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Discussions

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

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Summary and recommendation I have reviewed previous incarnations of this paper a number of times now, and although I never liked it, I have at turns oscillated between overtly critical to more supportive, to the point of recommending its publication last time around. Feeling exhausted from reading yet another incarnation, and being disappointed that the authors have still not managed to improve clarity and succinctness, having also identified new major flaws, I have reverted to my original assessment that the paper is neither a useful contribution to the topic of atmospheric energetics, nor of sufficient scientific standard to be publishable in ACP or any other atmospheric journal.

The following provide some of my major objections to the paper. The paper is also much too long, often too speculative, contains too many technical errors, it lacks a

consistent description of boundary conditions, and does not even appear to capture such an effect as the enthalpy flux to the atmosphere due to freshwater entering the atmosphere having a higher temperature than that of the precipitation, which one would expect to be part of the problem. Moreover, it also relies too heavily on the continuity equations (5), which I believe is not the way moisture is handled in most climate and numerical weather prediction models, but is never mentioned.

Major issues

1. The authors motivate their paper by reviewing a number of definitions for atmospheric power that are equivalent for a dry atmosphere but not necessarily so in presence of condensation/precipitation, to ask the question of how best to define it in this case. This approach to the problem, however, is not optimal and furthermore misleading, because to a large extent, what one should call the atmospheric power depends on the model assumptions and equations used to describe the atmosphere. For instance, if one were to use the hydrostatic compressible Navier-Stokes equations

$$\rho\frac{Du}{Dt} + fk \times u + \nabla_h p = \rho F \tag{1}$$

then the steady-state kinetic energy budget would be:

$$\underbrace{-\int_V u \cdot \nabla p \mathrm{d}V}_{P} = \int_V \rho\varepsilon_K \mathrm{d}V \tag{2}$$

and one would naturally use the pressure gradient work of the horizontal velocity against the horizontal pressure gradient as the net atmospheric power. This is not really satisfactory either, however, because as pointed out in Tailleux (GRL, 2010), the pressure gradient work is a source of kinetic energy only when $-u\cdot\nabla_h p$ is positive, but a sink of kinetic energy and hence a dissipative term when it is negative. The correct approach is to split $P = P^+ - P^-$ as the difference between production and dissipation, in which case the balance becomes:

$$P^+ = P^- + \int_V \rho\varepsilon_K \, \mathrm{d}V. \tag{3}$$

In my opinion, such a decomposition is essential to understand atmospheric energetics, because as it is now increasingly realised, e.g., Tailleux (2013,Physica Scripta), dissipation of kinetic energy is not just achieved by viscous dissipation, as is often erroneously assumed, but by thermal dissipation as well (the so-called APE dissipation), which represents a second form of Joule Heating. There are two main ways to seek a decomposition of P into a production and dissipation term from first principles, based respectively on using the APE or entropy budgets, which in both cases yields:

$$P = \gamma Q_{in} - D_{irr} \tag{4}$$

where the thermodynamic efficiency γ_{APE} obtained from the APE budget is usually distinct from $\gamma_{entropy}$ obtained from the entropy budget. In the ocean, the distinction matters because γQ_{in} and D_{irr} obtained via the entropy budget are about two orders of magnitude larger than the values obtained from the consideration of the APE budget, and only the APE budget appears to provide reasonable estimates. In the atmosphere, the differences between the APE and entropy budgets are less important. Pauluis (JAS, 2007) has made the point that based on his estimates of the sources of APE in a moist atmosphere, viscous dissipation alone could not balance the source of APE, which suggest that P^- might be larger than the viscous dissipation term when moisture effects are explicitly retained. In the ocean for instance, the difference $G(APE) - D(APE)$ is about two orders of magnitude than each term taken separately. As far as I understand

the issue, only the use of the entropy or APE budget can shed light on the term P^+, which is the only meaningful way to define atmospheric power, since that the only term that is associated with the production of kinetic energy. Of course, using non-hydrostatic compressible equations and retaining the effects of a drag with the condensate will yield different kinetic energy budget, and hence a different form for what should be called the atmospheric power, but in all cases, only the positive part will matter. In order to make progress, the authors would need to explain how the term the thermodynamic efficiency γ and Q_{in} in the expression γQ_{in} are affected by condensation/evaporation. I do not understand how their approach is able to achieve that goal.

2. **Section 3.1 Boundary conditions** In this section, the authors discuss the boundary conditions for the air velocity at the surface. Physically, the problem is very simple: one needs a way to describe how freshwater enters and leaves the atmosphere via evaporation and precipitation. In practice, this is often done by enforcing a no-mass flux condition for the air velocity, freshwater entering the atmosphere via specification of the diffusive flux of freshwater. In ocean models, this has been the common practice for many years, evaporation and precipitation being then treated as a virtual salt flux. Over the past 20 years, however, ocean modelers have decided that it would be more physical to allow for the freshwater to enter and leave the ocean, as this is needed for instance to deal with sea level change due to land ice melting. In that case, one needs to modify the boundary condition for the diffusive part of the salt flux, so that no salt leaves or enter the ocean. A priori, exactly the same ideas could be used for the atmosphere, but the authors seem to argue that using a non-zero boundary condition for the vertical air velocity at the surface would lead to absurd results. This point is made in paragraph lines 187-194, and is based on pointing out that the term $I_s = p_s w_s$ would be dominate the energy budget. I admit that I was extremely puzzled by this statement last time around, which seemed to be wrong, but let

it pass because I could not immediately figure out the origin of the error. Upon reflection, the origin of the problem is quite simple, and arises from Makarieva et al.'s focus on the compressible work term PDv/Dt in isolation, whereas the correct approach is to consider the internal energy budget in its totality. Indeed, if the full internal energy equation, the authors would have realised that the term $p_s w_s$ naturally combines with $\rho e_w w_s$, with e_w the internal energy for freshwater, leading to a term $\rho(e_w + p_s/\rho)w_s = \rho h_w w_s$ which can be recognised as a flux of the enthalpy of freshwater, whereby the non-zero w_s converts the low enthalpy of the freshwater into the high enthalpy of the water vapour. Physically, this term is related to the latent heat flux in the ocean, and with an additional enthalpy flux resulting from the temperature of the evaporated freshwater being on average larger than the temperature of the precipitating freshwater. The whole section is therefore completely erroneous and misleading. Without a proper understanding of how boundary conditions for freshwater work, I don't see how they can claim to provide new insights into the role of the condensation/evaporation in the atmospheric energy cycle.

3. **Section 4. Practical implications.** In this section, the authors criticize the neglect of the integral $\int_V dh/dt\, \mathrm{d}\mathcal{M}$ in Laliberte et al. (2015). If the equations considered were formulated in terms of the full barycentric velocity, I would agree that this term is non-zero, and physically related to the exchange of freshwater between the land/ocean and the atmosphere, whereby the atmosphere gains freshwater at a higher temperature than it returns it to the land/ocean in the form of precipitation. The existence of this term has long been known by oceanographers, and was recently mentioned in Tailleux (2015), but it is generally considered to be sub-dominant in the ocean heat budget and often neglected. In practice, this term is not easy to incorporate in climate models, as it requires predicting the temperature at which rain falls into the ocean, but I am aware of some climate models which do it. A priori, this term is a enthalpy sink for the ocean,

and hence a heat source for the atmosphere. Whether Laliberte et al. (2015) were right or wrong in neglecting this term depends on the equations of motion underlying the MERRA re-analysis. Makarieva et al.'s objection is based on the assumption that the water vapour and condensate in the MERRA re-analysis satisfy the continuity equations (5)

$$\nabla \cdot (\rho v) = \dot{\rho}, \qquad \nabla \cdot (\rho_c b_c) = -\dot{\rho} \tag{5}$$

These equations, however, apply to the non-averaged equations of motion, but not necessarily to the large-scale motions considered in most climate and numerical weather prediction models. Indeed, most models seem to assume that the velocity carried in the model satisfy a continuity equation of the form

$$\frac{\partial \rho}{\partial t} + \nabla \cdot (\rho v) = 0 \tag{6}$$

with a separate evolution equation for

$$\frac{\partial r_v}{\partial t} + v \cdot \nabla r_v = -\nabla \cdot F_v + \dot{r}_v \tag{7}$$

for the mixing ratio or specific humidity r_v. If one furthermore assumes a no-mass flux condition at the surface, and represent evaporation via a diffusive flux of r_v (the term F_v) above, then it seems correct to me to regard the integral of Dh/Dt as zero, as assumed by Laliberte et al. (2015).

4. **Section 6** This section is quite speculative and philosophical in nature. In this section, the authors try to make the link with their biotic pump/condensation-driven theories, which are widely regarded as controversial, and which I have been pondering about for a very long time. Recently re-reading Makarevia et al. (2009), Precipitation on land versus distance from the ocean: evidence for a forest pump of atmospheric moisture, Ecological complexity 6, 302-307, I believe

I finally understood where Makarieva et al. got it wrong. In this paper, Makarieva et al. correctly points out that for a hydrostatic atmosphere, neither the dry air nor the water vapor are individually in hydrostatic balance. However, they incorrectly argue that $-[\partial p_v/\partial z + \rho_v g]$ is the force driving water vapor upward, where p_v and ρ_v are the pressure and density of water vapor. Indeed, the equation obeyed by water vapor is

$$\frac{D_v \rho}{Dt} + 2\Omega \times v_v + \frac{1}{\rho_v}\nabla p_v = -gk + \frac{1}{\rho_v}F_{dv} + \cdots \tag{8}$$

where F_{dv} is the intermolecular forces between dry air and water vapor responsible for keeping the difference between the dry air and water vapor individual velocities very small. To ensure momentum conservation, F_{dv} appears with an opposite sign in the momentum equation for dry air. In general, the smallness of the velocity difference between dry air and water vapor means that F_{dv} must balance at leading order $-[\partial p_v/\partial z + \rho_v g]$ and hence that the force actually acting on the water vapour is considerably smaller than assumed by Makarieva et al., invalidating their idea.