

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

Anonymous Referee #2

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This study quantifies the impact of wind farms in northern China on PM_{2.5} 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.

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

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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.

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.

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.

4. Why does the model show a large decrease in PM_{2.5} 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.

5. 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.

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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?

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.

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