Response to M. Z. Jacobson & C. L. Archer on "Comment on 'Estimating maximum global land surface wind power extractability and associated climatic consequences' by L.M. Miller, F. Gans, and A. Kleidon"

L.M. Miller, F. Gans, and A. Kleidon

We thank M.Z. Jacobson and C.L. Archer (Jacobson & Archer, 2010a) for their comment. We would also like to thank D.B. Kirk-Davidoff (Kirk-Davidoff, 2010) and J.C. Bergmann (Bergmann, 2010) for responding to the Jacobson & Archer (2010a) comment and then to M.Z. Jacobson and C.L. Archer (Jacobson & Archer, 2010b, 2010c) for replying to the comments by D.B. Kirk-Davidoff and J. C. Bergmann. In our response here, we will incorporate the associated responses to Jacobson & Archer, 2010a by J.C. Bergmann (2010) and D. Kirk-Davidoff (2010) and the subsequent responses by Jacobson & Archer (2010b, 2010c).

In order to clarify our response while also responding to all points mentioned, we will categorize our response into 5 components:

1. Clarifying our main message
2. Main disagreement point - wind velocity is not a proxy for extractable wind power at very large-scales (continental to global)
3. Major comments
    a) wind power is renewable but finite
        1) How to conceptualize the process hierarchy?
        2) simple momentum balance model

        3) general circulation model
    b) neglect complete regeneration of kinetic energy
    c) variability of global dissipation estimates
    d) climate consequences of wind power extraction
    d) chaotic effects result in our climate consequences
    e) cannot model wind power as a drag coefficient
    f) dependence of results on model resolution

4. Minor comments
    i) wind dissipation as a wind power proxy
    ii) waste heat contribution from traditional power plants

5. Author Overview

1. Clarifying our main message

Our study demonstrates the necessity of considering the generation rate of kinetic energy in the atmosphere when estimating maximum wind power extractability - reviewers and commenters do raise several interesting points but this main message remains the same. As our original title suggests, the methodologies and assumptions included in this study were intended to provide an estimate of maximum global land surface wind power extractability. Additional processes could certainly be included that would change the realizable wind power potential on a global scale, but in our opinion, the main influences on wind power availability in the atmospheric boundary layer are adequately captured here. The original manuscript relied on 3 distinct methods: a back-of-the-envelope estimate, a simple momentum balance model estimate, and a sensitivity study using a general circulation model of T21 spectral resolution and 10 vertical layers. Based on the general and referee comments, it would appear that our original intention should be substantiated further. To address this, an additional suite of general circulation model simulations at T21 and 20 vertical layers, T42 with 10 vertical layers, and T42 with 20 vertical layers have been completed and will be added to the final manuscript. The general agreement between all the estimates adds scientific substance to the estimates and analysis but is also more forthcoming about the range of resulting estimates. Should future estimates significantly exceed our range of estimates or exceed the global generation rate of kinetic energy in the atmosphere($\approx$ 900 TW by Kleidon, 2010b), the estimate should be seriously reconsidered.

2. Main disagreement point - wind velocity is not always a proxy for extractable wind power

Many previous large-scale quantifications of extractable wind power wrongfully use wind velocity as a proxy for wind power (Jacobson & Masters, 2001; Archer & Jacobson, 2003; Archer & Jacobson, 2005; Archer & Jacobson, 2007; Archer & Caldeira, 2009; Lu et al., 2009; Santa Maria & Jacobson, 2009; Jacobson & Delucchi, 2010). We would agree that on a small-scale, extractable wind power can be estimated by quantifying the air density (ρ) and wind velocity (v) as $\frac{1}{2}\rho v^3$ but this cannot apply to very-large or global scales because:

1. each wind turbine, because of it's kinetic energy extraction, must result in a change in the global mean wind velocity
2. wind velocity quantifications at any model resolution or measurement density do not indicate the generation rate of kinetic wind energy in the atmosphere
3. wind turbines at any scale or of any quantity must alter the generation rate of kinetic wind energy in the atmosphere

An alternative view in strong opposition to these simple facts is reflected in the comments by Jacobson & Archer (2010a, 2010b, 2010c). For example, in Jacobson & Archer (2010c) they state:

> *"Energy loss occurs in the [wind turbine] wake, but not outside the [wind turbine] wake. Observations indicate that wind speeds some distance past turbines are similar to those in front of the turbines. That distance was defined as the wake distance. If this were not the case, wind should be reduced infinitely downwind, all the way around the world back to the original turbine, which clearly does not occur" from Jacobson & Archer (2010c).*

Jacobson & Archer (2010a) reinforce this and similar statements with previous research (Santa Maria & Jacobson, 2009) and by noting discrepancies between our quantifications of boundary layer dissipation (ECMWF ERA-40 in Fig. 2, general circulation model in Fig. 4, both in Miller et al. (2010)) and wind velocity measurement data (*e.g.* Quikscat data for 2006 at 10-meters from Fig. 1, Jacobson & Archer (2010a)). Although dissipation and wind velocity are related, they cannot be directly compared. Estimating global land-based wind power extraction potential is only related to the generation rate of kinetic wind energy in the atmosphere, which we assume in climatic steady-state must be equal to the dissipation rate.

Throughout this collective response, we will take our original top-down viewpoint of global wind power but also respond to each significant concern noted by Jacobson & Archer (2010a, 2010b, 2010c). We are very thankful for the opportunity of this open discussion forum to clarify these issues and look forward to the finalization of an improved final manuscript.

3a. Major comment - global wind power is renewable but finite

The ultimate limiting factor of wind power extraction from the atmosphere is the generation rate of atmospheric winds - this fact has nothing to do with engineering or economic constraints (Gustavson, 1979). Previous research suggests this generation rate is currently maximized for present-day radiative forcing (Lorenz,1960; Paltridge, 1978; Lorenz, 2001; Kleidon, 2003; Kleidon, 2006; Kleidon, 2010a, Kleidon, 2010b). Wind turbines are one type of atmospheric perturbation. Thus, very large-scale wind turbines unavoidably contribute additional turbulence during kinetic energy extraction, decreasing kinetic energy downwind, and because of the induced inefficiency to the atmosphere, decreased energy dissipation in response to a decreased generation rate of kinetic wind energy at the global scale. We used 3 different estimation methods to validate the correlation between wind power extraction and the generation rate of atmospheric winds. All 3 methods were criticized by Jacobson & Archer (2010a, 2010b, 2010c) as not applicable to estimating the maximum extraction of near-surface wind power over all non-glaciated land surfaces. We strongly disagree for the reasons noted below.

- **3.a.1.** Earth system process hierarchy (originally referred to as the back-of-the-envelope estimate) - Our intended purpose of using this illustration is to outline the key components in any thermodynamically consistent estimate for global land-based wind power. To clarify our own revised process hierarchy estimate (Fig. 1), taking into account the comments by J.C. Bergmann (2010) and D. Kirk-Davidoff (2010):

  (1) 175,000 TW ≈ incoming solar radiation (Kleidon, 2010b)
  (2) 45,000 TW ≈ differential solar heating (Kleidon, 2010b)

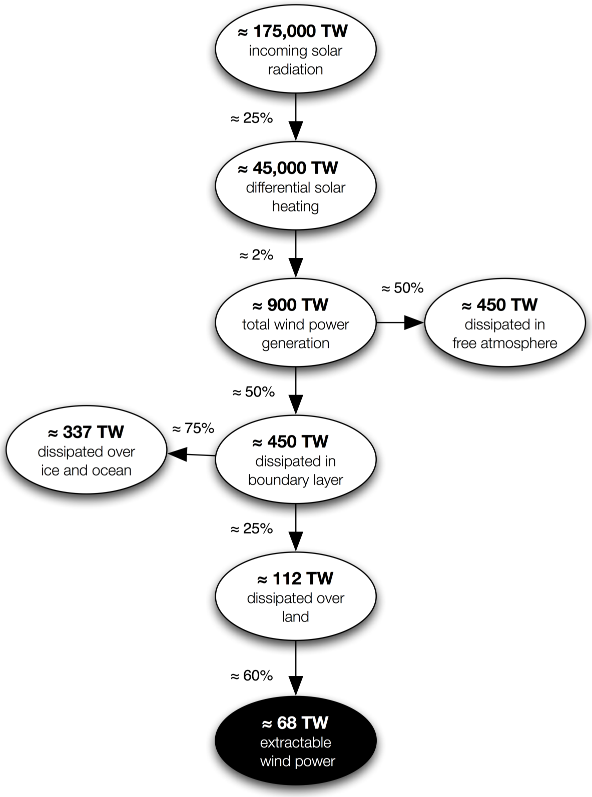

Figure 1: A very simple Earth system process-based hierarchy to illustrate the processes related to wind power extraction from the atmospheric boundary layer over land.

(3) 900 TW ≈ total atmospheric wind power generation using a 2% conversion efficiency (based on Lorenz (1960) and Kleidon, (2010b))

(4) 450 TW ≈ dissipation in the atmospheric boundary layer is ≈ 50% of total atmospheric dissipation (Peixoto & Oort, 1992)

(5) 112 TW ≈ dissipation over land (≈ 25% global surface area)

(6) 67 TW ≈ extractable wind power (assuming Lanchester-Betz Limit of ≈ 60% (Lanchester, 1915; Betz, 1920, as per the reviewer comment by D. Kirk-Davidoff (2010)).

To illustrate an alternate example, imagine Earth with ≈ 4-times the incoming solar radiation but the same atmospheric dynamics:

(1) 680,000 TW $\approx$ assuming incoming solar radiation is $\approx$ 4-times present-day forcing

(2) 170,000 TW $\approx$ differential solar heating

(3) 3,400 TW $\approx$ total atmospheric wind power generation using a 2% conversion efficiency

(4) 1700 TW $\approx$ steady-state dissipation in the boundary layer using 50% of total atmospheric dissipation

This theoretical result assumes optimum wind power extraction from the entire alternate-Earth atmospheric boundary layer yet is very similar to that of Jacobson & Archer (2010a) when they state:

> *"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 [kinetic energy] regeneration in the wake, has been determined as* $\approx$ **1700 TW** *before energy extraction from a 3-D global model at 2 degrees resolution (Jacobson and Delucchi, 2010; Archer et al., 2010)." from Jacobson & Archer (2010a).*

Given that our conceptualized process hierarchy does make several simple assumptions, it also immediately identifies an error in the incoming solar radiation assumed by Jacobson & Archer (2010a) or for the maximum conversion efficiencies assumed in our process-based hierarchial estimate (Lorenz, 1960; Kleidon, 2010b, Peixoto & Oort, 1992). These discrepancies diverge further from Miller et al. (2010) when Jacobson & Archer (2010a) later state:

> *[currently required] "...power extraction at 100 m amounts to <1% (11.5TW /1700 TW) of the world's available wind power at 100m," from Jacobson & Archer (2010a) based on the assumption that 11.5 TW of electricity can compensate for the 17 TW of current fossil fuel-based demands.*

No! Earth system dynamics do not generate 1,700 TW in the total atmosphere so 1,700 TW is not continually available for extraction at

100-meters, or even from all atmospheric layers of Earth. Furthermore, there are unavoidable physics-based limitations that prevent all kinetic energy from being extracted from a flow — this makes simply dividing 2 numbers incorrect as shown by Gans et al. (2010) using the methods of Santa Maria and Jacobson (2009). A simplified analogy would be a stream with one turbine, two turbines, or infinite turbines — turbulence from each turbine must affect all others and a stream without velocity (*e.g.* infinite turbines) results in no kinetic energy extraction.

To achieve 1,700TW of electricity, as appears to be achievable given the quote by Jacobson & Archer (2010a) above, again using Earth dynamics and an optimum condition where all wind power is within a frictionless cylinder so it cannot escape the boundaries (*e.g.* similar to a tidal channel with a web of frictionless turbines based on Garrett & Cummins (2007)) and 100% conversion from mechanical power to electricity:

(1) 2,000,000 TW ≈ incoming solar radiation (≈11-times current solar forcing)

(2) 500,000 TW ≈ differential solar heating

(3) 10,300 TW ≈ total atmospheric dissipation using a 2% conversion efficiency from differential solar heating

(4) 5,150 TW ≈ global boundary layer dissipation with 50% of total atmospheric wind dissipation occurring in the boundary layer **optimistically assumed here to be available for extraction at 100m**)

(5) 3,400 TW ≈ extracted power (maximum extraction achieved with 66% extraction with 33% allowed to pass through the turbines to maintain flow - Garrett & Cummins, 2007)

(6) 1,700 TW ≈ maximum electricity production possible from global 100-meter winds by Jacobson & Archer, 2010a (50% lost as wake turbulence, 50% extracted - Garrett & Cummins, 2007)

The scientific reasoning based on these simple assumptions identifies several immediate discrepancies to our understanding of the Earth system. The steps are easily followed and supported by sound scientific references. In reference to the process hierarchy in question though, Jacobson & Archer (2010a) only dispute one step:

> *[Miller et al., 2010 back-of-envelope estimate] "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," from Jacobson & Archer (2010a).*

No. Our assumption is actually based on referenced and scientifically defensible ratios of energy starting with the present-day incoming solar radiation influencing the atmosphere by differential solar heating which results in $\approx$ 900 TW of dissipation in the total atmosphere. Our reference to Lorenz (1955), a paper that has been cited 512-times, 23 of those citations in 2010 (as of Dec. 8, 2010 using ISI Web of Knowledge) allows us to state the 900 TW dissipation value as a general meteorological fact (disputed by Jacobson & Archer, 2010a). This unavoidable energy chain explains why dissipation values are routinely tuned in models (noted by Jacobson & Archer, 2010a) , as measuring total atmospheric dissipation directly is impossible, while the importance in equating energy-input and energy-output should be obvious.

Because of its simplicity and numerous assumptions, the process hierarchy perspective is only applicable to achieving a very rough estimate that relates to the present-day solar input and thereby the generation rate of kinetic wind energy in the atmosphere. Its benefit is derived from how the transparency allows the assumptions to be clearly illustrated. The resulting estimate of extractable wind power is not intended to be our best estimate but rather provides a scientific guess as to the result while identifying areas of understanding or lack-thereof. The lack of error bounds (requested by Jacobson & Archer, 2010a) is an outcome of the simplicity and its overall intention. We expect feedbacks to the atmosphere that cannot be captured with this level of simplicity, leading us to explore more complex methods. The contribution to understanding the generation rate of kinetic wind energy in the atmosphere of the process hierarchy clearly validates its place within the final manuscript.

- **3.a.2.** Simple momentum balance

The simple momentum balance model enables the Earth system process hierarchy estimate to be taken further, but with a different input dataset and an associated feedback from the atmosphere from additional kinetic wind energy extraction. Using the ECMWF ERA-40 surface stress and 10-meter wind velocity at a resolution of 2.5 ° by 2.5 °, an approximation for mean global boundary layer dissipation was calculated to be 513 TW. By adding an additional friction coefficient to these simple equations, the feedback between dissipation and extracted kinetic energy was derived — this shows that there is a peak amount of kinetic energy that can be extracted, after which, additional friction results in less extraction and less dissipation.

In this simple model, we assume that F_{acc} can be assumed constant but this is contrary to what Jacobson & Archer (2010a) expect:

> *"whereas in the real atmosphere in the presence of wind turbines, F_{acc} would increase by the rate of momentum extraction by wind turbines ($F_{acc} = M + F_{fric}$) and not ($F_{acc} = F_{fric}$)" from Jacobson & Archer (2010a)*

where F_{acc} is the rate of momentum generation by an accelerating force, F_{fric} is the frictional force resulting in boundary layer turbulence, and M is the rate of momentum extraction. Assuming this statement by Jacobson & Archer (2010a) is correct, then an infinite number of turbines ($M = \infty$) could produce an infinite amount of power, as F_{acc} would continue to increase with increased extraction. **This is a perpetual motion machine!** Miller, Gans, and Kleidon have no desire to recreate their perpetual motion machine (also identified by Bergmann (2010) in direct reference to Jacobson & Archer (2010a)) so we will maintain the simple momentum balance model with the associated feedback of F_{fric} and M (surface drag + wind turbines).

To further understand the validity of using a 10-meter wind speed and surface drag derived from ECMWF ERA-40 reanalysis data to approximate boundary layer dissipation, we will compare it to our general circulation model at T21 and T42 spectral resolution and 10 vertical layers where boundary layer dissipation is explicitly parameterized. The general agreement is noted in Fig. 2.

- **3.a.3.** general circulation model

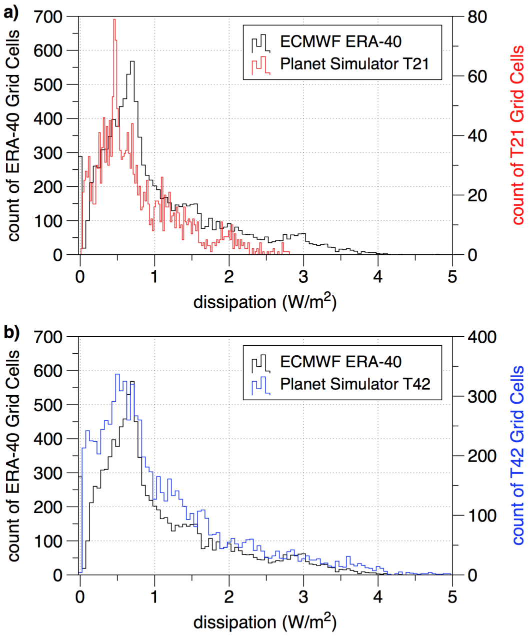

Figure 2: Comparison in the mean boundary layer dissipation values of grid values in the estimated ECMWF ERA-40 Reanalysis Data (ECMWF, 2004) and general circulation model (Planet Simulator) simulations with 10 vertical layers. We assume that in steady-state, the generation rate of kinetic wind energy is equal to the dissipation rate. In **a)**, the simulation at T21 and 10 vertical layers shown in red (2,048 grid values with a mean = 0.76 W/m^2) is compared with the estimated ECMWF ERA-40 reanalysis data (10,512 grid values with a mean of 1.05W/m^2). In **b)**, the simulation at T42 and 10 vertical layers shown in blue (8,192 grid values with a mean of 1.09W/m^2) is compared with the estimated ECMWF ERA-40 reanalysis data (10,512 grid values with a mean of 1.05W/m^2). Note the absence of T21 dissipation values $> 3W/m^2$ in (**a**), clarifying why this resolution will result in less extractable wind power compared to the T42 simulation (**b**).

After criticizing two other previously refereed studies (Keith et al., 2004; Wang & Prinn, 2010) that use a parameterization of wind power extraction very similar to that used by Miller et al., (2010), Jacobson & Archer (2010a) make an excellent point:

> *"none of these studies has demonstrated that they would get the same result simply by increasing the resolution of their model in the horizontal or vertical, " from Jacobson & Archer (2010a)*

We view this as an excellent opportunity to further reinforce the validity of our assumptions and the similarity in estimates. Using the same model (Fraedrich et al., 2004) but altering the horizontal resolution did indeed change the results and the general agreement between model simulations and the ECMWF ERA-40 reanalysis data (Fig. 1). These variations in horizontal resolution also affect the resulting dissipation and extracted power (Fig. 3).

It is clear throughout the comments by Jacobson & Archer (2010a, 2010b,2010c) that they have some severe concerns regarding the general circulation model we utilized. We also fully recognize that each general circulation model will result in slightly different estimates than those described in Miller et al. (2010). **All models are simplifications**, with each having limitations related to that model's original intention. Including additional processes such as vertical energy diffusion in the oceans, aerosol microphysics or chemistry, subgrid treatment of vegetation, and even the subgrid treatment of turbines (noted by Jacobson & Archer (2010a) as supportive evidence of why the model we utilized which lacks these processes cannot produce scientifically defensible results) will result in different wind power estimates, but they still miss the main focus of our paper — the generation rate of kinetic wind energy in the atmosphere is critical to estimating very large-scale wind power extractability.

Covering all non-glaciated land surfaces is an extreme scenario that we never expect to be realized. Given this, using a general circulation that is highly tuned to reproduce current conditions but also includes many of the above noted 'essential processes' would certainly be interesting, but it remains unclear if the resulting estimate is more or less representative of our one Earth with all land surfaces covered in wind turbines extracting kinetic wind energy from the atmospheric boundary layer. Until further understanding of Earth System dynamics to wind power extraction can be shown to be different than our viewpoint and supporting results, a wind power extraction sensitivity (*i.e.* progres-

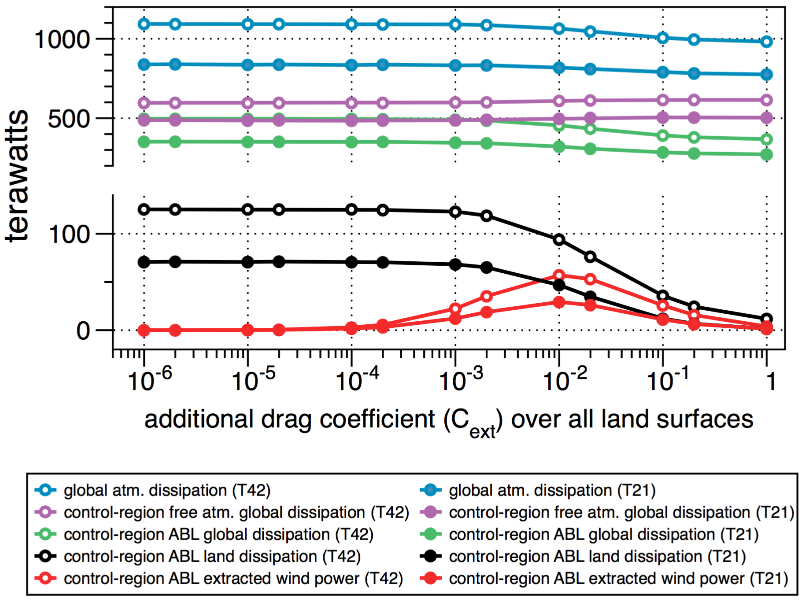

Figure 3: A sensitivity analysis of T21 and T42 spectral resolution and 10 vertical levels were completed to show the variability present in the model results related to dissipation and wind power extraction. Similar sensitivities were also completed with 20 vertical levels with very similar results (within 1%, not shown). Overall, T21 with 10 vertical levels shows that total atmospheric dissipation decreased from 838 TW to 819 TW with a peak wind power extraction of 29 TW. T42 with 10 vertical levels also shows a total decrease in atmospheric dissipation from 1094 TW to 1065 TW with a peak wind power extraction of 57 TW. Both sensitivities show corresponding decreases in the control-region atmospheric boundary layer (ABL) to additional drag: T21 = 70.7 TW (control) to 46.8 TW (maximum extraction), T42 = 125.0 TW (control) to 94.1 TW (maximum extraction). We attribute the differences between the T21 and T42 model resolutions to differential radiative forcing effects and the model parameterization of surface drag / topography. These dissipation values without wind power extraction are in agreement with previous estimates (Peixoto & Oort, 1992; Li et al., 2007). with the T42 simulation with 10 vertical levels being very similar to the estimated ECMWF ERA-40 reanalysis (Fig. 2b).

sively increasing the drag coefficient over land grid points (C_{ext})) using a general circulation model of intermediate complexity (*e.g.* Fraedrich et al., 2004) with multiple horizontal and vertical resolutions is both scientifically sensible and defensible with its range of estimates.

Although Jacobson & Archer (2010a) also criticize other large-scale wind power studies, we are encouraged by the similarity of our results with those of Keith et al. (2004), Kirk-Davidoff & Keith (2008), and Wang & Prinn (2010). We are optimistic that all of these results will be revisited as general circulation models continue to increase in complexity based on new understanding. Still, the generation rate of kinetic wind energy in the atmosphere is the keystone to understanding wind power extraction rates and is unlikely to significantly change in the future. We believe the necessary processes to estimate very large-scale wind power extraction are included in the general circulation model we used and are thermodynamically consistent with our more simplified estimates.

3.b. Major comment - neglect complete regeneration of kinetic energy

Thirty-one years ago, M.R. Gustavson (1979) took the viewpoint that there were ultimate limits to wind power utilization that were not dependent on engineering constraints or economics. In this seminal paper he stated:

> *" A distinction must be made between the amount of kinetic energy in the wind and the rate at which energy can be continually extracted. This is essential not only as an antecedent to the conclusions reached herein [estimating possible large-scale wind power extraction rates], but also because of the confusion surrounding this topic [content of kinetic wind energy in the atmosphere versus the replenishment rate]." from p.14 of Gustavson (1979).*

This top-down approach is very similar to the approach we utilized in this study (Miller et al., 2010) but is in stark contrast to the direct comments by Jacobson & Archer (2010a, 2010c) when they clarify how they model the wind resource as:

> *"In Santa Maria and Jacobson (2009), net KE [kinetic energy] 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 in KE is correct." from Jacobson & Archer (2010a).*

Jacobson & Archer (2010a) later state:

> *"In sum, enhanced KE [kinetic energy] dissipation [from wind turbines] ultimately must cause enhanced KE generation at an equal rate. MGK10 [Miller, Gans, Kleidon, 2010] did not take into account this regeneration mechanism." from Jacobson & Archer (2010a).*

This immediately brings to mind a future Earth covered with wind turbines, not only near the surface but also variously extending through all the higher-altitude atmospheric layers, all extracting momentum but not influencing one another because each is outside the turbine wake of any other. This regeneration mechanism that Miller, Gans, and Kleidon, (2010) do neglect somehow suggests that this is possible. According to Bergmann (2010), Jacobson & Archer's (2010a) suggested regeneration mechanism that has no influence outside the turbine wake,

> *"would represent a perpetuummobile [perpetual motion machine] of the second type, which is impossible due to the second law of thermodynamics," from Bergmann (2010).*

This is not a claim to be noted without substantial confidence, but we agree it is fully warranted here, as was previously noted in a related paper's comment response (Kleidon, 2010a). Jacobson & Archer (2010c) later clarify their reasoning to Bergmann (2010) as:

> *"Energy loss occurs in the wake, but not outside the wake. Observations indicate that wind speeds some distance past turbines are similar to those in front of the turbines. That distance was defined as the wake distance. If this were not the case, wind should be reduced infinitely downwind, all the way around the world back to the original turbine, which clearly does not occur" from Jacobson & Archer (2010c).*

As previously noted, it is unclear what Earth system processes Jacobson & Archer assume will continually increase to account for the increased wind power extraction rate — Miller, Gans, & Kleidon are not aware of any such mechanism or process. Increased turbulent mixing from wind turbines and wind power extraction from wind turbines are not forms of energy regeneration and at the global scale, these processes must decrease the previously maximized generation rate of kinetic wind energy in the atmosphere. Additionally, the energy transformation from kinetic wind energy to ultimately heat emission from electricity use will never result in the initial amount of kinetic wind energy because of unavoidable thermodynamic losses. Viewed at the global scale, kinetic energy removal from the atmosphere will result in a reduced generation rate of atmospheric kinetic energy and therefore less wind power available for future continual extraction.

3.c. Major comment - variability of global wind dissipation estimates

As one example of the variability in dissipation estimates, Jacobson & Archer (2010a) refer to Sorenson (2004):

> *"Sorenson (2004, p. 86) state that direct estimates of dissipation are 4-10 W/m^2 (2000-5100 TW), much higher than 900 TW..."*

Using the exact citation and page number referenced by Jacobson & Archer (2010a), it should be clarified what Sorenson (2004) actually stated on p. 86:

> *"On an annual and global average basis, the creation of kinetic energy in the form of large-scale motion amounts to 2.3 W/m^2 or 0.7% of the solar radiation at the top of the atmosphere. For consistency, the frictional losses must be of equal magnitude, which is not quite consistent with direct estimates (4-10 W/m^2). Newell et al. [1969] argue that the value of about 2.3 W/m^2 given in Fig. 2.50 is most likely to be correct." from Sorenson (2004) p. 86.*

Using the Sorenson (2004) value of 2.3 W/m^2 and a global surface area of $5.1{\cdot}10^{14}\ m^2 \approx 1173$ TW of total atmospheric dissipation. We can also approximate total atmospheric dissipation from Sorenson (2004) as: 175,000 TW $\cdot$ 0.7% $\approx$ 1,225 TW, where 175,000 TW is the incoming solar radiation (Kleidon, 2010b). These values also correspond very well to our simulations at T42 spectral resolution and 10 vertical levels control simulation (1,094 TW).

Jacobson & Archer (2010a) do note that dissipation values:

> *"...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."*

Yes! As the dissipation of kinetic energy in the total atmosphere would be altered by attempting to measure it, it simply makes scientific sense to approximate it to the sources of energy input, as indicated by the Sorenson (2004) quote above. Note that in the 3^{rd} process hierarchy estimate used in this response (suggesting the altered dynamics of how 1,700 TW of wind-derived electricity might be available for extraction based on Jacobson & Delucchi (2010)), to achieve ≈ 5,000 TW of wind dissipation in the total atmosphere, 2,000,000 TW of incoming solar forcing would be necessary (11-times current).

Jacobson & Archer (2010a) also state that the:

> *"...assumption of 900 TW of total wind power generation in the global atmosphere, referenced back to 1955 [Lorenz, 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." [thereby suggesting it no longer applies?]*

Model complexity will not refine the estimate of 900 TW. Their Sorenson (2004) p.86 reference actually uses Oort, (1964); Lorenz, (1967), and Newell et al. (1969) as scientific validation for the energy conversion efficiences for the ≈ 1200 TW of global total atmospheric dissipation.

Model complexity can add insight into the processes and more specific geographic locations of kinetic energy generation and dissipation, but the solar radiative forcing has not significantly changed since 1955 and the theory related to maximum conversion efficiencies continues! If dissipation rates could be measured, we are certain that there would also be some seasonal and annual variability in the dissipation rates (see Fig. 3). 900 TW is a well-founded and well-referenced number (Lorenz, 1955; Lorenz, 1960; Kleidon, 2010a; Kleidon, 2010b) but it is theoretical, not derived from direct atmospheric measurements. Our use of the 900 TW total atmospheric dissipation rate in the process hierarchy estimate and our conclusions that the control T21 simulation output (838 TW) and control T42 simulation output (1094 TW) as shown in Fig. 2 are all close enough to be useful in estimating maximum land-based wind power extractability is scientifically valid.

3.d. Major comment - climate consequences

Jacobson & Archer (2010a) have some severe concerns related to our climatic impact quantifications as they state:

> *"...the climate consequences stated by the authors [Miller, Gans, Kleidon] are overestimated by a factor of at least 50-100," from Jacobson & Archer (2010a).*

Jacobson & Archer (2010a) later clarify this as:

> *"...the factor of 50-100 difference in forcing is almost certainly*

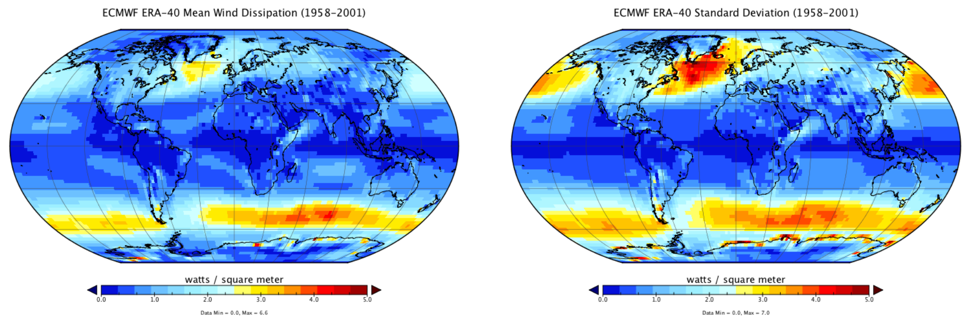

Figure 4: The mean (left) and one standard deviation (right) plots using 64,284 timesteps from 1958-2001 ERA-40 Reanalysis data of 10-meter u- and v-wind velocities and u- and v-surface stress (assumed here equivalent to boundary layer dissipation) are shown. The area-weighted mean global boundary layer dissipation is 513 TW, with a 19.8 TW standard deviation. Note that such mean values as the dissipation in the North Atlantic and North Pacific are actually the result of wind dissipation deviations that are reflected in the mean value. In contrast, the band of high dissipation in the Southern Ocean shown as high mean and high standard deviation values are more attributed to seasonal variations and near-continuous storm activity.

> *because they represent wind turbine effects in their model unphysically, with a grid-cell averaged change in a friction coefficient, rather than resolving the physics and treating the physics of flow around individual turbines, which are subgrid phenomena...," from Jacobson & Archer (2010a).*

Even later, (Jacobson & Archer, 2010c) go on to say:

> *"The question is whether the resulting changes (e.g. in temperature) modeled are realistic and of the correct magnitude and sign, not whether they occur." from Jacobson & Archer (2010c).*

Incoming solar forcing does not change in the simulations. We do not add or remove the heat contribution of fossil fuel power plants. We do not release the wind-derived electricity as heat. The only imposed alteration to the simulation is to remove the kinetic wind energy from the atmosphere at the modeled timestep based on a prescribed additional drag coefficient. In our opinion, the mean and absolute differences in 2-meter air temperature, heat flux, precipitation, and surface radiation are directly attributed to the presence of the wind turbines themselves.

It is our view that the presence of tall, geographically extensive wind turbines will increase turbulent processes in the boundary layer, entraining higher-altitude kinetic wind energy. This higher-altitude air typically has a higher potential temperature than the air temperature of the pre-existing atmospheric boundary layer (see Fig. 5). The unavoidable additional turbulence from the wind turbines increases the vertical extent of the atmospheric boundary layer (*e.g.* wind-turbine array boundary layer or WTABL as denoted by Calaf et al. (2010)) thereby resulting in a temperature increase.

Unless large-scale wind power extraction could result in dynamics that would increase the solar radiative gradient, any energy extraction or additional turbulent mixing within the system will therefore decrease overall atmospheric mixing, decrease heat and moisture transport, and decrease the kinetic energy generation rate. We discussed this in the original text as:

> *"this [area-weighted mean climatic impact on land] is to be expected since the primary cause for the expected climatic changes from wind power extraction (the decrease in atmospheric mixing and transport) are much less directly linked to surface temperature change than direct changes in radiative forcing due to elevated CO_2 concentrations," (Miller et al., 2010).*

As we clearly are expecting a climatic impact due to decreasing energy dissipation rates (decreased generation rate) that accompany the increased rate of wind power extraction, we then extend our analysis to include the area-weighted mean of the absolute value differences as: $\sum_n^1 |x_{simulation} - x_{control}|$, where x is the climatic variable under consideration. Using this climatic metric, we do find similarities between the elevated CO_2 simulation (720 *ppm*) and maximum wind power extraction simulations for precipitation and surface thermal radiation but not 2-meter air temperature or heat flux (Fig. 6, Miller et al, 2010).

We agree with the comment by Kirk-Davidoff (2010) regarding the questions of kinetic wind energy extraction on climatic consequences by Jacobson & Archer, (2010a) when he stated:

> *"...climate impacts are caused, not by mean changes to the global energy budget but by redistribution of heat and moisture from one place to another, caused by wind pattern changes in response to*

> *the wind turbines.*

Our estimated climatic consequences are not overestimated by a factor of 50-100 as suggested (Jacobson & Archer (2010a)) but are a direct response of Earth system processes to very large-scale wind power extraction.

3.e. Major comment - chaotic effects result in our climate impacts

In Jacobson & Archer, (2010a), they suggest that the climatic consequences we attribute to wind power extraction from the boundary layer are actually occurring "when a model assumes numerous perturbations in locations where none should occur, the chaotic variations multiply." It can be assumed that their reasoning is based on their interpretation of wind power extraction as, "Energy loss occurs in the [wind turbine] wake, , but not outside the wake...." (from Jacobson & Archer, 2010b). Kirk-Davidoff (2010) responded to this point directly:

> *"The climate impacts discussed in Keith et al. (2004), Kirk-Davidoff & Keith (2008), Wang and Prinn (2010), and in Miller et al. (2010) have nothing to do with chaos, but result rather, from linear and predictable stationary wave responses to the momentum lost at the model surface." from Kirk-Davidoff (2010).*

In response to Kirk-Davidoff (2010), Jacobson & Archer (2010b) later clarify a "...misunderstanding of our comment," as

> *"The question is whether the resulting changes (e.g. in temperature) modeled are realistic and of the correct magnitude and sign, not whether they occur."*

It is now unclear if Jacobson & Archer (2010b) associate the climatic impacts we relate to very large-scale wind power extraction as chaotic perturbations within our model or to our specific model parameterization.

To at least clarify our understanding of expected changes in temperature (sign not magnitude) related to very large-scale wind power extraction, we will provide a simple example. For wind farms with a length exceeding the height of the atmospheric boundary layer by an order of magnitude, it is expected that a significant proportion of the momentum extracted from the most downwind turbines is being extracted from higher altitude winds (Calaf et al., 2010). The increased turbulence introduced by wind power extraction by wind turbines also increases the vertical extent of the atmospheric boundary layer (Calaf et al., 2010). According to Wallace & Hobbs (2006), the potential temperature of the air above the cap inversion is typically higher than well-mixed atmospheric boundary layer (Fig. 5).

Thus, for very large-scale wind farms, entrained warmer air from higher altitudes is mixed into the colder air near the surface, resulting in a net increase in temperature. No heat was added to the atmosphere to result in this temperature increase — it is simply the result of a redistribution of heat and moisture. We agree with Kirk-Davidoff (2010), chaos has nothing to do with the climatic impacts we attribute to wind power extraction.

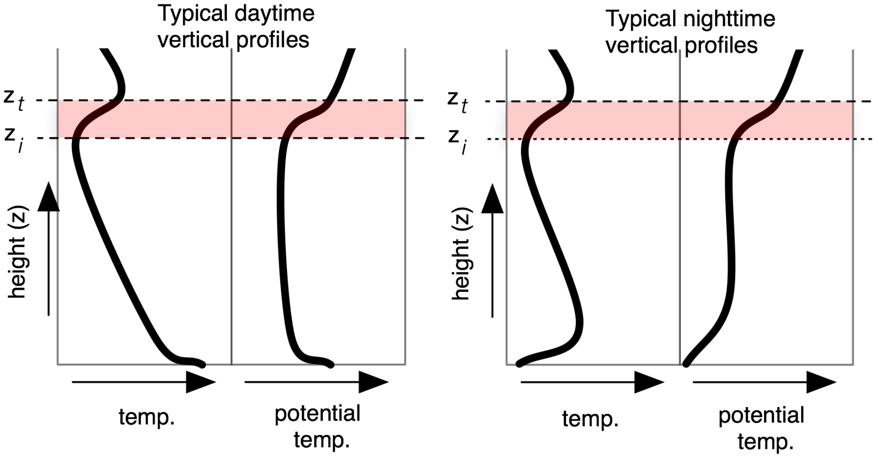

Figure 5: Adapted from Fig. 9.16, p. 392 of Wallace & Hobbs (2006), very large-scale wind turbines will increase the altitude of the boundary layer from Z_i to Z_t. The entrainment of this higher potential temperature air (shown in red) will increase the net temperature of the well-mixed atmospheric boundary layer.

3.e. Major comment - cannot model wind power as a drag coefficient

To note the changes from the original manuscript based on the comment by Bergmann (2010), boundary layer dissipation is now parameterized by the commonly used surface drag parameterization in the atmospheric boundary layer (l) in the form:

$$F_{drag} = \rho(C_n|v_l| + C_{ext}|v_l|) \cdot \vec{v_l} \tag{1}$$

where ρ is the air density, C_n is the volumetric drag coefficient for natural turbulence (which depends on surface roughness and atmospheric stability among other factors) and v_l is the wind velocity, and C_{ext} is the effect of an additional drag coefficient to simulate momentum extraction by wind turbines.

Jacobson & Archer (2010a) state that the method of parameterizing wind power extraction used in this study (Miller et al. (2010) 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" from Jacobson & Archer (2010a).*

Neglecting the fact that Jacobson & Archer have historically calculated large-scale extractable wind power from wind velocity, air density, and specified turbine spacing (Jacobson & Masters, 2001; Archer & Jacobson, 2003; Archer & Jacobson, 2005; Archer & Jacobson, 2007; Santa Maria & Jacobson, 2009; Jacobson & Delucchi, 2010), they are still unclear about the alternative method they are proposing, noted as:

> *"The proper method of simulating the effects of wind turbines is by resolving the turbines and the flow around them" from Jacobson & Archer (2010a).*

Jacobson & Archer (2010b) agree with an associated comment by Kirk-Davidoff (2010) that the higher resolution more complex wind power simulations by Calaf et al. (2010) correctly calculate the loss of momentum by increasing the roughness length, noted by Jacobson & Archer (2010b) as simulating:

> *"wind farms at the resolution of wind turbines themselves and show no application of roughness to the global scale or even the regional scale."*

Bergmann (2010) made his suggested refinements to modeling the wind power drag coefficient very clear for this study (changing $F_{drag} = \rho(C_n|v_l| + C_{ext}) \cdot \vec{v_l}$ to $F_{drag} = \rho(C_n|v_l| + C_{ext}|v_l|) \cdot \vec{v_l}$) which was an oversight of the authors, but the quest for 'wind farm resolution' does not provide us with a physics-based reason for our questioned estimates.

Scale within the model framework is relative to the intention of the model — correlation with an Earth-based measurement scale is only theoretical and must be based on mathematical approximations. As stated by Calaf et al. (2010) on p. 015110-2, the main goal of their large eddy simulation of a fully developed wind-turbine array boundary layer is:

> *"...to determine effective wind-farm roughness parameters and to examine the structure of exchanges in the region close to the wind turbines..."*

This is very different from the main intention of our manuscript — we are trying to estimate maximum global land surface wind power extractability and associated climatic consequences.

Each of these respective studies could be altered to represent larger or small Earth-based spatial areas by simply altering coefficients and parameterizations of the models but achieving the same model behavior. This reinforces our continual focus on the importance of the generation rate of kinetic wind energy as the ultimate limit to wind power extractability — ultimately constrained by the solar input rate, this alters the model to an Earth-based approximation. Additionally, our model simulations do show a similar behavior to Calaf et al. (2010) with higher-altitude momentum contribution, deemed acceptable by Jacobson & Archer (2010c) and illustrated in Fig. 6.

We are also encouraged by the similarity of our drag parameterization methods for modeling very large-scale wind power with other scientifically defensible studies by Keith et al. (2004), Kirk-Davidoff & Keith (2008), and Wang & Prinn (2010) although these are also openly criticized by Jacobson & Archer (2010a) A large eddy simulation study at a resolution similar to Calaf et al. (2010) but applied to the global scale would certainly be interesting but also difficult to implement, because of the necessary computation intensity. Jacobson & Archer would certainly prefer this, but we would remind them that any model with this intention would need to operate under the constraints of the generation rate of kinetic wind energy, just like a coarser resolution general circulation model. This rate is renewable but finite, making quantities like the 1,700 TW of global extractable wind power at 100-meters (Jacobson & Delucchi, 2010) impossible at any resolution, with any physics-based drag parameterization, or with any complexity.

3.f. Major comment - dependence of results on resolution

Absolutely — but only to a point! We would even extend this statement to suggest that our resulting estimates will be different with a different general circulation model. We are thankful for the opportunity to provide a suite of estimates, as our original intention of this paper is to clearly state the critical importance of the generation rate of kinetic wind energy to wind power extractability. In the 4 general circulation model configurations (T21

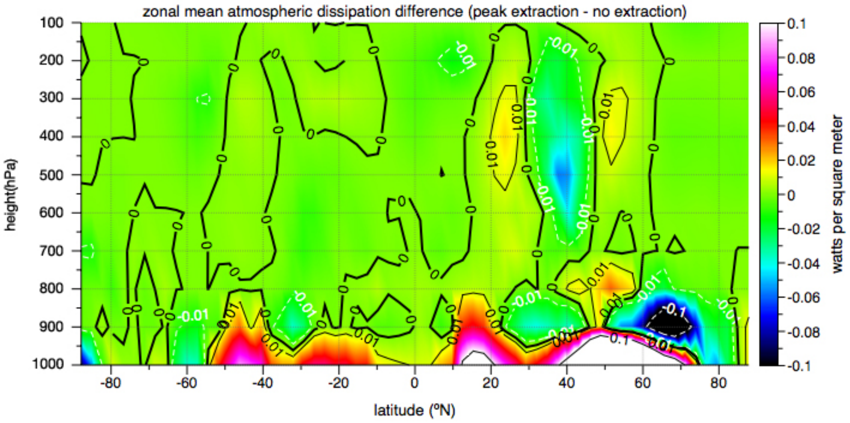

Figure 6: For a 20-year mean simulation with T42 spectral resolution and 20 vertical levels, the difference in wind dissipation between the maximum wind power extraction ($C_{ext} = 0.01$) and no extraction ($C_{ext} = 0.00$) is especially apparent in the northern hemisphere. This reinforces previous research by Bergmann (2010) and Calaf et al (2010) which suggests that wind turbines in the atmospheric boundary layer will result in changes to the adjacent free atmosphere. Our simulations show this effect as increased dissipation within the region of wind power extraction in the boundary layer and a decrease in dissipation at higher altitudes adjacent to these regions.

spectral resolution and 10 vertical layers, T21 with 20 vertical layers, T42 with 10 vertical layers, and T42 with 20 vertical layers) we intend to include in the final manuscript, all 4 configurations result in slightly different maximum wind power extraction rates over all non-glaciated land (T21,10 = 29.2 TW; T21,20 = 29.3 TW; T42,10 = 56.9; T42,20 = 56.9).

Yes, we found that with increased resolution, the maximum wind power extraction rate did increase. This is where theory helps to educate stand-alone model calculations and provide additional scientific validity to our estimtes. As the process hierarchy states, there are numerous processes that transfer energy through the system, for this application bounding atmospheric kinetic energy generation by the incoming solar radiation rate and its conversion efficiencies (Lorenz,1960). This provides an estimated upper-bound of $\approx$ 900 TW of kinetic wind energy in the current global atmospheric system and an idealized but unrealizable maximum over-land extraction limit of 112

TW. The supporting simple momentum balance model is set up to explore the associated feedbacks to the generation rate that cannot be included in the process hierarchy perspective and results in an maximum wind power extraction estimate of 34 TW.

All of this information then provides a feedback of understanding on a different general circulation model estimate — should extractable wind power exceed the wind power generation rate (theoretical $\approx$ 900 TW by Kleidon (2010b), this model's various configurations $\approx$ 840-1100 TW), the estimation method to achieve this result needs to be re-evaluated. Furthermore, as previously discussed, there are thermodynamic constraints on the various energy transformation processes and extraction limits that restrict this estimated quantity to something substantially less (extracted wind power $\cdot$ 60% for electrical power estimation: process hierarchy $\approx$ 68 TW with stated incorrect assumptions; simple momentum balance model $\approx$ 21 TW, T21 simulations $\approx$ 18 TW, T42 simulations $\approx$ 34 TW). Thus, our estimates are consistent with Earth processes as are some previous estimates (Keith et al. 2004; Kirk-Davidoff & Keith, 2008; Wang & Prinn, 2010) but disagree strongly with the 1,700 TW of extractable wind power noted by Jacobson & Archer (2010a). It is not about the resolution of the model but the incorporation of the necessary processes and associated feedbacks.

4i. Minor comment - wind dissipation as a wind power proxy

Jacobson & Archer (2010a) correctly state that "the dissipation rate is not a proper proxy for wind energy potential." Dissipation is affected by wind stress and wind velocity, so altering surface roughness (*e.g.* 100-meter wind turbines) would change the global dissipation patterns. We instead use wind dissipation as a proxy for wind power, as it bounds our process-based hierarchy and simple momentum balance model estimates within realistic quantities of continual energy availability. Using atmospheric dissipation as an upper-bound also alleviates the problems associated with simply extrapolating a wind velocity (v) to turbine hub height, estimating the maximum density of turbines based on their wake volumes, and then applying the bulk wind power equation ($\frac{1}{2}\rho v^3$ where ρ is air density) to determine thermodynamically impossible, and thereby unattainable wind power estimates. We fully recognize that after the installation of one wind turbine, global dissipation patterns and rates will begin to change. As noted by Bergmann (2010) in direct response to Jacobson & Archer (2010a) though, it does have a basis "simply for energy conservation reasons" and we agree.

4ii. Minor comment - waste heat contribution from traditional power plants

No — we do not take into account the waste heat contribution from traditional power plants. Our intention of this study was not to analyze the climatic consequences of the waste heat contribution of human energy utilization or a direct comparison to the climatic impacts of kinetic wind energy extraction of the same magnitude. We only compare our control simulations with progressively increased kinetic wind energy extraction to assess the associated climatic consequences and changes to the global wind generation rate (Fig. 6 of Miller et al. (2010)).

5. Author Overview

To estimate maximum global land-based wind power extraction potential, the thermodynamic efficiencies of kinetic energy generation within the atmosphere must first be acknowledged. This generation rate is the critical component to any estimate of very large-scale wind power estimate. We have been highly critical of nearly all comments by Jacobson & Archer (2010a, 2010b, 2010c) because their engineering approach is in direct conflict to the 1^{st} and 2^{nd} law of thermodynamics. Their statements such as "Energy loss occurs in the [wind turbine] wake, but not outside the wake," (from Jacobson & Archer, (2010b)) and "whereas in the real atmosphere in the presence of wind turbines, F_{acc} [generation rate of kinetic wind energy] would increase by the rate of momentum extraction by wind turbines," (from Jacobson & Archer (2010a)) are in direct contradiction to our understanding of the Earth system. We appreciate the efforts of J.C. Bergmann (2010) and D. Kirk-Davidoff (2010), who both attempted to correct confusion regarding Jacobson & Archer (2010a) with comment responses, but Jacobson & Archer (2010b) and Jacobson & Archer (2010c) never acknowledge their lack of a physics-based understanding regarding wind power.

Previous studies have confirmed that the generation rate of kinetic wind energy in the Earth's atmosphere is already maximized (Lorenz, 1960; Kleidon, 2003; Kleidon, 2006, Kleidon, 2010b) and this study found a similar atmospheric response with our simulations of surface-based wind power extraction. We also confirmed previous smaller-scale work suggesting much less than the generation rate of kinetic energy into a system can actually be extracted (Lanchester, (1915); Betz, (1920), Garrett & Cummins (2007)) and must be associated with climatic consequences (Keith et al., (2004), Kirk-Davidoff & Keith, (2004), Barrie & Kirk-Davidoff (2009), Wang & Prinn (2010)). Our revised range of general circulation model simulations with multiple horizontal and vertical resolutions (in part suggested by Archer & Jacobson, (2010a)) will increase the scientific validity of our estimates while clearly identifying their variations. These variations can be improved by accounting for a more detailed interaction scheme of wind turbines with the surrounding atmosphere and more model complexity based on a more clear understanding of Earth system processes. Still, the fundamental limits of thermodynamics must not be contradicted — ignoring them will result in exaggerated estimates of potential wind power extractability at any scale.

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