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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Weather response to management of a large wind turbine array" *by* D. B. Barrie and D. B. Kirk-Davidoff

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We would like to thank the referees for their thoughtful reviews. We take the opportunity here to address the comments, suggestions, and issues raised by the reviewers.

In general, our response to the comments posted by Anonymous Referee #1 is that this research project is ongoing. We agree with the reviewer that any dependence upon the scale, location, and shape of the wind farm as well as the influence of the atmospheric initial conditions are worth characterizing in future work, and we are actively planning these studies. To address the issue raised by the reviewer, we will enhance our description of future work in the conclusion section of the paper. We believe that the findings described in the already submitted paper are worth publication based on their own merits, and also because the findings are timely. However, we will continue to investigate this topic with attention paid to the reviewer's suggestions. In the meantime,

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we direct the reviewer to the small discussion of threshold and scale dependence in Kirk-Davidoff and Keith (2008), which described an investigation of those topics in a shallow water model.

With regard to the third point in the review posted by Anonymous Referee #1, we intend, as part of our research plan, to study the weather impacts in a higher resolution weather forecasting model. Baidya Roy et al. (2004) carried out a study with the Weather Research and Forecasting (WRF) model of a much smaller 10,000-turbine array, which would produce on the order of 4 GW of electricity. We will expand our discussion of Baidya Roy's work to summarize for readers the impacts of smaller wind farms on the atmosphere.

Anonymous Referee #2 raises an issue that we have received many questions on, and that deserves enhanced discussion in the paper. Baidya Roy et al. (2004) modeled the wind farm as an elevated momentum sink in the WRF model. Quite simply, we did not use this approach because of the comparatively coarse vertical resolution of the Community Atmosphere Model (CAM). Adding a momentum sink to the first atmospheric layer wouldn't be significantly different form the surface roughness approach we used. The second layer midpoint in CAM is at 970 millibars, which is approximately 300m in elevation. This is much higher than a typical hub height elevation of 100m, so adding a momentum sink to the second layer in CAM would be physically unrealistic. Because of the dearth of field observations, and the ease of deriving a relevant roughness length using the Lettau method, we chose to represent the wind farm as an increase in surface roughness. We do hope to implement the momentum sink approach in future work with higher resolution models, and will discuss this in our revised manuscript.

Referee #2 asks whether CAM treats the wind farm, as represented by a value of surface roughness, as a porous obstacle or something more akin to a topographic feature. The model wind farm is treated in a similar fashion to vegetation, in that its friction impact is given by a surface roughness length and displacement height. However, unlike the model vegetation, the wind farm does not respirate. We will clarify these points in

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the paper.

With regard to the title, the term "management"; refers to the turning of the wind turbines in such a way that surface roughness is altered. We will clarify this term in the introduction to avoid confusion. Referee #2 raises a larger issue, by suggesting, "they should present their work as a theoretical problem with real-world applications instead of a purely applied problem."; While we are not in a position to assert that wind farms can be used to perturb weather, we do think it is important to characterize the extent to which perturbations might be minimized or otherwise, optimized. Because there aren't yet observations of the impacts we describe in the paper, and we haven't yet performed experiments with a high-resolution weather forecast model, we do in fact view this work as a theoretical problem with the potential for real world applications, and will clarify this in the manuscript.

Lastly, we will include a reference to Keith et al. (2004). This was neglected in the manuscript because the model runs described therein are also discussed in Kirk-Davidoff and Keith (2008), but we agree that we should have included a direct reference.

Interactive comment on Atmos. Chem. Phys. Discuss., 9, 2917, 2009.

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