Author response to A. Speranza "Interactive 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 A. Speranza for his time and effort in reviewing our submitted manuscript. Here we will clarify several points in the manuscript that he noted require additional description and respond to his main concerns on the phenomenological approach we used.

Overall, it is our interpretation from his review that A. Speranza is particularly concerned that our study tries to quantify the exact quantity of maximum wind power extractability over land. Instead, it was and continues to be, our sincere aspiration to provide a series of estimates that are firmly based on the generation rate of kinetic wind energy in the atmosphere based on thermodynamic constraints. We apologize that our submitted manuscript may have been received a different way and will rectify this with a significantly enhanced final version to directly address this confusion.

Phenomenological approaches do not apply

We disagree. We recognize that our process-based hierarchy does drastically simplify the Earth systems that relate to atmospheric wind power. We also recognize that it may initially seem erroneous on our part to not even include wind velocity in this first-order estimate. This process-based hierarchy is certainly not applicable to a small geographic scale (*e.g.* Italy) but at near-global scales, it is the conversion efficiency from incoming solar radiation to kinetic wind energy within the atmospheric boundary layer that provides an upper-bound for potential global wind power extractability. Our intention in using a process-based hierarchy, a simple momentum balance model, and a general circulation model (with 4 different resolutions rather than 1 used in the original submission) is to first illustrate the processes that restrict the maximum extraction rate to less than the kinetic energy generation rate and then provide a suite of estimates based on various assumptions and simplifications.

- Process-based hierarchy - [*"What about potential energy? And thermal energy?" by A. Speranza*] Potential energy and thermal energy do relate to wind power extraction, particularly after the wind farm achieves a spatial size that effectively entrains higher-altitude air, as our extreme scenarios would. By focusing on the climatic steady-state, the role of these different types of energy that are transformed into kinetic wind energy are included in the generation rate. The ≈ 2% conversion efficiency from differential solar heating to total atmospheric kinetic energy over time (≈ 900 TW based on Lorenz, 1960; Kleidon, 2010) is also operating at the maximum rate for present-day radiative forcing (Lorenz, 1960; Paltridge, 1978, Kleidon, 2003, Kleidon, 2006, Kleidon, 2010) so any additional energy extraction will decrease the generation rate based on thermodynamics.

  The process-based hierarchy makes numerous simplifications, such as the inability of dissipation to be redistributed when influenced by a perturbation such as wind turbines, resulting in a general estimate that is not capable of clearly illustrating how the Earth processes will be altered. It does help frame the question as to what processes are involved and provides scientific substance to our response to such comments 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). The phenomenological approach, with the process-based hierarchy in the form of a back-of-the-envelope estimate, would directly clarify our simplified reasoning and more general response to similar comments such as these before moving on to increased complexity.

- Simple momentum balance model - [*"...there are some unclear assumptions and procedures..." by A. Speranza*] Several input parameters and methods do require a more thorough description. Those noted by A. Speranza include:

  • Our approximation of boundary layer dissipation used in the simple momentum balance model is based on a 10-meter u- and v-wind velocity and u- and v-surface stress from the ECMWF ERA-40 reanalysis dataset at a 2.5° by 2.5° resolution.

  • F_{acc}=1.1918 $\cdot 10^{14}$ N, defined in the original text as "the rate of momentum generation by an acceleration force," is the rate of conversion between incoming solar radiation and kinetic wind energy. We assume this parameter is constant, constrained by thermodynamic limits and currently operating at the maximum rate achievable as discussed by thermodynamic arguments (Paltridge, 1978; Lorenz et al., 2001) as well as climate model simulations (Kleidon et al., 2003, 2006).

  • [*Eq. 10 - other definitions of wind velocity seem to be involved, by A. Speranza*] No. Eq. 10 ($D = \vec{\tau} \cdot \vec{v}$) still utilizes the 10-meter u- and v-wind velocity and u- and v-surface stress from the ECMWF ERA-40 reanalysis dataset used in the simple momentum balance model (Eq. 1-10). It is possible that there was some later confusion with v in Eq. 11-13 which are indeed different but this has been corrected in the final manuscript.

- general circulation model simulations - *"....noting the limitations of numerical models in representing surface wind" by A. Speranza.*

  • [*Eq. 13 - "...again what speed are we talking about? How do we derive it from numerical integrations?", by A. Speranza*] Yes - this point is unclear and requires clarification. The velocity (v_l) in Eq. 11-13 is the wind velocity at the vertical midpoint of each spectral grid cell of the lowest atmospheric level of the general circulation model. We also recognize the previous work by F. Chèruy et al, (2004) recommended by A. Speranza concerning the inclusion of super-dissipations inserted into the parameterizations for numerical stability. Our general circulation model of intermediate complexity (Fraedrich et al., 2004) definitely makes a number of numerical simplifications that would be more well-represented by a complex high-resolution regional climate model. By using the suite of estimation methods shown in the original manuscript, and reinforcing improvements to the final manuscript (process-based hierarchy is now posed as a question rather than an explicit method, T21 and T42 spectral resolution simulations with 10 and 20 vertical levels), we are striving to clarify that different models will result in different estimates for maximum global land-based wind power extraction from the atmospheric boundary layer but they are ultimately bound by the energy conversion efficiencies of Earth's atmospheric system and thermodynamic constraints on power extractability.

- [*"...at such limited resolution, computed surface wind is totally unreliable in view of the envisaged estimation process.", by A. Speranza*] Based on the referee recommended topic paper by F. Chèruy et al, (2004), surface wind velocities in numerical models suffer from power deficits, especially at the smallest grid scales. We agree that numerical models do not adequately represent reality. Given the thermodynamic constraints on the processes related to the conversion of incoming solar radiation to kinetic wind energy, the general agreement of total atmospheric wind dissipation between our various model simulations (T21 with 10 vertical levels = 838 TW, T42 with 10 vertical levels = 1094 TW) and our general understanding ($\approx$ 900 TW based on Lorenz, 1960; Kleidon, 2010) suggests a range of estimates in general agreement with the generating constraints of this energy conversion. We would welcome a numerical enhancement of this study at a higher-resolution but computing the precise wind velocities measured near the Earth's surface would not illustrate scientifically valid success for our given intention.

Author Overview

A. Speranza raises a number of interesting points and comments in his review. There are a number of additional descriptors that need to be included in the final manuscript for clarification and reproducibility. We also believe his more general opinion of "the proposed estimations are, in my opinion, drastically inadequate" are based on a misunderstanding that we are trying to estimate wind speeds to estimate global wind power — this is not directly the case. Our 'top-down' estimate assumes that to estimate the realizable wind power potential on a global scale, the generation rate and induced wind power extraction processes that may alter this rate, are critically important. We encourage the replication of this study with higher-resolution general circulation models but believe the critical processes are included here. To substantiate these conclusions, we have completed additional general circulation model simulations at T21 with 20 vertical levels, T42 with 10 vertical levels, and T42 with 20 vertical levels — all result in a similar estimate range that is also within the range suggested by the process-based hierarchy and simple momentum balance model estimates. In the final manuscript, we will clearly note that all of our estimates include simplifications that do not adequately represent the true complexity of Earth. In support of our conclusions though, we will also make our theoretical viewpoint clear — should future estimates from any simulation significantly exceed our range of estimates or exceed the global generation rate of kinetic energy in the atmosphere($\approx$ 900 TW by Kleidon, 2010), those estimates should be seriously reconsidered.

References

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