WRF-Chem model, which could potentially advance our knowledge of the air quality impact of wind power generation. This version of the manuscript has been improved to some extent compared with the last version I reviewed. The authors have addressed some of my major comments. However, I think more evidence is still needed to support their main findings.

Response: We thank the Referee #2 for his/her comments and suggestions to this manuscript which help us to considerably improve the revised paper. We have made

major revisions to our manuscript following the Referee # 2's comments and address all comments from the Referee #2. The presented below are our point-by-point responses to the Referee # 2' comments.

1. I appreciate that the author added an evaluation against meteorological observations. However, the present evaluation results are obviously not enough for the purpose of this study. First, the author only evaluated the BASE scenario without wind farms. In fact, the scenario with wind farms included is supposed to be more close to real-world situations. The author needs to compare simulations in all model scenarios with observations to examine whether the simulated impact of wind farms is in line with observations. Second, the authors just used five selected met observational stations, while NCDC has hundreds of (or at least tens of) stations in the modeling domain which should be included in comparison. Also, the stations the author selected are far away from the wind farms, I suggest that the author specially look into some stations close to the wind farms to see if the simulated perturbation of meteorology by the wind farms is consistent with observed patterns.

Response: Following the Referee #2's suggestions, we collected measured PM2.5 concentrations, and the winds and air temperature data from additional 3 monitoring sites to evaluate modeling results. Among the 8 sites, four sites are proximate to the WFC, these are Chengde, Siping, Ligong, and Baotou (Table S3). New figures and Tables are provided further evaluations of modeled PM2.5 concentrations and the winds and air temperatures. Now the revised Supplementary includes 8 figures and 14 tables showing the model evaluation results. We also validated modeled meteorology and PM2.5 concentrations from the two wind farm parametrization schemes against measurements at the 5 sampling sites. Results are presented in Tables S12 to S17. As expected, the meteorology and PM2.5 derived from the two schemes do not differ significantly with the BASE simulation. Overall, our results show that the DFP scheme yield slightly better prediction for meteorology and PM2.5 at the sampling sites proximate to the wind farms but slightly worse at the observational stations in megacities

which are far away from the WFC, such as Beijing, as compared with the measured data. These statements have been added to the revised section 2.3.

2. The authors have also added a comparison of their simulated meteorology perturbation by the wind farms with a couple of previous studies. However, the current comparisons are all qualitative (either increase or decrease). Can the authors do some more quantitative comparisons to examine if the magnitude of meteorological perturbation in their simulations are roughly consistent with previous work? Although different studies are looking at different locations and time periods, I think the perturbation should at least be within the same order of magnitude. If a larger difference is found, I would expect a reasonable explanation why this happens.

Response: Following the Referee #2's suggestions, in the revised manuscript we have extended significantly Discussion section by inputting detailed comparisons of meteorological conditions (winds, TKE, and temperatures) between the results from our studies and previous results (2nd paragraph of Discussion section, line 444-492). We added two new figures (Fig. S17 and S19) in the revised Supplementary. Although the magnitude of windfarm induced changes in these met variables differ somewhat from previous results due to different scale and installation of wind farms, and meteorological conditions (spatial scales), we show that these modeled meteorological variables and their downwind distribution agree reasonably well with the previous results. We have made major revisions to this section.

3. The authors only conducted simulations in two months, which weakens the robustness of the conclusions given the large variability of the simulation results. The author at least needs to highlight that the magnitude of the wind farms' impact might be quite different for other years or time periods.

Response: Following the Referee #2's comment, we have added statements "Given large seasonal, inter-annual, and intra-annual variabilities of meteorology and climate, the results and conclusions from the present study might not be applicable in other

years. Further extensive investigations of the influences of wind farms with different scales and installations on air quality are needed." (section 2, line 164-167)

4. Why does the model show a large decrease in PM2.5 concentrations at the locations of wind farms in winter but nearly no change in summer? Is it attributed to the local atmospheric circulations you mentioned in Section 3.2? I am a bit surprised if the local circulation fully counteracts the wind farm's influence.

Response: Observational data show that the mean wind speed in north China in winter under the East Asian winter monsoon are stronger than summer months. Compared with large-scale winter circulations, small-scale summer atmospheric circulations are forced, to a large extent, by local surface heating and cooling. In the revised manuscript, we have added a new reference (Feng et al., 2020) to support this argument (line 346).

4. In the abstract, the author only described the results in winter, which bias readers' understanding because the roles of wind farms are so different in winter and summer. I suggest that the author include both seasons to give a complete and unbiased picture of the wind farms' impacts.

Response: We have added new statements in the revised Abstract.

6. The authors mentioned in Line 72 that "The total number of wind turbines in the outer domain (northern China, Figure 1) was approximately 81,000". Is this calculated from the wind farm area and average wind turbine spacing set in your model? Line 77: Which year is the number 72% for?

Response: The number of wind turbines at 81,000 is calculated by total power capacity 121,500 MW divided by nominal power 1.5 MW of each turbine. 72% is for 2015. This has been specified in the revised paper (line 76-80).

Technical corrections: 1. Figure 3a, Figure 6a, Figure 8: Please use date instead of hours for the X-axis. 2. Line 65: WFC has already been defined before.

Response: Done!

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