Reply to Author's Response to Comment on "The problem of the second wind turbine – a note on a common but flawed wind power estimation method," by F. Gans, L.M. Miller, and A. Kleidon (Earth Syst. Dynam. Discuss., 1, 103-114, doi:10.5194/esdd-1-103-2010, 2010)

Cristina L. Archer!, Mark Z. Jacobson*, Magdalena R.V. Sta. Maria*

! Department of Geological and Environmental Sciences, California State University, Chico

*Atmosphere/Energy Program, Department of Civil and Environmental Engineering, Stanford University

Gans et al. (2010b) (G10b), in their reply to the comment by Archer et al. (2010), claim that "The intention of our model…should be to account only for the most important processes," thereby implying that terms they ignored, such as sources of new kinetic energy (KE) due to enhanced vertical and horizontal pressure gradients upon extraction of energy downwind of a turbine, are not important. Yet, they provide no quantitative evidence of this claim, so it is not clear why their model or its results should be correct or believed.

Second, they claim that the regenerated KE in our example 1-D model calculation has to come from potential energy (PE), but since we didn't reduce the large reservoir of potential energy in our example, we did not conserve energy, and our entire point should be disregarded. However, the authors have not shown that the enhanced conversion of PE to KE draws down pressure gradients significantly. In fact, much of the PE converted to KE during wind-turbine extraction of KE should be regenerated by the internal energy (IE) produced from the additional KE dissipation due to the turbines. The increased IE increases buoyancy, evaporation/latent heat transport/condensation, and thermal-infrared emissions/absorption, replenishing pressure gradients. Not all IE is converted to PE; the rest goes toward increasing temperature slightly. The net temperature increases is proportional to the KE dissipation minus the IE-to-PE conversion, not just to the KE dissipation. Again, they do not provide evidence that the conversion of additional PE to KE upon energy extraction is trivial, a result essential to their paper and to their claim that previous wind energy resource estimates are much too high. The burden is on them to quantify the importance of the different terms affecting kinetic energy availability in the presence of wind turbines, yet they have provided no quantification of such terms.

Finally, the authors now provide more inaccurate information about the Santa Maria and Jacobson (2009) (SJ09) paper. They provide another document that contains the same error as in their original paper, ignoring our original clarification of equations in SJ09 provided in Archer et al. (2010). Specifically, they continue to suggest that the change in KE in the boundary layer (ΔE) in SJ09 represents a KE loss alone rather than a net change (a loss minus a partial regeneration of KE). In their notation, they incorrectly assume (their Equation 3)

$$\Delta E/E_0 = P_{ex} / P_{nt} \qquad (1)$$

where P_{nt} = 1/c = average power at a given time without turbines. Thus, they assume the energy change from the equation represents only energy (or power) extracted by the turbines. Instead, the equation in SJ09 represents energy (or power) extracted minus that regenerated. The equation is exactly mathematically derived as

$$\begin{aligned}\Delta E/E_0 &= NS_w(E_{nt} - E_t) / S_aE_{nt} \qquad (2)\\ &= N[S_d(E_{nt} - E_t) - (S_d - S_w)(E_{nt} - E_t)] / S_aE_{nt}\\ &= (KE_{ex} - KE_{reg}) / KE_{nt}\\ &= (P_{ex} - P_{reg}) / P_{nt}\end{aligned}$$

where N is the number of turbines, S_w is the wake volume of a single turbine, S_a is the total boundary layer volume, S_d is the volume of air larger than the wake volume but smaller than S_a and extends over some distance far downwind of a turbine (e.g., the length of the wind tunnel in Gans et al., 2010a), E_{nt} is the kinetic energy per unit mass of the wind in the absence of turbines, E_t is the kinetic energy per unit mass of the wind in the wake of a turbine (and in the downwind volume of air in the absence of kinetic energy regeneration), $KE_{ex}= NS_d(E_{nt} - E_t)$ is the summed kinetic energy (assuming uniform air density and no kinetic energy regeneration in this term) extracted downwind of the turbine over the entire volume S_d, $KE_{reg}= N(S_d - S_w)(E_{nt} - E_t)$ is the summed kinetic energy regenerated past the wake volume (in volume $S_d - S_w$), KE_{nt} is the total kinetic energy in the boundary layer before turbines, and the P's are just the KEs divided by time. The term $NS_d(E_{nt}-E_t)$ is both added to and subtracted from the numerator.

The interpretation of this equation is that, in the absence of KE regeneration (e.g., such as in the wind tunnel assumed by Gans et al., 2010a), the loss in KE would occur over some long distance past the turbine because no mechanism would be present downwind to regenerate the wind once it has been reduced by a turbine. However, in the real atmosphere, pressure gradients and vertical turbulent fluxes of momentum produce winds continuously in front of and behind a turbine. The reason that a turbine reduces wind speed behind it, rather than only at the turbine itself, is that it reduces advection of momentum past the turbine. This does not stop the pressure gradient force and vertical turbulent flux from acting to refill the lost momentum downwind (completely by the end of the wake volume), where the vortices in the turbine wake dissipate sufficiently. In the absence of this "regeneration" of the winds, the extracted KE would be lost over a long distance. In the SJ09 study, the regenerated KE is presumed to be the KE regenerated due to pressure gradients and turbulent fluxes past the wake volume.

In sum, G10b confuse P_{ex} for $P_{ex}–P_{reg}$. They assume that the extracted KE by a turbine (or KE per unit time) is merely the KE lost in the wake volume only rather than the KE lost over the long distance past the wake. Because their numerator in Equation 1 above is too large, they ascribe to us a value of P_{nt} for a fixed $\Delta E/E_0$ much larger than is obtained from a correct interpretation of the results in SJ09. Stated another way, they ignore the kinetic energy regeneration term, which is implicitly the basis for the ΔE on the right side of SJ09, Eq. 17 (or G10b, Equation 1), as demonstrated above. This also explains why P_{ex}

can be a larger percentage of the total wind power available at 100 m than is the loss of kinetic energy relative to the total kinetic energy in the boundary layer ($\Delta E/E_0$).

G10b cannot claim, as they do, that if "we use their method" "it leads to unrealistic values." They are not using the method of SJ09. They are making up their own method yet ascribing it to SJ09.

Reference

Archer, C.L., M.Z. Jacobson, and M.R.V. Sta. Maria, Comment on "The problem of the second wind turbine – a note on a common but flawed wind power estimation method," by F. Gans, L.M. Miller, and A. Kleidon (Earth Syst. Dynam. Discuss., 1, 103-114, doi:10.5194/esdd-1-103-2010, 2010), Earth Syst. Dynam. Discuss, 1, C71-C71, 2010.

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