

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

A. Speranza (Referee)

antonio.speranza@unicam.it

Received and published: 25 October 2010

In consideration of the fact that “The availability of wind power for renewable energy extraction is ultimately limited . . .” the authors propose three different methods “. . . to provide a maximum estimate of wind power availability over land.”

The objective of the proposed research is interesting but the proposed estimations are, in my opinion, drastically inadequate.

General Comments

Estimates based on phenomenological approaches are known to hold (at most!) in the

C86

observed regimes and cannot be extrapolated to other regimes. As a consequence it is a priori questionable that methods like 1 (Back-of-the-envelope estimate) and 2 (Simple momentum model with reanalysis wind data) are proposed under circumstances in which the authors themselves claim that relevant changes take place in atmospheric flow regime. Moreover, even in the context of the proposed methods, there are some unclear assumptions and procedures (see specific remarks below).

As to the third method (Climate model simulations, section 2.3), the authors may consider going over F. Chèruy, et al., 2004: *Surface winds in the Euro-Mediterranean area: the real resolution of numerical grids* (Annales Geophysicae, **22**, 4043–4048), or papers of analogous subject, in which a critical discussion concerning the limitations of numerical models in representing surface wind is proposed. In extreme synthesis, the artificial numerical friction (adopted for reasons of computational stability) is such as to over-smooth surface wind to the point that the actual resolution is much less than the geometrical one. I do not have sufficient information about the model adopted in the proposed climate model simulations, but it seems to me clear that, at such limited resolution, computed surface wind is totally unreliable in view of the envisaged estimation process. Here again appear in the paper some unclear (at least to me) assumptions and procedures (see specific remarks below).

Specific remarks

Back-of-the-envelope estimate

What about potential energy? And thermal energy?

Simple momentum model with reanalysis wind data

In equations 1 and following it is unclear to me what velocity v we are dealing with: the surface one or the mean ABL flow speed, or what? Initially “. . . . the rate of momentum generation by an acceleration force, . . .” is mentioned (by the way, what physical process are we talking about?), but later (in equation 10, for example) other definitions of

C87

wind velocity seem to be involved.

Climate model simulations

Equations 11-13: again, what speed are we talking about? How do we derive it from numerical integrations?

I do not dispose of detailed information concerning the adopted numerical model, but I guess direct frictional dissipation is small compared with numerical dissipation operating on surface wind (wind in the lower layer of the model).

Interactive comment on Earth Syst. Dynam. Discuss., 1, 169, 2010.