Comment on "Estimating maximum global land surface wind power extractability and associated climatic consequences," by L.M. Miller, F. Gans, and A. Kleidon (Earth Syst. Dynam. Discuss., 1, 169-189, doi:10.5194/esdd-1-169-2010, 2010).

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Miller et al. (2010) (hereinafter MGK10) claim to provide the maximum wind power availability worldwide based on three methods and that "the climatic effects at maximum wind power extraction are similar in magnitude to those associated with a doubling of atmospheric CO_2." We believe the wind power resources from MGK10, estimated as 17-38 TW over land, are low by a factor of up to four due to the unphysical nature of MGK10's calculations and the fact that such calculations are not comparable with data-derived wind resources. Further, even if MGK10's wind resources were correct and their scenario realistic, the climate consequences stated by the authors are overestimated by a factor of at least 50-100. In addition, when their scenario is put in a realistic context whereby wind energy replaces thermal power plants, the effects of wind turbines can only be no net change or a reduction in internal energy added to the atmosphere and a significant reduction in other forcings due to the elimination of greenhouse gases (GHGs) and black carbon (BC) from such power plants.

1. Discussion of Climate Consequences

First, we will start with the climate consequences, assuming for now that MGK10's 17-38 TW of wind energy extraction from turbines is correct. The area of the Earth is 5.106 x 10^{14} m^2, suggesting such extraction corresponds to a boundary-layer radiative forcing of 0.033-0.074 W/m^2. The tropopause direct radiative forcing due to doubling of CO_2 is 3.7 W/m^2 (Boucher et al., 2001), a factor of 50-100 higher than the forcing due to 17-38 TW of energy extraction by wind turbines. Despite this difference, the authors claim that "the climatic effects at maximum wind power extraction are similar in magnitude to those associated with a doubling of atmospheric CO_2." They base their conclusion about climate consequences on coarse-resolution climate simulations unverified against data. The reason for the similar result in both cases despite the factor of 50-100 difference in forcing is almost certainly because they represent wind turbine effects in their model unphysically, with a grid-cell-averaged change in a friction coefficient, rather than resolving and treating the physics of flow around individual turbines, which are subgrid phenomena with a horizontal spacing 40 times their vertical swept area and 6000-17,000 times their horizontal footprint on the ground. The spreading of subgrid effects evenly over a large grid cell (horizontally and vertically) is well known to cause erroneous feedbacks in the exact same way that any random perturbation in one variable in one location in a model grid domain causes deterministic chaotic variations in multiple variables in multiple locations, causing more feedbacks and larger changes over time (Lorenz, 1963). In the end, the simulation with the perturbation will give a noticeably different result from the simulation without the perturbation simply due to the chaotic variation of the model. When a model assumes numerous perturbations in locations where none should occur, the chaotic variations multiply.

Second, their scenario of merely adding 17-38 TW of wind is an irrelevant scenario since it ignores the fact that wind would replace existing combustion technologies, which add direct heat to the atmosphere along with CO_2, other GHGs, and BC. The 0.033-0.074 W/m^2 of heat from turbines displaces at least the equivalent heat from thermal power plants (e.g., Santa Maria and Jacobson, 2009), but wind turbines simultaneously eliminate nearly all CO_2, other GHGs, and BC from such power plants. Thus, replacing thermal plants with wind turbines serves only to eliminate nearly 100% of CO_2, other GHGs, and BC without any additional heat added to the atmosphere. MGK10 do not take into account the waste heat from traditional power plants.

Third, a more practical large-scale scenario is replacement of 50% of all 2030 energy worldwide with wind together with electrification and/or hydrogenation of transportation and other sectors (Jacobson and Delucchi, 2010). This would require ~5.75 TW (0.011 W/m^2) of wind replacing fossil-fuel heat as well as GHGs and BC. Again, this would result in no net heat added to the atmosphere (as it would displace fossil-fuel heat). Such heat added by wind represents only 0.4% of the heat added due to doubling CO_2, but that 0.4% merely replaces heat by fossil plants, so again, no net heat is added due to wind.

2. Discussion of Wind Resources

With respect to MGK10's estimation of wind resources, we believe all three methods used are unphysical as all three erroneously assume that natural kinetic energy (KE) dissipation is a proxy for wind power available and that KE extracted by wind turbines is not regenerated in the atmosphere.

First, when turbines are present, local KE extraction by them to produce electrical energy enhances KE dissipation above that of natural frictional dissipation near the surface, since turbines take KE directly from the wind flow. As such, a wind turbine at a given location forces the atmosphere to perform work, thus to be dissipative, regardless of whether that location had a low or high natural dissipation prior to the turbine installation. This is the first reason that natural KE dissipation is not a good proxy for wind power availability.

Second, the natural dissipation rate of KE accounts only for the conversion of KE to internal energy (IE), which initially increases atmospheric temperature. However, much of the added IE must be subsequently converted to potential energy (PE) via increased buoyancy, evaporation/latent heat transport/condensation, and thermal-infrared emissions/absorption, replenishing the pressure gradients. The conversion of IE to PE thus reduces the initial increase in temperature. In sum, wind dissipation ultimately cycles some portion of KE back to PE. In addition, to maintain the KE balance and compensate for the increased KE dissipation, the atmosphere must convert PE to KE (KE generation via adiabatic processes) at the same rate as the KE dissipation. In sum, enhanced KE dissipation ultimately must cause enhanced KE generation at an equal rate. MGK10 did not take into account this regeneration mechanism. For example, by assuming that F_{acc}, the rate of momentum generation by an acceleration force, is constant and equal to its current value (their Eq. 1), MGK10 implicitly neglected the feedback from KE to PE through dissipation. The current value of F_{acc} is only a function of the natural frictional force, whereas with wind extraction it also becomes a function of M, the momentum extracted by turbines.

An increase in dissipation will cause not only an equal increase in KE generation (from PE), but also a net decrease in KE in the atmosphere (Santa Maria and Jacobson 2009). The increase in KE generation occurs physically due to the fact that when turbines extract momentum, they enhance vertical and horizontal momentum gradients and pressure gradients, enhancing vertical and horizontal turbulent diffusive transport of momentum and air to even out those gradients. Although the smoothing of the gradients reduces PE, much of the PE is recreated from the enhanced IE from the enhanced KE extraction. In equilibrium the total PE (potential + internal) energy will have to increase by the same amount as KE decreases. How this increase will be redistributed between IE and PE is not precisely known. We agree with MKG10 that if turbines were only added to the atmosphere (not replacing thermal plants), they would reduce KE and slightly increase IE, and this was accounted, at least theoretically, in our studies (See Section 3). However, the resulting positive radiative forcing on the climate is lower by orders of magnitude than the estimate of GMK10, as discussed earlier, because not all IE goes toward radiative forcing.

3. Other Issues

MGK10 claim that previous studies of wind energy "neglect energy conservation," which they argue is important at large penetrations of wind based on the analysis of Gans et al. (2010). However, as elucidated in Archer et al. (2010), the Gans et al. method was erroneous, as it assumed the atmosphere is a closed channel with no sources of energy along the channel (thus no conversion of PE to KE). In Santa Maria and Jacobson (2009), net KE losses due to a wind turbine were calculated but limited to its wake. Thus, so long as turbine spacing is such that wakes do not overlap, the method used to determine global losses of KE is correct. However, we agree with MGK10 that the feedbacks to the climate system were not directly considered. However, as explained above, such feedbacks are negligible since the radiative forcing due to KE dissipation by wind turbines in the absence of replacement is only 1-2% of the radiative forcing of doubling CO_2 and since wind turbine heat will merely replace thermal power plant heat. Thus, we think such feedbacks are much smaller than MGK10 suggest.

MGK10 claim to provide three “independent” methods of calculating wind potential. These are discussed in turn.

The first method is a “back-of-the-envelope” method that rests entirely on an assumption of 900 TW of total wind power generation in the global atmosphere, referenced back to 1955 before it was possible to calculate the global potential with a 3-D computer model, let alone the available energy in the presence of wind turbines. MGK10 also state that this “900 TW of kinetic wind energy” dissipation is a “meteorological fact.” This claim is unsupported by the literature, which gives rates of KE dissipation ranging from 450-3800 TW (Stacey and Davis, 2008; Li et al., 2007; Sorensen, 2004; Peixoto and Oort, 1992; Gustavson, 1979; Lorenz, 1967). MGK10 provide no uncertainty analysis for the 900 TW number. They do not even provide error bounds. Li et al. [2007] suggest that some of the earlier estimates were too low since they did not include polar regions or high altitudes and provide an estimate of 1270 TW from one reanalysis field. Sorensen (2004, p. 86) state that direct estimates of dissipation are 4-10 W/m^2 (2000-5100 TW), much higher than 900 TW, but are generally reduced in back-of-the-envelope estimates to ensure consistency with estimates of sources of energy, suggesting the dissipation is tuned, not calculated from first principles.

Second, MGK10 use a “simple momentum model” to estimate wind power availability. They assume the KE available for wind power can be determined by a mean wind speed multiplied by a global wind stress, giving dissipation. However, as discussed in Section 2, the dissipation rate is not a proper proxy for wind energy potential. Further, they do not evaluate the wind speed values they use against in situ (e.g., sounding) or satellite data, let alone provide information about the height of the wind speed nor provide any evaluation of their wind stress. MGK10’s result depends entirely on a tunable “friction coefficient,” for which no observed value is given and for which the authors unphysically assume one value.

MGK10 make another unphysical assumption in their momentum model, namely that the momentum generation term F_{acc} is constant (and equal to its current value), whereas in the real atmosphere in the presence of wind turbines, F_{acc} would increase by the rate of momentum extraction by turbines ($F_{acc}=M+F_{fric}$ and not $F_{acc}=F_{fric}$).

Third, the authors use an overly-simplistic climate model at coarse resolution (5.6 degrees x 5.6 degrees x 10 vertical levels) with no vertical energy diffusion in the oceans, no treatment of gas photochemistry, aerosol microphysics or chemistry, cloud formation from size-resolved aerosols, spectral radiative transfer accounting for size-resolved clouds or aerosols, subgrid treatment of different soil types, subgrid treatment of vegetation, subgrid treatment of turbines, or subgrid treatment of snow on different soil types, etc, to model energy dissipation. They parameterize wind farms by erroneously assuming that each entire model grid cell is subject to a constant friction coefficient that would be increased if scattered wind turbines were added to the landscape. When friction is spread evenly over a grid cell, the individual pressure gradients resulting from individual turbines are not accounted for, giving the wrong answer.

They justify the use of this method by its previous use in Keith et al. (2004) and Wang and Prinn (2010). However, neither of those studies nor MGK10 has demonstrated that perturbing even one global model grid cell with a grid-scale friction coefficient gives anything close to the same result as resolving thousands of individual turbines in the grid cell and treating the physics of flow past each such turbine. Further none of the studies has demonstrated that they would get the same result simply by increasing the resolution of their model in the horizontal or vertical. MGK10 would have us believe that their conclusions should be about the same regardless of the poor resolution of the model they used and the lack of resolution or physical treatment of wind turbines, which are subgrid features. Individual turbines consist of a small volume of actual material and lots of open space between them. The proper method of simulating the effects of wind turbines is by resolving the turbines and the flow around them.

Finally, the authors do not compare their modeled wind speeds with data, such as with the QuikScat data shown in Figure 1, below. If they did, they would note that their North Atlantic and offshore west-coast North America winds are lower than they should be relative to mid-North-Pacific winds, a problem that does not occur in other models at higher resolution. They are also missing the strong, distinct, and well-known winds of the Great Plains of the U.S. and Canada and winds over the Sahara desert. This further supports our previous point that natural wind dissipation is not a good proxy for wind power availability.

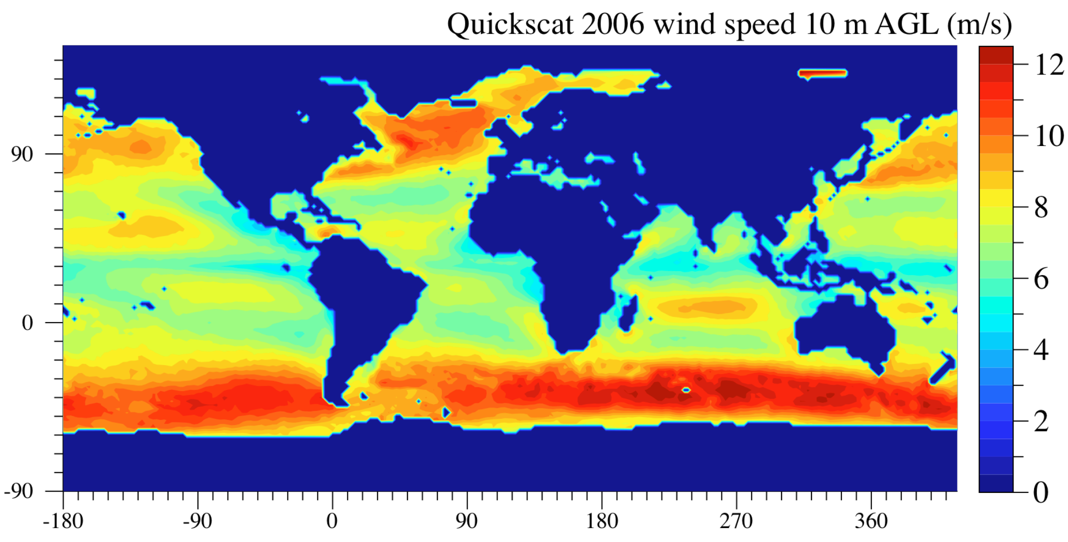
Summary

In sum, MGK10 propose climate effects of wind energy extraction that we believe are overestimated by orders of magnitude, likely because their parameterization of subgrid wind turbines with a grid-scale parameterization is unphysical and creates numerous additions of deterministic chaotic variation in their model. In addition, they ignore the fact that heat from wind energy is less than that from the thermal plants wind energy displaces. They also provide unphysical estimates of wind energy resources. They ignore the conversion of PE to new KE upon removal of KE by wind turbines and the regeneration of PE from IE in their calculations for estimating world wind resources. Also, they erroneously rely on the natural dissipation of winds in the absence of turbines as a proxy for wind power. Their 3-D model is based on an unphysical treatment of determining the effects of wind turbines on the atmosphere (the use of a constant friction coefficient) since such a method does not resolve turbines.

The total 80- to 100-m wind power worldwide, before energy extraction, over land at high-wind locations outside of Antarctica and below the Arctic Circle from data and model simulations have been independently calculated as ~72-150 TW (Archer and Jacobson, 2005; Lu et al., 2009; Jacobson and Delucchi, 2010). Wind power at all wind speeds worldwide at 100 m calculated theoretically from wind speeds, a real wind turbine power curve, and assumed turbine spacing that attempts to account for energy dissipation downwind of a turbine and KE regeneration in the wake, has been determined as ~1700 TW before energy extraction from a 3-D global model at 2 degrees resolution (Jacobson and Delucchi, 2010; Archer et al., 2010). Since only 5.75-11.5 TW of power is needed to power 50%-100% of the world for all purposes in 2030 (Jacobson and Delucchi, 2010), power extraction at 100 m amounts to <1% (11.5 TW/1700 TW) of the world's available wind power at 100 m.

In sum, we believe the results of MGK10 are erroneous in terms of their wind power estimates and exaggerated in terms of the effects of wind turbines on the atmosphere.

Figure 1. Quik Scat wind speeds at 10 m for 2006. Figure produced by D. Whitt and M. Dvorak from QuikScat data (http://manati.orbit.nesdis.noaa.gov/products/QuikSCAT.php)



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