

Interactive comment on “Potential climatic impacts and reliability of very large-scale wind farms” by C. Wang and R. G. Prinn

C. Wang and R. G. Prinn

wangc@mit.edu

Received and published: 17 December 2009

Responses to the Comments of Reviewers

We appreciate the constructive comments and suggestions of both reviewers. We have revised the manuscript based on these comments. The following are our point-by-point responses to the reviewers' comments (listed using bold and *italic* font).

Reviewer 1:

In order to aid in the interpretation of the results, the authors should present some of the wind fields corresponding to the presented temperature fields. A figure showing the change of wind speed with height, analogous to figure 3, would be helpful, and figures showing the change in wind speed in color, with vector

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direction changes shown with arrow superimposed with be a very helpful complement to figures 2 and 7. Could the change in the profile of temperature change with height from the H to the VH runs have to do with an advective temperature change aloft? Are the temperature change profiles similar over all continents or do they have a different character depending, say, on latitude? It would also be helpful to have more discussion of why the temperature change results for L and H in figure 3 are so similar- the relationship between temperature perturbation and wind energy generated seems otherwise reasonably linear.

We have added a new figure (Figure 4) in the revised manuscript showing both the geographical distribution and vertical profile of wind magnitude changes (we use actual kinetic energy change or wind magnitude change – this avoids the need to show wind direction). Related discussion is also added: “Our results suggest that the latter effect prevails over the majority of the installation regions (Figure 4). When averaged over the land, the reductions of wind magnitude in the various land-installation cases display a similar vertical profile, with a peak in the second model layer above the surface (Figure 4c). Note that the difference between Run L and Run H is defined more clearly in the wind reduction than in the temperature change (Figure 3), implying that the temperature change is more closely related to the vertical turbulent heat exchange. The effect of wind reduction over the installed land regions appears to exceed the effect of the surface roughness increase. The wind reduction specifically weakens the near-surface vertical turbulent transport, and thus warms the surface (Figure 5)”.

The temperature changes in the various cases appear to be similar over different continents as shown in Figure 3.

Specific Comments

p. 19083: l. 27: "Legitimate interest" seems loaded. How about "substantial interest" or "vigorous efforts to harness".

“legitimate” has been changed to “substantial”.

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p. 19084 I. 8: Thirteen million turbines to generate 4.4 TW implies 300 kW/turbine, which seems pretty small. If the bulk of the turbines were 2 MW turbines, and we assumed that they typically operated at 50% of capacity, we'd need 4.4 million turbines, rather than 13 million. What's the basis of this number? Barrie and Kirk-Davidoff (2009) present a methodology for relating a given density of wind turbines to a particular grid-scale roughness length; perhaps the authors could make use of this or another procedure.

As described in the beginning of the paragraph, the number of 13 million came from the assumed 1-MW capacity turbines with a 1/3 operating efficiency that is typical for current turbines and locations. The paper by Barrie and Kirk-Davidoff (2009) along with Frandsen (1992) and Vermeer et al. (2003) are cited in the manuscript.

p. 19085 I. 4: Confinement to shallow ocean regions is unlikely to be necessary-floating designs are in the works and likely to be deployed in a world that depends on wind for 10% of its total energy use.

The reviewer's point is well taken. We have added a new sentence in the summary: "Future advances in floating turbine technology might enable the installation of wind turbines over oceans at depths exceeding our assumed 200-meter maximum depth range, presuming that the issue of the needed long-range electricity transmission is resolved."

p. 19087 I. 5: This sentence is puzzling: "The relevant model parameters were changed to remove the amount of near-surface atmospheric kinetic energy needed to match various energy production targets." Are the roughness lengths and displacement heights given on the previous page correct, reflecting these changes, or if not, what were the actual roughness lengths and displacement heights?

This sentence described the actual procedure in defining the values of parameters that would produce the needed energy output in trial runs. Once the parameters were

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determined, they were used in the long-term integrations. The values listed in the previous page are indeed the real values used in the corresponding model runs. To avoid possible confusion, we have modified the words "parameters were changed" to "parameters were derived in short trial runs".

p. 19089 I. 3 ff: The discussion here should mention what kind of ocean model is used. If it's a slab, the authors should note that in a fully coupled run, changes in ocean fluxes might modulate the changes observed in the present experiments.

Agreed. We have added "using a slab ocean model" after "ocean-based runs". Also, the sentence: "These results might also differ from those derived using a full ocean GCM that allows changes in oceanic circulation" has also been added near the end of the paragraph.

p. 19089 I. 20: It would be good to have more discussion of the change in precipitation shown in fig. 8. Is this due to change in wind direction, causing anomalous southerly winds over the Atlantic, and northerly wind over eastern North America? That would be consistent with the analysis in Kirk-Davidoff and Keith (2008), who argued that remote climate impacts of surface roughness changes were generally due to advection changes associated with the Rossby wave response to the roughness anomaly.

Agreed. We have added after (Wang, 2007), "reflecting an alteration to the large-scale circulation by the surface roughness changes caused by wind turbines (see e.g., Kirk-Davidoff and Keith, 2008)". Also (Wang, 2009) has been cited, who discusses some of the most recent studies (e.g., using idealized GCMs) on the shift of the ITCZ caused by hemispheric scale asymmetric forcing. Please note that the remote effect through dynamical processes (with citation of Kirk-Davidoff and Keith, 2008) has also been briefly discussed in the previous text (from the end of page 19088 to the beginning of 19089).

p.19090 I.1 ff: There are lots of references for the seasonal variability of wind

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power, most recently Lu et al. (2009).

Please note that our numbers listed here regarding seasonal variability represent a quite different calculation than those currently in the literature that give only the “potential wind power” estimated without considering perturbations to the wind fields induced by the large-scale wind turbine installations.

Interactive comment on *Atmos. Chem. Phys. Discuss.*, 9, 19081, 2009.

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