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Interactive comment on "Quantifying the global atmospheric power budget" *by* A. M. Makarieva et al.

Anonymous Referee #4

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Summary and recommendation

The main aim of this paper is to clarify the atmospheric power budget by seeking to exploit the divergent character of the gaseous mass flux in order to identify those terms in the power budget that can be related explicitly to the condensation/evaporation rates. The paper makes some valid point (Sections 2 and 3), such as pointing out that a term neglected in a recent study by Laliberte et al. (2015) is not only different from zero but too large to be really negligible, but the solution proposed does not seem valid. As to section 4, which claims to revisit the current understanding of the atmospheric power budget, it merely consists in some manipulation of the equations for a hydrostatic atmosphere that arguably sheds no light on the problem. The final section is too speculative. I don't think the paper makes a meaningful contribution to the understanding of atmospheric energetics, and I therefore cannot recommend publication.

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Discussion paper

Main comments

1. Abstract and elsewhere. I believe that the authors abuse the word *power*, which is used generically for all terms that enter the energy budget, such as in: *Kinetic power associated with horizontal motion, the kinetic power associated with vertical motion, and the gravitational power of precipitation.* In discussions of ocean and atmospheric energetics, it is more usual to restrict the term 'power' to the particular energy conversion responsible for supplying external energy to the system considered, and to be explicit as to what kind of energy conversions the other term represent. For instance, the term $u \cdot \nabla p$ is as far as a I can judge a conversion between available potential energy and kinetic energy, which is considerably more informative that 'kinetic power', and the authors should similarly clarify the physical meaning of the other terms.

2. The water cycle is generally regarded as making the atmospheric heat engine less efficient as the result of part of the solar forcing being expanded in lifting water vapour against the gravity field, part of which is then removed through precipitation, leaving only the residual to power the atmospheric circulation, an idea proposed by Pauluis and reprised in Laliberte et al. (2015). It seems that this should be discussed.

3. Remarks on the methodology. Physically, the atmospheric energy budget is best understood by introducing some kind of available enthalpy $ape = h(\eta, q_t, p) - h_r(\eta, q_t)$, where h is the moist specific enthalpy, η is some suitable definition of moist specific entropy, and q_t the total specific humidity, p is pressure, where $h_r(\eta, q_t)$ representing the part of the total enthalpy that is not available for adiabatic conversions into kinetic energy, so that

$$dh = (T - T_r)d\eta + (\mu - \mu_r)dq_t + \alpha dp$$

As a result, it is possible to express the total power term as

$$\int_V p\frac{D\alpha}{Dt}\rho dV = \underbrace{\int_V \frac{D(p\alpha)}{Dt}\rho dV}_{=0} - \int_V \alpha\frac{Dp}{Dt}\rho dV = \int_V \frac{T-T_r}{T}\dot{q}\mathrm{d}m + \int_V (\mu-\mu_r)\frac{Dq_t}{Dt}\mathrm{d}m$$

where $\dot{q}$ represents diabatic heating terms by all manner of conduction of radiation. This neglects the integral of dh/dt, but this term could be retained if desired. The passage from the first term to the second term requires $\nabla(\rho v) = 0$, and ρv to the total mass flux, in order to be able to claim that the integral of $D(p\alpha)/Dt$ vanishes, so the authors should clarify this point, as well as boundary conditions assumed by the different velocities entering the definition of v. In any case, the above formalism is usually what constitutes the starting point for linking the atmospheric power budget to a Carnot-like theory and for constraining the atmospheric power budget to solar heating, sensible heat fluxes, and condensation/evaporation process. The approach proposed by the authors seem to be quite unrelated to this standard view.

4. Sections 2 and 3. The whole point of the exercise of this exercise seems to establish that the term $\int_V dh/dt\rho\mathrm{d}V$ assumed to be zero in Laliberte et al. is actually not zero, and that it is too large to be neglected. I agree with this statement, but the result obtained by the authors seems unphysical. The simplest way to show that the above term is not zero is through using using standard integration by parts

$$\int_V \frac{dh}{dt}\rho\mathrm{d}V = \int_V \nabla\cdot(\rho h v)\,\mathrm{d}V - \int_V h\nabla\cdot(\rho v)\,\mathrm{d}V = \int_{\partial V} \rho h v\cdot n dS - \int_V h\nabla\cdot(\rho v)\mathrm{d}V$$

How to estimate this term depends on how the velocity v, the density ρ and enthalpy h are defined. If v is the fully barycentric velocity, and ρ the full density, then mass conservation imposes $\nabla\cdot(\rho v) = 0$, and the term is controlled by

boundary fluxes of enthalpy and is equal to the difference between the enthalpy evaporated minus the enthalpy precipitated. If ρv is the mass flux of the gaseous component of moist air, then how to estimate this term is more complicated, since $\nabla \cdot (\rho v) \neq 0$. Physically, the term $h\nabla(\rho v)$ is unphysical, since condensation or evaporation converts water vapour enthalpy h_v into liquid water enthalpy h_l and conversely, so should only involve the difference $h_v - h_l = L$, where L is latent heat, it should not involve the dry air enthalpy; the formula $h\nabla(\rho v)$ involves the dry air enthalpy, however, which is part of the definition of h.

Physically, the result should not involve the dry air enthalpy, and should also be independent of the different constants entering the definition of the three forms of enthalpy, which the authors have not shown.

5. Section 4. I don't really understand why this decomposition is useful. Indeed, a well known consequence of making the hydrostatic approximation is to filter out the contribution of the vertical velocity to the kinetic energy. As a result, the evolution equation for the kinetic energy becomes

$$\rho \frac{D}{Dt}\frac{u^2}{2} + u \cdot \nabla p = \rho F \cdot u \tag{1}$$

so that in equilibrium

$$\int_V u \cdot \nabla p \, \mathrm{d}V = Friction, \tag{2}$$

which shows that only what the authors call the kinetic energy power (the conversion between kinetic energy and available potential energy) becomes relevant to understand how the atmospheric circulation is powered. As is also well known, even without the hydrostatic approximation, the budget of gravitational potential energy is zero

$$\int_V \rho g w \, \mathrm{d}V = 0 \tag{3}$$

where ρw is the total mass flux, and hence decoupled from the kinetic energy budget. One may if one so desires to separate the total mass flux into gaseous and liquid components, and restrict attention to the former, for which the GPE budget becomes

$$\left.\frac{d(GPE)}{dt}\right|_{gas} = \underbrace{\int_V \rho g w \, \mathrm{d}V}_{-SW_P} + GAS\ DESTRUCTION = 0, \tag{4}$$

where ρw is now the gaseous mass flux only, GAS DESTRUCTION means GPE sink due to destruction of water vapour mass by condensation, but that does not make it less decoupled from the horizontal kinetic energy budget, where the underlined term is what the authors call the *power of precipitation*, whatever that means. Physically, this term represents primarily a conversion with internal energy, and is not directly related to the kinetic energy of the system, making its usefulness for clarifying the atmospheric power budget dubious. Moreover, it is also well known that for a hydrostatic fluid, it is the total potential energy of the system (i.e., the enthalpy) that matters, given that large variations in gravitational potential energy are compensated by large variations in internal energy, with no impact on kinetic energy. The focus on gravitational potential energy, therefore, is at odds with the common wisdom that GPE is not useful to consider on its own. The claim that GPE variations are somehow connected with kinetic energy production is odd, given that the hydrostatic approximation is unconnected to the vertical velocity field.

6. On a last note, I have a hard time accepting that the term $u \cdot \nabla p$ is something *observable*, given that the only way to estimate this term can only be done by means of a numerical model; likewise for the internal condensation/precipitation terms.

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