

Interactive comment on “The problem of the second wind turbine – a note on a common but flawed wind power estimation method” by F. Gans et al.

F. Gans et al.

fgans@bgc-jena.mpg.de

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We thank the anonymous referee for his review, which helped to improve the manuscript in a revised version. Our response to the referees suggestions is as follows:

1) However, the tunnel model does not include the expected enhanced downward transport of momentum and kinetic energy that would help offset, at least in part, the affects of the extracted power. This would constitute an additional term in equation 5. This effect should be discussed in sections 2.1 and 2.2 and some quantitative aspects of it addressed.

This is an argument made quite frequently to justify the use of the common method,
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however we do not think that adding downward transport of kinetic energy would invalidate the conclusions reached within the paper, because this would still assume an infinite reservoir of kinetic energy in higher altitudes. We have added a discussion on this issue in the revised manuscript.

2) The extension of the authors' discussion to much larger scales in section 2.3 is comparatively deficient. At the very large scale of deployment needed to generate, for example, 5TW of electric power globally, Wang and Prinn (2010), using a 3D general circulation climate model, have already shown that the resource (computed as the kinetic energy per unit mass in the boundary layer in the land installation regions) was reduced by 10-20 % due to the installations (see their Figure 4 and associated text). Section 2.3 should include discussion of this work and whether or not the authors' analysis is consistent with it (note that Wang and Prinn appear in the references but are not cited in the text).

We agree that a quantitative assessment of resource consumption by wind turbines can only be done using a GCM and we highly appreciate the work that has been done by Wang and Prinn so far. In the revised manuscript, we added some results of our own GCM simulations to substantiate our argument. However, the subject of this particular study is to demonstrate using basic physics how the common method violates fundamental physical laws. Since work like the one done by ? or ? is permanently criticized by advocates of the common methodology, we think it is necessary to point out the explicit deficiencies of this method which leads to the difference in results.

3) To better clarify which previous wind power studies have neglected wind power consumption on the resource, it would be useful to add "using observed or re-analysis wind fields" after "estimates" on line 1, and to place quotation marks around "common methodology" on line 25 and around "common method" on line 19 to differentiate them clearly from the Keith et al (2004) and Wang and Prinn studies that include consumption effects.

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Something is missing after “and” on line 14 (Wang and Prinn perhaps?).

We changed the phrasing of the paragraph to clarify the difference between the two types of studies. We added the term "using observed or re-analysis wind fields", too. Thanks also for pointing at the missing reference, which was a Latex-Coding error and is corrected in the revised manuscript. We added quotation marks around "common method" throughout the manuscript.

Interactive comment on Earth Syst. Dynam. Discuss., 1, 103, 2010.

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