Earth Syst. Dynam. Discuss., 1, C90–C93, 2010
www.earth-syst-dynam-discuss.net/1/C90/2010/

Earth System Dynamics Discussions

ESDD

1, C90–C93, 2010

Interactive Comment

Interactive comment on "Estimating maximum global land surface wind power extractability and associated climatic consequences" *by* L. M. Miller et al.

D.B. Kirk-Davidoff (Referee)

dankd@atmos.umd.edu

Received and published: 26 October 2010

This paper attempts to set an upper limit to the mechanical work able to be extracted by wind turbines from the atmosphere over the global land surface. Using a simple general circulation model, the authors show that when surface drag, parameterized by a linear drag law, is increased over a wide range, a maximum value of mechanical power generation occurs.

The general point that extensive exploitation of the wind resource over a broad area would tend to reduce the mean wind, and thus that calculations of the maximum resource for a given region must be done in a model which includes the stresses due to the turbines, has been made before (Baidya Roy et al., 2004; Kirk-Davidoff and Keith, 2008; Barrie and Kirk-Davidoff 2010; Wang and Prinn 2010). However the author's examination of the maximization problem in their simple GCM is a very interesting and provocative exploration of the parameter space of this problem. Their discussion of the limits of wind power in the context of the global kinetic energy budget is also a welcome advance.

I recommend the following changes in the paper prior to publication.

1. The back-of-the-envelope calculations in section 2.1, and figure 1 should be rephrased as questions. They do not significantly constrain the availability of wind power, since only points 2 and 5, that about 900 TW of kinetic energy are generated by the general circulation, and that wind turbines can only convert about 1/3 of the kinetic energy of the wind flowing through them into mechanical energy, are anything like a hard limit on the energy available for wind power production, and no one is claiming that wind turbines could produce 300 TW of electrical power. Points 3 and 4 (that only 1/2 of the kinetic energy is dissipated at the surface, and that 1/4 of the earth's surface is non-glaciated land) assume that dissipation wont shift from one place to another in response to an increase in drag. That's a conclusion to be drawn from modeling, not a law of nature to be invoked on an envelope.

As we noted in Barrie and Kirk-Davidoff (2010), total dissipation over large regions seems to change little when surface roughness in that region is increased strongly, despite a substantial reduction in wind speed. This supports the idea that the energy available for wind power is set by the large scale circulation. The present study's confirmation of this is an important experimental result, but should be discussed as such, not as though it were obvious a priori.

2. The simple momentum model discussed in 2.2 is flawed by the assumption of a well-mixed boundary layer. Turbines with hubs at 80 m will obviously experience a different (and larger!) wind speed than the wind at the surface. It's much more reasonable to

ESDD
1, C90–C93, 2010

treat a wind farm as a distributed array of roughness elements (the turbines), in which the drag exerted by the turbines makes the pre-existing roughness elements (grass, for example) completely irrelevant to the momentum balance in the wind farm. There is extensive justification for this in the literature (e.g. Frandsen, 2007; Calaf et al. 2010). Can the authors justify why F_{fric} should not be considered negligible in equation (1)?

3. The discussion of climate impacts should be extended to include a map of the changes in temperature and precipitation, and a discussion of their physical basis. Kirk-Davidoff and Keith (2008) claimed that climate changes due to roughness increases could be understood as essentially advective effects: wind direction changes caused more or less warm advection, which, together with accompanying cloudiness changes, explained the temperature effects. Is that what is going on here, or are the results (as in Wang and Prinn) more dominated by the local impact of surface heat flux changes?

4. The paper would be greatly improved by the addition of a test of the sensitivity of the conclusions to the vertical resolution in the boundary layer. If the lowest few model levels were no thicker than a wind turbine height, would that change the ability of the model atmosphere to skim over the turbines, reducing the climate impacts? A very high drag coeffcient, exerted over a very deep boundary layer, seems likely to maximize the climate impacts of surface roughness. Computational demands could be minimized by reducing the number of drag coefficients tested.

References:

Barrie, D.B., and Kirk-Davidoff, D.B.: Weather response to a large wind turbine array, Atmos. Chem. Phys., 10, 769-775, 2010.

Calaf, M., Meneveau, C., and Meyers, J.: Large eddy simulation study of fully developed wind-turbine array boundary layers, Phys. Fluids, 22, [015110-1]-[015110-16], 2010.

Frandsen, S.: Turbulence and turbulence-generated structural loading in wind turbine clusters, Riso National Laboratory, Riso-R-1188(EN), 2007.

Kirk-Davidoff, D.B., and Keith, D.: On the climate impact of surface roughness anomalies, J. Atmos. Sci., 85, doi: 10.1175/2007JAS2509.1, 2008.

Interactive comment on Earth Syst. Dynam. Discuss., 1, 169, 2010.