

Interactive comment on “Estimating maximum global land surface wind power extractability and associated climatic consequences” by L. M. Miller et al.

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The article makes several statements on energetics of turbine flow and Atmospheric-Boundary-Layer (ABL) flow, which are not well founded, partially directly wrong. On the other hand, it is correctly demonstrated that the radiative processes in the atmosphere define the limits of atmospheric energetics for energy-transformation-rates to wind (and hereby, maximal theoretically possible extraction-rates). However, for practicable large-scale wind-energy utilisation, ABL energetics plays a crucial role. There-

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fore, the present comment attempts some contributions towards bridging the gap.

Turbine Energetics

The article states the maximal efficiency of a turbine array to be only ca. 0.3, in contrast to the theoretical limit of 0.59 of the solitary turbine. This difference is assigned to kinetic-energy dissipation caused by wake-turbulence in case of turbine array. However, reality seems not be so because observations downwind offshore wind farms do not indicate increased flow-variability (Hasager et al. 2007). Therefore, the authors of the article should not emphasise the issue too strongly, before the crucial prove of increased turbulence has been demonstrated. Increased flow-variability found in other observations is not necessarily turbulence (Bergmann 2010, GRA). The article's consideration disregards the practical problem of energy-replenishment at turbine level by assuming a constant power density, maintained by an unspecified, constant "accelerating force" (see also next section). The crucial point is that power-maintenance by this force depends linearly on velocity, whereas turbine power has a cubic dependence on velocity. That does simply not fit! Turbines' power extraction at turbine level is therefore much larger than maintenance by the "accelerating force" that acts on the entire ABL, and the energy-replenishment issue for turbine level arises quite naturally and not negligible. The efficiency of the usually assumed vertical turbulent transport of kinetic energy for energy-replenishment at turbine level, provided the necessary increased turbulence really exists, is only ca. 60% for an equilibrium energy resource at turbine level of 50% of the free flow (Bergmann 2010, esdd). This introduces additional dissipation of order 2/3 of the equilibrium turbine power (50% of the free-flow value). Reduced array efficiency caused by atmospheric-dynamics, which can have larger resource-reduction effects than those processes considered above, does not affect extraction of wind energy as such (as also stated in the article), but it imposes additional practical and economical limits, especially to offshore installations. The area-density of economically feasible wind-power installation is, of course, crucially affected by the complete process of extraction-caused resource reduction.

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The article describes ABL energetics on a quasi-phenomenological level because it treats ABL flow like a ‘normal’ flow, in which all relevant vectors are aligned in flow direction. This is clearly not the case in ABL flow: the pressure-gradient force is almost perpendicular to the flow, with some relevant consequences in regard to the quasi-phenomenology.

The article’s Equation (1) introduces a constant “accelerating force” parallel to the wind velocity without specifying it. The atmospheric equation of motion reveals that this force is mainly caused by down-gradient flow, i.e., by the intensity of motion perpendicular to the geostrophic velocity, whose direction is close to the ABL flow velocity’s. Thus, the dominant x-component of the accelerating force is not a definite external force as assumed in Equation (1), it is a variable rotational effect in dependence on ageostrophic flow: $F_{\text{acc}}/m = f v_y$, here as acceleration with f the Coriolis parameter and v_y the ageostrophic velocity component. As down-gradient (ageostrophic) flow is the energy source of the flow, the ABL flow can react on additional energy- and momentum-extraction with increased down-gradient flow and thus increased “accelerating force”! The phenomenological effect is minimal regarding changes in wind speed. The wind just turns its direction a bit more towards the low pressure. This real existing dynamics of the ABL, relevant to turbines’ power extraction, is inexistent in the article’s concept.

The article’s Equation (3) continues the quasi-phenomenological concept by identifying total power with the scalar product of the parallel vectors F_{acc} and v , with v the unspecified (no reference height) wind velocity vector. This power is partitioned between ‘natural dissipation’ and power-extraction by turbines in Equations (4) . . . (9), neglecting the above-mentioned specific ABL dynamics.

The article’s Equation (10), $D = \tau v$, is directly wrong because v is taken as “near-surface wind velocity” (what ever this may mean), τ is surface shear stress vector. The

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surface-area-density of energy-dissipation in the ABL has been derived precisely by Lettau (1962) as τv_g , with v_g the geostrophic wind velocity (the value in the frictionless region on top of the ABL), and the vectors are not parallel, so that Lettau (1962) states the scalar form as $\tau v_g \cos(\alpha)$ with α the ageostrophic angle of the surface stress. The angle is normally not the same as the angle of the near-surface wind, with relevant consequences to the ABL's energy supply, see Bergmann (2010, GRA) for more details. With introduction of Equation (10), the described quantities are changed without changing the symbols. Up to Equation (9), it is dealt with force, energy and power (dimensions!), and the friction coefficient k is a dimensional quantity, [kg/m] (what with the horizontal axis of Figure 3?). From Equation (10) and on, the article deals with surface-area densities of momentum- and energy-fluxes employing dimensionless coefficients.

The article's Equations (11) and (13) are also wrong because turbine drag is not linear with wind speed, but also quadratic, as is surface drag. Therefore, Equation (13) arrives at a wrong square-dependence of extracted power on velocity, which should have been noticed immediately, latest at checking dimensions! What may be the effects on the model results for different assumptions on power-extraction?

The range of applied extraction constants is shocking: the maximal value is 10.0, whereas the total cross-sectional-area power density of the flow is only $0.5 \rho v^3$. (The same applies to the quantity that has in fact been evaluated, the momentum-extraction by turbines: the total momentum flux density is only $1.0 \rho v^2$.) In comparing the order of magnitude of surface-area power density, Equation (10), to the cross-sectional-area power density, it would have been immediately evident that the maximal possible value of the extraction constant is considerably below 0.1 (here, the exponent-error seems to play a crucial role in Figure 5).

Disappearance of ABL energy dissipation with large extraction-parameter values (Figures 3 and 5) demonstrates that the quasi-phenomenological concept is seriously flawed. It works just qualitatively at low parameter values - as long as total energy-

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extraction ('surface' dissipation plus turbines) does not shrink significantly. Remember: it was an essential and correct statement of the article that turbines compete with 'surface' dissipation for the energy-flux supplied by the global circulation (cf. Figure 1 and Bergmann 2010, GRA).

Climatic Effects

The author cannot comment on the climate-change computations because it is not evident whether, respectively how the climate model incorporates ABL physics.

Conclusion

Energetically relevant wake-produced turbulence (not flow-variability) is an assertion and not yet proved by real-world measurements, which indicate rather the opposite. Therefore, energy-replenishment at turbine level is still an unsolved issue. Atmospheric-dynamics processes of energy-replenishment and of resource reduction are still widely unconsidered, but seem to be most relevant. Basic atmospheric dynamics is not correctly applied in the article, so that the model results are questionable.

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