Supplement of response to the comment by Archer, Jacobson, Sta. Maria

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1 Detailed response to the comment by Archer, Jacobson, Sta. Maria

In the following section, we will reply to all comments made by Archer, Jacobson, and Sta. Maria (AJS10). For reference, the original comment points of AJS10 are included as indented italics.

> *The main error with GMK10 is the setup of their experiment. They make the unphysical assumption that wind turbines are in a closed channel with no sources or sinks of energy in the channel other than turbine dissipation. As a result, they ignore wind production and loss within a wake downwind of the turbine due to basic terms in the momentum equation. They also ignore the thermodynamic energy equation, which accounts for energy advection, adiabatic compression/expansion, and diabatic heating of the air.*

The intention of our model, and in our opinion of any model, should be to account only for the most important processes that describe the behavior of a natural system. It should not include processes that have little effect on the overall behavior of the system. It is the task of the modeler to determine which processes are the important ones to address the scientific question. A model is not automatically better because it accounts for a large number of processes but only makes it less understandable.

Our model was designed to be as simple as possible to highlight the major flaw in the noted wind power estimation methods — there is an implicit assumption of an infinite reservoir of energy. This has been well-hidden in modeling details thus far but with a simplified model such as this, these flaws become immediately apparent. Hence, the model is simple, but neither erroneous nor unphysical.

> *They further ignore the conversion of kinetic energy to electricity and then to heat; instead, they assume that the kinetic energy converted by wind turbines just disappears. Therefore, GMK10 do not represent real flows or real sources and sinks of energy correctly, so their method cannot conserve energy.*

Adding heat dissipated by electricity would not add a large term to the kinetic energy balance, because other heating terms are much more significant. Solar heating contributes $\approx 200\mathrm{Wm}^{-2}$, while waste heat is $\approx 1\mathrm{Wm}^{-2}$. Therefore waste heat is not important to the main point of GMK10. The usage of waste heat for power generation would also lead to a violation of the second law of thermodynamics making it possible to construct perepetual motion machines.

> *First, GMK10 erroneously assume that the only physical process affecting the wind is energy loss by the turbine and transfer of the remaining energy not dissipated by one turbine to the hub of another. They ignore all other terms in the momentum equation responsible for production, loss, and transport of winds, including the pressure gradient force (PGF), the apparent Coriolis force (ACF), local acceleration (based on velocity gradients), friction, and turbulent flux divergence. It can be shown (e.g., Figure 1) that when these effects are accounted for, winds lost at a turbine are regenerated downwind by the PGF and ACF in the horizontal and by the vertical transport of horizontal momentum. The ultimate source of energy for these terms is sunlight, whose differential heating creates pressure gradients that drive winds. GMK10 ignore this renewable energy as a source of regenerated winds within a wake downwind of turbine.*

The main claim that AJS10 make in their comment is that processes not included in our simple model lead to a 'regeneration' of kinetic energy within the wake without affecting anything outside of a defined wake volume. This assumption unavoidably leads to a violation of the first law of thermodynamics and therefore must be false. Energy can not be generated, it can only be transformed from one form into another. This means, when you increase the kinetic energy within the turbine wake as a result of extraction, it must decrease elsewhere.

We never made the claim there the is no momentum and energy transport from higher altitudes to the wake volume, but as the word 'transport' suggests, the kinetic energy is moved from higher altitudes to the wake volume, while some is transformed into heat during that process. Accounting only for the energy increase in the wake volume and neglecting the decrease of energy in higher-altitude winds simply does not conserve energy. The same is valid for the pressure gradient force (PGF) which is a manifestation of potential energy due to pressure gradients. However, as this force is doing work on the system, the potential energy decreases by at least the same amount, which in turn leads to a reduction of the reservoir of potential energy. Naturally, the pressure gradient is regenerated by differential solar heating, but not at an infinite rate.

In the tunnel setup, one can interpret the 100MW entering the tunnel as the rate at which kinetic energy is continually generated at the model boundary. To account for the contribution of higher altitude wind, one could simply increase the height of the tunnel to an arbitrary value and increase the initial power input but related to power extraction potentials using the methods of SJ09, there will still be an imbalance in the energy budget.

The Coriolis force is an apparent force always perpendicular to the velocity vector — coriolis does not add power to a system.

> *With respect to Figure 1, as wind speeds first decrease within a wake, the vertical gradient in wind speed increases, increasing the downward turbulent flux of faster winds from aloft to hub height and decreasing the downward flux from hub height to the surface, replenishing winds downwind of each turbine. Similarly, the horizontal pressure gradient force continuously acts on the winds at a given height and contributes establishing the quasi-geostrophic balance among the PGF, ACF, friction, and turbulent flux divergence terms.*

It is not the three-dimensional fluid mechanic modeling that makes the difference between their model and our simple model, it is a difference in boundary conditions. Their model setup assumes a constant pressure gradient force which represents an infinite reservoir of momentum. Their model setup also assumes another infinite higher altitude geostrophic motion energy reservoir which is responsible for the downward mixing of momentum. Both of these forces, the pressure gradient force, as well as the vertical velocity gradient, do not get depleted as they are dissipated. Assuming a fixed pressure gradient force and high altitude velocity may be a valid approximation for modeling the impacts of single turbines or small wind farms and may reproduce experimental results. However, when linearly extrapolating from the impacts of 4 turbines (AJS10 Fig.1) to millions or even the 2.87 billion 1.5MW turbines suggested by AJS10, the model boundary condition approximation that was valid at the small scale no longer holds.

> *The simple model in Figure 1 is by no means complete nor indicative of actual spacing required to optimize wind farm energy output, as the most rigorous and ultimately accurate method of simulating the effect of wind turbines on the atmosphere is with a three dimensional model at high resolution accounting for many more terms than used for Figure 1. However, it illustrates plainly that GMK10s assumption that wind is not regenerated at all within a wake downwind of a turbine is not realistic. The distance downwind that regeneration occurs varies with meteorological conditions, but the fact is, the wind always regenerates at some point so long as horizontal pressure gradients and vertical wind speed gradients exist. Thus, the loss of energy in the atmosphere due to wind turbines occurs within some wake volume. Within that volume, wind speeds first decrease then increase, eventually converging to the background wind speed. This was the assumption in the papers criticized by GMK10, and this assumption is physical. GMK10, on the other hand, claim that wind speeds at the end of the wake can only be lower than the wind speeds upstream of a turbine; thus, they believe it is not possible for winds to regenerate in the wake. This assumption is invalid. In sum, the major error in GMK10 is their assumption of channel flow for wind energy, where no sources or sinks of energy occur within the channel. In their scenario, they assume that the only exchange of energy is loss by turbine dissipation. This would be the case for turbines in a river of water, but such is not the case for atmospheric flows.*

As shown in the response above AJS10 correctly recognize that their method does not work when it is applied in a setup where the rates of energy addition from sources outside the wake are too low. That was the purpose of our simple model experiment and the overall intention of our paper: to show that kinetic energy generation rates actually limit available wind power at large scale. We are also glad that we seem to agree on this. However, the authors miss the apparent implications. We want to emphasize that in the papers we criticize, no attempt was done to estimate the generation rates of kinetic energy. Their calculations of global wind power are based on modeled or measured hub height wind velocities only and then integrated with specific wind turbine characteristics. At no time in this process can the wind turbines deplete the actual reservoirs of kinetic and potential energy.

We would again clarify that this is not done in all wind power related papers. The work of Keith et al, 2004 and Wang & Prinn, 2010 do not, in our opinion, illustrate the same flaws currently undergoing discussion with AJS10. We recognize the need to determine an estimate of very-large scale wind power potential using momentum extraction from atmospheric flow using a general circulation model with full 3D dynamics. We recently completed this scientific study and it is now available in the discussion section of this journal (Miller, Gans, & Kleidon, 2010).

> *GMK10 argue further that several papers make the same unrealistic channel flow assumption as they themselves do. However, this is not the case. For example, Sta. Maria and Jacobson (2009), hereinafter SJ09, assume that the atmosphere is three dimensional (as it is in reality). The losses of energy due to a wind turbine occur in a wake volume. Within the wake volume, wind is regenerated so that by the end of the wake, the wind speed has regenerated to that upstream of the turbine (e.g., Figure 1). One can argue that the wake volume is too large or too small (since the wake volume was simply estimated based on historic spacing in actual wind farms), but one cannot claim that the methodology accounts for energy less accurately than does the methodology of GMK10 since GMK10 ignore all sources of kinetic energy aside from the initial wind entirely. In SJ09, energy is lost in the wake volume, and that is accounted for, and energy gained with increasing distance from the turbine within the wake volume is due to regeneration of winds from solar energy producing pressure gradients, which ultimately produce all winds.*

This is not true. As was stated before, even in part by AJS10, the wind energy within the wake can be replenished by the contribution of kinetic energy from other areas. AJS10 does not consider the requirement that to increase the kinetic energy in one area means decreasing it in another. This is not accounted for by SJ09 when they estimate the kinetic energy reduction within the lowest 1km of the atmosphere.

> *GMK10 make a further error in their claim that the calculation in SJ09 that the worlds energy needs can be met by extracting only 0.007% of the kinetic energy of the lowest 1 km of the atmosphere implies that the wind power in the lowest km must be 242,000 TW. This number, which is a factor of over 140 higher than available world wind energy, was surmised by Gans, Miller, and Kleidon but is not derivable from a correct interpretation of the data in SJ09. The authors did not attempt to replicate the calculations in SJ09 (Eq. 17 in particular), for if they did, they could not have made such a significant error. Instead of contacting the authors of SJ09 to clarify their uncertainty, they submitted it for publication and broadcast it at a scientific meeting, calling it a flawed methodology that violates atmospheric energetics. GMK10s error arose because they confused power at a given height (hub height) with energy integrated over the boundary layer. For example, they divided their estimate of world power extracted by wind turbines at hub height (e.g., 100 m) needed to supply their estimate of power demand of 17 TW, by the fractional loss in boundary layer integrated energy, 0.00007, calculated in SJ09. This gives the erroneous 242,000 TW number. However, wind power extracted by a turbine through its swept area centered at hub height is not a boundary layer averaged value; nor does power (energy per unit time) even have the same units as energy. Power extracted by a turbine is the energy per unit time passing through a turbine swept area that is converted to electric power. Atmospheric kinetic energy lost in the wake of turbine is the kinetic energy averaged over the wake volume as if the turbine were absent minus that over the same volume as if the turbine were present. The kinetic energy in the presence of the turbine must account not only for the loss of energy due to its extraction by the turbine, but also production along the wake, as demonstrated in Figure 1 here. The boundary layer averaged fractional loss in kinetic energy is that difference multiplied by the ratio of the volume of all turbine wakes to the volume of the boundary layer, all divided by the kinetic energy of the entire boundary layer. In sum, power extraction alone can-*

not be equated properly with energy extraction minus production and hub height power extraction cannot be equated with boundary layer integrated energy change. GMK10 mixed up two unrelated parameters, resulting in a factor of > 140 *error in their result.*

A detailed description of how the number in question (242,000) was derived using the methodology of SJ09 is included in the following chapter.

2 Our interpretation of the total atmospheric generation rate implied by Sta. Maria & Jacobson, 2009 (SJ09)

Here, we will illustrate how we use their method to illustrate that it leads to unrealistic values of total wind power in the atmosphere using the methods of Sta. Maria & Jacobson, 2009 (SJ09). This chapter is included separately from the main comment response because it does not reflect the main intention of our paper and could be entirely removed without altering our main message. As AJS10 did include an additional reference not initially included in SJ09, we would now modify our previously derived estimate based on SJ09 from 242000TW to 164000TW, which is again nearly equivalent to the solar constant and completely unattainable for global wind power extraction. For clarification, the generation rate of global wind power has been shown to be approximately 900 TW [Peixoto and Oort(1992), Li et al. (2007)].

We will first have a look at Equation 17 of SJ09 which they use to calculate the relative kinetic energy reduction $\left(\frac{\Delta E}{E_0}\right)_{tot}$ within the lowest 1km of the atmosphere by wind turbines. Using their notation:

$$\left(\frac{\Delta E}{E_0}\right)_{tot} = \frac{N S_{wake}}{S_{atm}} \frac{\sum_{V_0=5}^{25} \Delta E(V_0) f_8(V_0)}{\sum_{V_0=5}^{25} (1 - A_{land}) E_0(V_0) f_{8.6}(V_0) + A_{land} E_0(V_0) f_{4.8}(V_0)} \tag{1}$$

where f_x are the Rayleigh wind speed distributions centered around x, A_{land} is the fractional land area, ΔE is the kinetic energy reduction in the wake volume and E_0 the kinetic energy in the undisturbed flow. Note that due to a missing effect of kinetic energy removal on V_0, all parameters are independent of N, so the kinetic energy reduction within the lowest 1km of the atmosphere is proportional to the number of wind turbines.

In the next equation (18) of SJ09

$$N = \frac{E_S}{E_t} \tag{2}$$

where E_t is the power extracted by a single turbine and E_S is the overall extracted power. Here, they assume that the extracted power is proportional to the number of turbines, again neglecting any possible atmospheric feedback effects and limited generation rates.

In order to not get confused with kinetic energy and power, which indeed have different units, we rather use $P_{ex} = E_S = N \cdot E_t$ and $P_i = E_t$, which makes it clearer that it is a rate of energy extraction in contrast to ΔE and E_0 which are kinetic energies but have the same symbol in SJ09. Replacing N in equation (17) by $\frac{P_{ex}}{P_i}$ results in:

$$\left(\frac{\Delta E}{E_0}\right)_{tot} = c \cdot P_{ex} \tag{3}$$

where c stands for the remaining terms in Equation 1 that do not depend on the number of turbines. This shows that under the assumptions made by SJ09, the kinetic energy reduction within the lowest 1km of the atmosphere is proportional to the hub-height power extraction, though being two different quantities, as AJS10 mentioned in their comment.

SJ09 then used their assumed global electricity demand of 11.5TW (a number that was not referenced or mentioned in SJ09, leading us to substitute the current global power demand of 17TW [EIA (2009)]) to arrive at the reduction in kinetic energy of 0.007%, resulting in $c = 6.08 \cdot 10^{-18} W^{-1}$. Since we assumed a current human energy demand of 17TW in our initial calculation, we arrived at $c = 4.12 \cdot 10^{-18} W^{-1}$.

In their comment,AJS10 extend this calculation by increasing the number of installed turbines to $2.87 \cdot 10^9$, which would cover Earth with turbines and call the resulting maximum power of 1700TW a "theoretical limit" for wind power extraction. This alone should be suspect, since the generation rate of the whole atmosphere is estimated to be in the range of 900TW [Peixoto and Oort(1992), Li et al. (2007)] , but at least these numbers have the same order of magnitude.

The error of their method gets obvious when one uses equation (17) of SJ09 to estimate the reduction of kinetic energy in the lowest 1km of the atmosphere for that limit case: it is $\left(\frac{\Delta E}{E_0}\right)_{tot} = c \cdot P_{ex} = 6.08 \cdot 10^{-18} W^{-1} \cdot$ 1700TW $\approx 1\%$ only. This means that, following SJ09, it would be theoretically possible to continuously extract 1700TW of kinetic energy from the boundary layer, leaving the flow therein nearly unchanged, which means the atmosphere would still generate enough KE to sustain its motion. To calculate a number that demonstrates this fact, we used SJ09's equations to calculate how much power would be extracted if one installed a number of wind turbines that reduces the kinetic energy in the atmosphere to 0 or $\left(\frac{\Delta E}{E_0}\right)_{tot} = 1$. This would be a measure of available power in the system and one obtains $\frac{1}{c} = 242000$TW, assuming an energy demand of 17TW. Even assuming a reduced global electricity demand of 11.5TW, one would arrive with a total energy generation rate of 164000TW, which is still very close to the solar constant and orders of magnitude larger than current estimates of kinetic energy generation rates in the atmosphere.

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