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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NO ADDITIONAL POWER, POTENTIALLY BIG IMPACTS AND PHYSICAL PROB-LEMS

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INTRODUCTION

The article by Miller, Gans and Kleidon (MGK 2011) discusses the very relevant issue that the real wind energy resource for large-scale utilisation is not described by the power density of the undisturbed flow. Of course, the discrepancy is very pronounced in the extreme case of the upper-troposphere jet streams, which have very small en-

ergy throughput in the undisturbed case. A similar discrepancy for the atmospheric boundary layer (ABL) has been discussed by the same authors in previous articles (GMK 2010, MGK 2010) and the present author commented on MGK 2010 (Bergmann 2010b) in regard to deficiencies in formulating momentum and energy balances for the atmospheric flow (rotational influence).

To some extent, the deficiencies continue in the accepted version of MGK 2010 (MGK 2011a) and also in the actual discussion paper, MGK 2011. The basic concept in MGK 2011 is correct and adequate: The energy density of the undisturbed flow and the associated pressure field's energy represent a reservoir, which is rapidly depleted in case of large-scale energy extraction. MGK 2011 does not consider the processes of energy-replenishment to that reservoir, but focuses on depletion of the pressure gradient of the jets only. Fundamental difficulties created by that concept are discussed below.

DRAG ON THE JET STREAM

MGK 2011 introduces a drag term kv "that characterizes friction and kinetic energy extraction by turbines, .." (page 438 line 26), with v the velocity vector and k a drag parameter of dimension [s-1]. All mass-related quantities in the subsequent equations are formulated in this sense as 'quantity per unit mass', but the text does not specify this and treats the symbols as representing the quantities themselves. Therefore, many positions in Table A1 do not agree with the symbols used in the equations, which is confusing. (Also confusing is the incorrect application of 'meridional' and 'zonal' in regard to the velocity components on page 438 lines 23, 24.) A direct physical error is committed by considering KE-extraction equivalent to a quantity that is a force (quotation above and Table A1).

Thus, turbine drag is presented as friction-equivalent. Such conception is incorrect because friction is completely dissipative, so that no electric energy can be provided by a 'turbine' that is a frictional apparatus, and all extracted energy is fed into the trans-

formation chain via turbulent kinetic energy (TKE) and subsequent viscous dissipation to heat. In absence of phase boundaries like Earth's surface, turbulence scales are very large in the horizontal, and their limitation in the vertical is defined by the buoyancy forces in the stable stratification present in upper troposphere. As discussed in Bergmann (2010a, 2010b), real turbines with good efficiency (little friction!) do not produce significant amounts of (additional) TKE. There is a general misconception in the wind energy meteorology 'community' that interprets turbine drag as friction-equivalent – in analogy to natural flow obstacles like trees – and MGK 2011 reproduces it. 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.

A second deficiency is the linear dependence of drag force on velocity. Turbine drag (and turbulent friction) has a square-dependence on velocity! This has been criticised in regard to MGK 2010 (Bergmann 2010b) and re-appears in the actual discussion paper MGK 2011. MGK 2011a took this into account and works with square dependence – why does the actual paper repeat an old error?

MGK 2011 is completely right in emphasising that (additional) drag on the jet stream necessarily leads to (additional) down-gradient, i.e., poleward flow. However, additional poleward flow in upper troposphere is only possible if there is the same amount of lesser poleward flow in the ABL below (mass-conservation!). That is only possible if ABL winds are weakened (or if other layers' equatorward flow in upper troposphere is intensified, which, however, cannot happen by energy extraction). Thus, no additional wind power can be gained by tapping the jet streams with very expensive turbines. Model results from Table 1 show large monotone reduction of natural dissipation that exceeds jet-stream extraction by a factor of 22 in the ABL and by a factor of 40 in the free troposphere, so that the ABL 'gains' power relative to the free troposphere. That model result is in contradiction to the basic analysis presented above. Therefore, it appears plausible to expect that tapping the jet streams could lead to very severe

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changes in atmospheric circulation. A definite answer, however, requires very thorough studies of the physics involved in the jet regime. Due to lack of specialised knowledge on modelling issues, the present author cannot go into details of the circulation model applied in MGK 2011. As indicated above, the present author is not convinced that simple application of a pre-existing circulation model, which is not elaborated in regard to incorporation of complete jet stream physics, can give reliable answers.

DEPLETION OF THE ENERGY RESERVOIR

MGK 2011 calculates the energy throughput of the undisturbed jet stream by consideration of its vertical velocity gradients (lateral gradients are neglected) and connected dissipation of kinetic energy. The crucial assumption behind this concept is that these gradients are associated with friction (drag at jet boundaries). Without drag, the gradients exist without any energy dissipation. On page 439 bottom, undisturbed-jet drag is introduced ad-hoc by formulating the energy equation with a free drag parameter kn. There is no further reflection on whether there in reality is a drag or not and no reflection on where the momentum flux is being absorbed (partner of interaction with the jet, Newton's Third Law). The same is done for the circulation model in section 3.1. An additional physical error is connected with the interaction problem: On page 440, lines 18, 19, the text states "natural dissipation of momentum at the edges of the jet stream". Momentum cannot be dissipated! It is exchanged between bodies that interact (Newton III). The concept of dissipation is defined for transformation of (mechanical) energy to heat. In distinction from energy, momentum cannot be transformed to different forms – and not at all to heat.

Figure 6 presents the model results for zonal and meridional velocities for undisturbed jets and for maximum extraction. It is evident that the undisturbed jets are located in a zone of equatorward meridional flow, which is counter-pressure-gradient! This is in agreement with common knowledge, e.g., Dima et al. (2005). Following the drag assumption of MKG 2011, this would mean that the natural frictional drag worked against the pressure gradient, and thus added energy to the flow. This is, of course,

impossible. The counter-pressure-gradient flow is necessarily driven by another energy source, also in the model (if the problem is not simply 'solved' by parameterisations). Therefore, it is also highly questionable whether a significant natural frictional drag on the undisturbed jets exists at all.

Figure 10 demonstrates that the modelled depletion (natural dissipation) (b) is about 20 times the modelled maximum extraction (c). Figure 6 demonstrates that meridional flow is reversed in case of maximum extraction (up-gradient flow turns into down-gradient flow), despite minimal energy extraction, which could only replace ca. 5% of dissipation (at constant replenishment rate of the energy reservoir). These enormous discrepancies indicate severe misconceptions of jet energetics.

In section 2's analytical model, depletion is parameterised as an additive counter-gradient force that depends linearly on down-gradient velocity. The natural conditions (up-gradient flow) imply negative 'depletion' also here. A central deficiency of this parameterisation is based on the fact that down-gradient flow characterises the drag force in steady-sate flow (Equation 1 left). But it does not characterise the processes that replenish the energy reservoir of the pressure field because there is simply no connection to those processes. The fact that 'depletion' has to be guessed by introduction of the free parameter gamma confirms this analysis. Therefore, MGK 2011's concept of depletion of the energy reservoir of the jets is insufficient. It does not include the processes that replenish the energy source of the jet, the pressure field, which is only represented by a force per unit mass, F, that is smaller than F0, but F is not equal to the pressure-field energy. Force is temporal rate of change of momentum and pressure energy is energy, and there is no possibility of equality whatsoever. Momentum and energy are independent conserved physical quantities. Another crucial deficiency of section 2 is the fact that the "depletion term" depends on down-gradient velocity but not on the actual value of the pressure-gradient force, which would be necessary for formulation of depletion of a definite reservoir of energy. The reference value F0 corresponds to a case of zero friction and zero turbine drag, which is of hypothetical charac-

ter within MGK 2011's concept. It cannot be specified from its observation because the concept considers the real undisturbed jet as subject to frictional drag. Therefore, the observation-based value in Table A1 cannot be F0. The concept is completely analogous to an infinite reservoir of electric energy (infinite capacity) whose zero-current voltage corresponds to F0, current corresponds to v and gamma corresponds to the internal resistance of the capacitor. Counter-voltage due to internal resistance is equal to the product of this resistance with the current (Ohm's Law). The concept is completely unrealistic in being (implicitly!) built on an infinite energy reservoir, and the paper's introduction of "depletion" is utterly inadequate in this context. Even if it is applied to the pressure gradient, the latter is proportional to the real finite pressure-energy reservoir of the jets. Moreover, an infinite reservoir with finite F0 would be possible only with infinite volume. As real depletion never stops, a non-zero steady state is impossible for a realistic finite reservoir because no replenishment term is included in MGK 2011's concept.

CONCLUDING REMARKS

In conclusion, drag (momentum-related) parameterisation of energy transformations in the numerical model is highly contradictory and cannot account for basic energetics of the real jets. The analytical energy-reservoir depletion concept is insufficient because a depleting reservoir's steady state value is necessarily zero. (The analytical concept is implicitly built on an infinite reservoir.) A real steady-sate value is necessarily determined by the processes that replenish the energy reservoir of the pressure field, which in its turn determines the power density of the flow and the maximal extractable power. MGK 2011's concept of pressure-gradient depletion is an example for introduction of unphysical conditions through parameterisation, and it is surely not the only one in atmospheric sciences. Physical reasoning reveals directly (without utilisation of numerical models) that no additional wind power can be gained and that the jets' resource must be very small. In regard to the impacts, specified detailed statements should only be made on the basis of models, which explicitly consider complete jet physics.

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