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

Anonymous Referee #3

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General Comments

This study presents a modeling work investigating the influence of large-scale wind farms on surface PM2.5 concentrations. Two wind farm parameterization schemes were adopted in the simulations to simulate the influence of the intensive wind farms over northern China. The changes in surface PM2.5 concentrations induced by the wind farms were then estimated by comparison between simulations with and without wind farm schemes. Theoretically, the wind farms will affect the local and regional meteorological condition, particularly the boundary layer turbulence. However, the analysis and presentation showed in this study did not convince that the wind farms would have significant impacts on regional PM2.5. Further analysis and clarifications are needed before publication.

Major Comments

1. Line 143 of page 7, it is not appropriate to conduct t-test based on hourly data because there is evident diurnal variation of surface PM2.5 concentration. It makes more sense to conduct t-test based on daily mean values. This may be the reason that figures (e.g., Fig. 2) show that the areas are with significance at 95% confidence level but in fact with very small changes. This also raises another concern about the ensemble runs, which is discussed in the comment below.

2. In the Model evaluation section (Text 1 in Supporting Information), only the simulation without wind farm parameterization was compared with measurements. However, in the real world, the intensive large-scale wind farms have been on operation and might have influenced the atmospheres and measurements. Therefore, the simulations with wind farm parameterization schemes should also be validated against measurements in order to demonstrate (1) whether the two schemes could reproduce the impacts of intensive large scale wind farms on the atmospheres and the pollution distribution; (2) whether the simulations with wind farm schemes perform better for reproducing the meteorological fields (e.g., near surface wind, temperature, and PM2.5 concentration) than the simulation without wind farm scheme; (3) the sensitivities and uncertainties of the two wind farm schemes in simulation of the wind field and TKE variation in the domain studied.

3. In terms of evaluation, Fig. S4 seems indicating the model performance is so poor over these 5 stations. Any specific reason? Many studies investigated WRF-Chem simulations of surface PM2.5 concentration over China. Their results are much better than this. Can we really trust the sensitivity analysis based on such model configuration?

4. Many figures used fraction to indicate the impacts. It may be good for some cases, but for the results in this study it may not be appropriate. Fraction sometimes is misleading. For example, in Fig. 2, the area with larger fraction is simply due to its small

concentration instead of due to its large change. The absolute values of concentrations and changes are preferred. The same issue for Fig. 3. These figures show the fraction seems large, however, it is just due to small concentration. Do we really care about the increase from 1 ug/m3 to 2 ug/m3 even though the fraction change is 100% increase? Many places in the text show high fraction value such as 400% in the main text or -40% to 250% in the abstract, which is just misleading. If we look at the absolute values of changes of PM2.5 concentration over the polluted region, mostly less than 10%. This must be revised and clarified.

5. The authors attribute the increase of surface PM2.5 in the downstream of wind farms (Figure 2 and Figure S6) to the decreasing wind speeds (Figure S8) and decreasing turbulent mixing that weaken the vertical mixing and transport of the air pollutants (LINE 277-285). The analysis is not convincing. First, according to Figure S8, the wind speeds reduce in the downstream (south) of the wind farms, and the reduction is about $0.5 \sim 0$ m/s in regions of Beijing-Tianjin and negligible in the central Hebei province. However, the positive fraction of PM2.5 is much higher and more evident in the central Heibei province than that in Beijing-Tianjin, as shown in Figure 2 and Figure S6. Why? Second, wind turbines reduce the hub-height wind speed in downwind areas, at the meantime, generate intensive turbulence in turbine wakes, as the authors cited in LINE 73-74 (Porté-Agel et al., 2014; Li et al., 2018; Fitch et al., 2013; Baidya & Traiteur, 2010; Frandsen et al., 2006). The intensified turbulent strength could enhance the vertical mixing of momentum, heat, moisture and air pollutants. In this sense, the PM2.5 concentrations near the surface in downstream should decrease instead of increasing as shown in this study. Could the authors provide more information and discussion about how meteorological fields in downstream respond to the large-scale wind farms?

6. According to Figure 3a, the monthly average hourly concentration fraction between SLP simulation and BASE simulation is 3% for Zhangjiakou (blue dash line in Figure 3a and LINE 304-308). Figure 8 shows the same information but for the hourly fraction between DFP simulation and BASE simulation. The monthly mean hourly fraction in

Figure 8 is -12.0% for Zhangjiakou (black solid line in Figure 8 and LINE 486). It seems that there may be large difference between the simulations with the two wind farm schemes (3% in SLP vs -12.0% in DFP). Could the authors evaluate the possible difference between the two wind farm schemes?

7. Another major concern is about the case analysis without ensemble runs. For example, Fig. 4, Fig. 6, and Fig. 8. Without ensemble runs, i.e., comparison with one single continuous run with another, it may be fine to compare their monthly mean fields because we can take each day as one ensemble member. However, it doesn't make sense to compare the results at a particular day, because the difference between the two simulations can be simply due to the chaotic signals. If the case analysis is needed, the ensemble runs are necessary.

8. Fig. 5d shows the double of wind farms reduce the overall impacts, why?

9. Figure S10 demonstrates the correlations between ΔPM2.5 and ΔTKE, and between ΔPM2.5 and ΔV in January 2016 within and downstream of the WFC. Could the authors provide more analysis of the correlation between ΔTKE and ΔV within and downstream of the WFC? This may be helpful in depicting the changes induced by the intensive large-scale wind farms on atmospheric dynamics and thermodynamics.

Minor Comments

1. Many figures (e.g., Table S2, Fig. S6, Fig. S8) are discussed extensively but are put in the supporting material, which is not convenient for readers.

2. Fig. 9, SRL is top panel, DOU is bottom panel. Need correction.

3. Line 645-650, these statements are not convincing based on the analysis in this study.

4. In the summer case, "Positive ΔPM2.5 can be discerned in the south (downstream) of the WFC, . . ." (LINE 396). The prevailing wind in northern China in summer consists of mainly southerly and southeasterly. So "downstream" should be "upstream"?

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