Response to Reply of Miller and Keith

By Mark Z. Jacobson
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Previously, I commented on Miller and Keith's papers, "Observation-based solar and wind power capacity factors and power densities" (*ERL*, 2018) and "Climatic Impacts of Windpower" (*Joule*, 2018) at these two links, respectively:

https://web.stanford.edu/group/efmh/jacobson/Articles/I/CombiningRenew/18-RespERL-MK.pdf

https://web.stanford.edu/group/efmh/jacobson/Articles/I/CombiningRenew/18-RespMK.pdf

They responded to my comments. Below are my replies to their responses (their responses are in bold, my replies are in plain text).

In sum, both papers give fictitious and/or misleading results.

The ERL paper underestimates installed and output wind power densities by up to a factor of ~19 and utility PV power densities by a factor of around 3.2-4.1 compared with realistic numbers. Clear errors in their methodology and errors in the use of other authors' results are identified.

The Joule paper conclusion that wind turbines will warm U.S. climate is similarly misleading and flawed, as it fails to account fully for the impacts of changes in water vapor on regional and global climate.

ERL Paper

1. Comment. First *ERL*. Figure 1 of MZJ's critique does not correctly represent our method. As described on pages 2-4 of the our *ERL* paper, our method calculates a Voroni polygon for each of the wind turbines in the USGS data base, aggregates these Voroni polygons for each wind farm, selects the median Voroni polygon area from each wind farm's aggregation (in order to ignore the large polygons at the margins), and then multiplies this median area by the number of wind turbines comprising each wind farm.
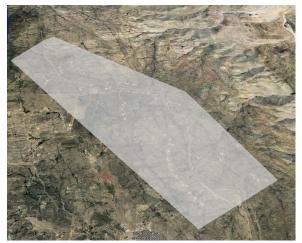
Reply. Figure 1 (repeated below) of my original critique unambiguously shows why the result and technique of Miller and Keith is 100% fallacious. The average output power density from Miller and Keith (ERL) Table 1 for 2016 installations is 0.5 We/m^2 and the average capacity factor is 33% from the same table. This gives an average installed power density of 0.5 / 0.33 = 1.52 MW/km^2. They also admit in their paper that the "average installed capacity density of all wind farms was 1.5 MW/km^2."

Figure 1 here shows an unrealistically large spacing envelope around the Tule wind farm east of San Diego, California, which consists of 57 GE 2.3 MW turbines for 131.1 MW total. The envelope represents an area of 46 km^2, giving an installed power density of 131.1 / 46 = 2.85 MW/km^2.

Given that Miller and Keith's U.S. average installed power density is even lower, the envelope of their area if calculated with their average values would be even larger, 46 x 2.85 / 1.52 = 86.3 km^2, or 1.51 km^2/turbine. Miller and Keith acknowledge in their caption to their own Figure 1 that, in the example wind farm illustrated in their figure, that their Voroni polygon method gave 1.0 km^2 per turbine for that specific farm.

Any reasonable observation of Figure 1 here, however, indicates that the envelope shown around the Tule wind farm is incredibly and unrealistically large, and Miller and Keith's envelope is even 88% larger than that.

Figure 1. Envelope around the Tule wind farm east of San Diego, California. The farm consists of 57 GE 2.3 MW turbines. The envelope, which is unrealistically large for a spacing area, gives an installed power density of 2.85 MW/km^2. Miller and Keith's average for all farms is 1.52 MW/km^2, thus their envelope would be 88% larger. An actual analysis of the area around turbines with the method in Enevoldsen and Jacobson (2018) gives an installed power density of 29.0 MW/km^2, or ~19 times that of Miller and Keith.

In fact, an analysis of the actual spacing area of a wind farm that takes into account the envelope of the farm, excluding areas outside the boundary and between clusters of turbines as in Enevoldsen and Jacobson (2018) gives an installed power density of 29.0 MW/km^2 and an output power density of 8.7 We/m^2 (assuming a capacity factor of 30%), which are 19.1 and 17.4 times larger, respectively, than those based on Miller and Keith's technique.

Clearly, the area beyond the edge of a wind farm is not land taken up by the wind farm, yet in the case of 1 km^2/turbine, Miller and Keith assign at least 0.4 km^2 per turbine outside of a farm to the farm, which is an error. Further, their median polygon is calculated ignoring the fact that many wind farms contain clusters separated by distance and the space between the clusters is not land taken up by the wind farm. They include the distance between clusters when calculating their polygon areas,

giving unreasonably large polygon sizes, although such distances can be large and are open space. Regardless, as illustrated by Figure 1, their technique results in fictitious installed and output power densities of wind farms.

2. Comment. MZJ does not identify a specific error in our method, but rather references a paper under review that apparently gets different results using a different method. As things stand now, our paper represents the only peer-reviewed calculation of annual average power density over a large population of US wind farms.

Reply. Please see Response 1 above for two specific errors and to illustrate on its face the erroneous result of the Miller and Keith method.

3. Comment. Any choice of method is somewhat arbitrary for small wind farms so some differences are to be expected.

Reply. A factor of up to ~19 difference is not "some difference." It is a major flaw in the method of Miller and Keith.

4. Comment. For solar PV, we did not perform our own new calculation of the capacity density of solar PV, but relied on Appendix B data collected by Ong et al. (2013) and derived a statistical fit from these 192 solar PV plants (Fig. 2A). To achieve MZJ's higher power density, the observed utility-scale installed capacity density would need to be about 4-times higher than the 30 MW$_{dc}$ km^{-2} we used.

Reply. The data from Ong et al. (2013) show the misleading and outdated nature of the numerical results of Miller and Keith, in which they assume that the installed power density of utility PV is 30 MWdc/km^2 and calculate the output power density as 5.4 We/m^2. Below is a list of the problems with their calculation:

A) Miller and Keith intentionally use the "total area" of a PV Plant from Ong et al., which is "all land enclosed by the site boundary" rather than the "direct area," which comprises the land "directly occupied by solar arrays, access roads, substations, service buildings, and other infrastructure." Thus, from the get-go, they show their intention to count unused land within a site boundary as "land area occupied by an energy system," even though the literature source they use for these data clearly defines the land "directly occupied" by an energy system differently.

B) Ong et al. (2013) clearly conclude, "For direct-area requirements the generation-weighted average is 2.9 acres/GWh/yr," and these are for U.S. solar PV farms installed before August 2012. That output translates to 9.73 We/m^2, a number already 80% higher than Miller and Keith's 2016 claim of 5.4 We/m^2.

C) However, Appendix B of Ong et al. (2013) shows that the installed-power-weighted mean solar panel efficiency of the installed PV panels of the dataset they used was only 9.8%. Yet, commercial panels in 2016 (e.g., Sunpower panels) were already above 20%, with one as of 2015 at 22.8% (http://businessfeed.sunpower.com/articles/understanding-solar-panel-efficiency).

D) With an average panel efficiency in 2016 of even 15%, the output power density of utility PV rises to 14.9 We/km^2. With an average efficiency of 20%, which is more representative of new solar installations, the output power density rises to 19.9 We/km^2. For an average efficiency in the future of 23%, that increases to 22.8 We/km^2.

E) In sum, using the data from Ong et al. (2013) combined with actual and updated commercial panel efficiencies gives output power densities of utility solar of 14.9-22.8 We/km^2, where the low number corresponds more closely to 2016 averages and the high number corresponds to when all solar farms install the most efficient panels as of 2015. These numbers are close to the range of 17.4-22 We/km^2 assumed in Jacobson et al. (2017) and derived in my original reply to Miller and Keith. As such, the output power density of utility solar from Miller and Keith is underestimated by a factor of 3.2-4.1.

F) The assumption of 30 MWdc/km2 and an output power density of 5.4 We/m^2 by Miller and Keith gives a mean capacity factor of 18%, which is lower than any capacity factor they list in their table 1 suggesting an inconsistency somewhere in their calculations as well.

Joule Paper
5. Comment. The most important test of any model is how effective it is at predicting reality. Figure 4 of our *Joule* paper shows that our model does as good a job of predicting the seasonal and diurnal cycle of observed warming from wind power.

Reply. Figure 4 of the Joule paper does NOT compare the model used by Miller and Keith with data showing the same parameter, thus the comparison is not a validation of their theory. The model is showing temperature differences, averaged over some region in Texas, between two simulations in which wind turbines were present and absent, respectively. The data shows the temperature differences, presumably averaged over the same region, due to all physical, dynamical, and radiative processes that affect temperature during the time period, only one of which is wind turbines.

Aside from the fact that the model result consistently shows a temperature perturbation twice as high as the data, the temperature differenced in the data accounts for global warming, other urbanization and land use change in the study

area, changes in weather patterns, and the occurrence of extreme weather events, etc, whereas the differences in the model result do not.

Observed differences in temperature never show what caused the difference in temperature, yet Miller and Keith incorrectly claim in the caption to their Figure 4 that the data are showing "night and day response" to wind turbines. Thus, they mislead readers into thinking the observed temperature differences are due to wind turbines when they have no proof whatsoever of this and have not filtered out other causes of temperature change.

Further, because the magnitude of the change is not replicated, the only conclusion from the figure, even if the comparison were consistent, is that the model overestimates the impact of wind turbines.

6) Comment. MZJ has not, to our knowledge, published any comparison of his model of wind power's climate effects against observations. We look forward to seeing such a comparison in print.

Reply. Despite their inaccurate claim to the contrary, Miller and Keith have never published a comparison of results from the model they use with data showing wind power's climate effects. As stated under Response 5, their model result is compared with a different parameter.

The GATOR-GCMOM model has been evaluated extensively with paired-in-time-and-space and other data for gas, aerosol, radiative, and meteorological parameters in almost two-dozen studies. Here are just two.

https://web.stanford.edu/group/efmh/jacobson/Articles/V/GATORGCMM201.pdf

https://web.stanford.edu/group/efmh/jacobson/Articles/III/2007JD008922.pdf

7. Comment. The *Joule* paper uses a regional model. As we say in the paper, this has the *advantage* that it is much higher-resolution (10 km vs. the roughly 400 km used for most simulations in MZJ Joule Critique ref #1) and the *disadvantage* that it can't capture global responses. Our paper says nothing about global warming one way or the other. We make no such claims in the paper.

Reply. Miller and Keith's comment is misleading and false. The title of their paper is "Climatic impacts of wind power," not "Regional climate impacts of wind power" and the abstract states, that generating US electricity demand with wind "would warm Continental US surface temperatures by 0.24 C" which implicitly means that that warming accounts for global impacts even though it does not.

Further, the authors failed to report results from two previous studies that found that large scale wind turbines cause global cooling, not warming (Jacobson and Archer, 2012; Jacobson et al., 2018).

8) Comment. Previous work by many different investigators including Keith et al. (2004) have used global models to explore the climate response to wind power[i]. They have not generally found strong global average warming or cooling. All models have found regional warming and cooling. Regional warming, cooling, or other climate changes matter. Climate impacts are, in part, the sum over local impacts, which often depend on magnitude of the deviation from preindustrial. It would be ridiculous to claim that just because global average temperatures are unchanged, there is no impact.

Reply. This response by Miller and Keith is intentionally misleading because they are clearly aware that my original comment showed (including three plots) and referenced results from two published papers (Jacobson and Archer, 2012; Jacobson et al., 2018) that concluded that wind turbines caused global cooling. In fact, the figures shown in my original comment showed average global cooling of 0.03, 0.06, and 0.17 K in the three plots. Yet, Miller and Keith pretend simply that "all models have found regional warming and cooling," refusing to acknowledge the conclusion from the papers that turbines cause net global cooling.

9) Comment. Far from disproving our point, MZJs' work in Ref #1 is in fact consistent with the broad findings of Keith *et al.* (2004; Fig. 1, 2, 4) which found roughly similar magnitudes. Results which, at the time, MZJ dismissed much as he is doing here.

Reply. This statement is misleading. Keith et al. (2004) presented results where the noise was greater than the signal so it was impossible to determine the climate effects of wind turbines. Further, that study did not represent wind turbines correctly. On the other hand, the global cooling found in Jacobson and Archer (2012) and Jacobson et al. (2018) is statistically significant, based on a realistic wind turbine parameterization, consistent across numerous simulations, and explainable physically. While the magnitudes of some of the changes may be similar due to chaotic variation in both models, the signal is absent in Keith et al. (2004) and present in Jacobson and Archer (2012) and Jacobson et al. (2018).

10) Comment. MZJ writes: "Based on the fact that Miller and Keith don't discuss radiative transfer calculations in their model, it is also not clear that they even account for the impacts of perturbations to water vapor on infrared radiative transfer in the regional portion of the domain that occur before such perturbations disappear to the larger domain." This is a highly misleading claim. It's true that we don't discuss radiative transfer calculations in our paper, and that's because we used WRF, an open community model with public access to

code and documentation. WRF does, of course, do infrared radiative transfer within the model domain. MZJ could easily have checked this in the [WRF documentation](). Moreover, interactive radiative transfer is standard in such models, so MZJ might well have guessed this answer in advance. If he did, then his statement was deliberately deceptive.

Reply. This is a deceptive comment by Miller and Keith. The documentation link that they claim I easily could "have checked this" contains zero information on whether water vapor is treated interactively in their calculations as opposed to assumed to have a constant profile. Regardless, because they lose more than 99% of the water vapor perturbations due to wind turbines and the subsequent energy transfers that such perturbations result in (latent heat release) when the water vapor perturbations leave the model domain boundaries, they capture very little of the effect of water vapor changes even if their model does treat interactive absorption by water vapor.

In sum, they capture the warming due to reducing water evaporation in the soil but don't capture the cooling due to reducing latent heat release during cloud condensation or the cooling due to the reduction in greenhouse gas absorption by water vapor outside the model domain. As such, no wonder they see a net warming due to wind turbines.

11) Comment. MZJ's work uses GATOR, a private model without deep public documentation,…

Reply. This smear indicates the inexperience of Miller and Keith in the area of model development. They appear to be primarily black-box users of models and have never developed a model from the ground up or researched the literature of model development.

I developed GATOR-GCMOM from the ground up, writing around 90% of the code for it since 1990. Much of the history of GATOR-GCMOM model development is summarized here

https://web.stanford.edu/group/efmh/jacobson/GATOR/GATOR-GCMOMHist.pdf

The entire model has been documented in over 60 public peer-reviewed papers that describe in detail the algorithms, overall model results, and evaluations against data, as well as a public textbook that has gone through two editions and a reprint (Fundamentals of Atmospheric Modeling)

https://web.stanford.edu/group/efmh/jacobson/FAMbook2dEd/index.html

that describes many algorithms in even more detail. In addition over 1000 researchers have obtained algorithms from the model and several PhD students have used the model for their research.

Further, the model has taken part in 11 multi-model intercomparisons. In an independent review of comprehensive models, Zhang (2008, https://www.atmos-chem-phys.net/8/2895/2008/acp-8-2895-2008.pdf) states,

> "Similar to GATOR-MMTD on urban/regional scales, this (GATORG) is the first fully-coupled global online model in the history that accounts for all major feedbacks among major atmospheric processes based on first principles."

and

> "Jacobson (2001b, c) linked the regional GATORM and global GATORG and developed the first in the history unified, nested global-through-urban scale Gas, Aerosol, Transport, Radiation, General Circulation, and Mesoscale Meteorological model, GATOR-GCMM."

I am unaware of any other model whose algorithms are summarized and available in a textbook on top of over 60-peer-reviewed articles or of a more comprehensive global, regional, or nested-global-regional model than GATOR-GCMOM, as illustrated by Zhang (2008). I welcome an updated one-to-one comparison of algorithms in any model worldwide.

12) Comment. The papers to which MZJs' critique refers do not themselves discuss details of GATOR's radiative transfer.

Reply. The radiative absorption by water vapor in GATOR-GCMOM is treated as described clearly here:

https://web.stanford.edu/group/efmh/jacobson/Articles/IX/JASJacobson05.pdf

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