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Earth System Dynamics Discussions

Interactive comment on "Jet stream wind power as a renewable energy resource: little power, big impacts" *by* L. M. Miller et al.

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REPLY TO L.M. MILLER ET AL.'S RESPONSE TO COMMENT

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The response starts (second sentence) with a statement, which introduces non-scientific criteria and can produce a negative image of the author and, more importantly, of the arguments. "Instead of responding in the same inflammatory dogmatic tone, we will instead focus on how to use Bergmann (2011) as a means to result in an improved final manuscript."

The present author refrains from introducing such criteria, even if it would be easy to

do that. A short (incomplete) reply to Miller et al.'s response (Miller et al. 2011) is considered to be sufficient, also in regard to soon closure of discussion period.

Miller et al. 2011 states that the comment is "focused on a few small mistakes, ", whereas the 'small mistakes' only occupy a minimal fraction of the comment. That statement is followed by a suggestive formulation: "It would appear that Bergmann (2011) agrees with our overall premise - ", whereas the comment in fact does agree with the main premises. The "would appear" is followed by two citations from the comment presented in reversed sequence. The first citation is from the concluding remarks (its end) and the second citation is from the introduction (its beginning). However, the "would appear" is not substantiated in the response.

At the bottom of page 2, Miller et al. 2011 states that the linear dependence of drag on velocity is applied in order to maintain analytical solutions. This could easily have been clarified in the discussion paper with one short sentence. Then it would have been clear to the reader that the sensitivity analysis in Figure 3 is of not more than semi-quantitative character. The energy-replenishment issue, which is a central point of the comment, is also mentioned in this context: "It also includes the replenishment term as the pressure gradient force F0 (Bergmann 2011 p.C245 "MGK2011 does not consider the processes of energy-replenishment to that reservoir)"." 1. The pressure-gradient force F0 is force and not power. 2. Replenishment by vF0, which is power, concerns the kinetic-energy (KE) reservoir only. 3. The energy reservoir in the citation from the comment (Bergmann 2011) is the sum KE + PE (PE is potential energy of the pressure field). 4. As the KE-replenishment happens on expense of the PE reservoir (cf. 1.), the relevant replenishment process is that of the PE reservoir. This process is, indeed, completely disregarded in the discussion paper, as well as in the response. The comment (Bergmann 2011) does not "desire a much more detailed simple model", but the physically correct application of square-dependence of drag on velocity.

On page 3, the response repeats wrong-context application of citations from the comment. The very long citation is introduced by: ". . . , the manuscript progressed to the

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parameterization and sensitivity analyses of a general circulation model. Bergmann (2011) is not satisfied with this parameterization either:" Citation from comment follows directly after the colon. The citation is from the section entitled "Drag on the Jet Stream" and has no relationship to the general circulation model. Moreover, the citation is incorrect at its end: " .. – MGK 2011 reproduces it!"" The correct citation is: " . . . – and MGK2011 reproduces it." More remarkable is the reply's omission of the subsequent explanation of the energetic difference between trees and turbines in favour of the comment's description of 'community'-errors. "Trees' wake flow is strongly turbulent and dissipates the extracted energy completely to heat, but turbines' wake flow cannot do that if the rotating turbine extracts large amounts of non-dissipative energy by producing a torque about its axis."

The inverted-v-axis argument on page 4 is completely wrong if the usual convention that northward velocity is positive is applied. This can be easily recognised from the reply's Fig.1, where the 200 hPa v in the northern-hemisphere tropical circulation is negative (southward), whereas the (common-knowledge) real meridional flow is northward (positive), cf. Dima et al. (2005), Fig. 1. Also, the statement that the undisturbed jets are located in zones of poleward meridinal flow is not correct. The annual-average jet core in northern hemisphere is located between 30°N and 45°N (Dima et al. (2005); Fig. 1), which is a zone of equatorward meridional flow at 200 hPa (Dima et al. (2005); Fig. 1). Insofar, all related arguments from the comment remain valid.

The v-differences plot in the reply's Fig. 2 shows a mass-balance consistent behaviour at ca. 35°N (provided that the pressure-levels have equal thicknesses), whereas the rest of the jet region does not. Especially between 40°N and 65°N, the disproportion between pressure-levels is very pronounced. Also, the values of the v-differences in the frictional layer are very large, approximately equal to their undisturbed values! Last, but not least, it is not clear whether Fig. 2 is based on the reversed v-axis or not. Too many questions to be able to make more comments on those issues. Also, in this regard, the comment's argument of contradictory results from the numerical model remains valid.

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REFERENCES Same as in comment

Miller et al. 2011 L.M. Miller, F. Gans, & A. Kleidon Response to comment by J.C. Bergmann esdd-2-C251-2011-supplement

Bergmann 2011 Bergmann JC, No additional power, potentially big impacts and physical problems. Earth Syst. Dynam. Discuss., 2 C244-250, 2011

Interactive comment on Earth Syst. Dynam. Discuss., 2, 435, 2011.