

The impression I had already had from reading the abstract and some comments was confirmed when I read the paper. The paper is not convincing for the following reasons:

- 1) It is not possible to deduce the total amount of energy that can be extracted from the atmosphere through wind farms alone from a “tunnel view” of the boundary layer. Important is how the boundary layer is coupled to the total kinetic energy of the atmospheric circulation, and how fast, in turn, the extraction of kinetic energy from the total atmospheric circulation through wind farms can be replaced by energy from the sun’s radiation. .
- 2) In other words: the problem can be tackled quantitatively only with an atmospheric general circulation model. It should not be too difficult to modify an AGCM by introducing a higher friction at the surface, or, more precisely, through a higher (tower-like) resistance in the boundary layer which reproduces the desired effect. I wonder why the authors have not referred at all to the work of Wang and Prinn who have apparently already done something like that.
- 3) The errors of other authors (Magdalena & Jacobson, and Archer & Caldeira) in estimating the maximal possible energy abstraction are pointed out. These are due apparently to wrong assumptions of the authors regarding the kinetic energy of the lowest 1 km of the atmosphere and the jet stream, respectively.- accepting the numbers given by Gans et al. This criticism is perhaps correct, but in no way justifies the authors’ own calculations.
- 4) Accepting the numbers given by Ganz et al (I did not check them), namely that the total production of kinetic energy of the atmosphere is about 900 TW while the human use is about 17 TW, i.e. about 2 percent of this amount, the estimate of Wang and Prinn (cf.. anonymous reviewer no 2) appears plausible, namely that human energy extraction would reduce the kinetic energy of the boundary layer by about 10 to 20 percent, if the extraction was applied worldwide to continental boundary layers.

Conclusion: The paper should be judged in relation to the work of Wang and Prinn. In contrast to Wang and Prinn, the authors offer no numbers, only the general statement “significant reduction”. If they can improve on Wang and Prinn’s estimate, fine. But a tunnel model won’t do it.

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