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Discussions

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

Anastassia M. Makarieva et al.

ammakarieva@gmail.com

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We thank our referee for the time spent on our work and for the numerous comments and criticisms which we sincerely value. While as we clarify below we do not agree with the criticisms, we nevertheless believe that this exchange is useful as it once again exposes and illustrates the many confusions and misconceptions surrounding the notion of atmospheric power.

1. Atmospheric power

1a. The referee characterises our approach to finding a proper formulation for atmospheric power as *"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."*

This is incorrect: there is no such freedom. To get a complete set of equations in atmospheric modelling one must use the first law of thermodynamics, which, as is well-known, relates heat input, mechanical work and change of internal energy of a given body. Since work in the atmosphere is performed by air parcels, to be consistent with, and constrained by, the laws of thermodynamics, global atmospheric power must be defined as work per unit time of all air parcels constituting the atmosphere. There is no other option.

We showed that independent of the presence of phase transitions –and indeed, contrary to the referee's claim, independent of the formulation of the equations of motion– total work per unit time of air parcels is given by the volume integral of $p\nabla \cdot \mathbf{v}$ (Eq. (3) on page 2), where p is pressure and $\mathbf{v}$ is the velocity of gaseous air. The referee has never commented on this result (item 1 on page 34 in the list of results in the concluding section).

1b. The referee's statement that in a hydrostatic atmosphere the "net atmospheric power" is given by the integral of $-\mathbf{u}\nabla p$ (Eq. 2 in the review, $\mathbf{u}$ is horizontal velocity) is likewise incorrect. In the presence of phase transitions "net atmospheric power", i.e. cumulative work performed per unit time by expanding (positive work) and contracting (negative work) air parcels, is given by the sum of the integral of $-\mathbf{u}\nabla p$ and the gravitational power of precipitation (see Figure 1 in our paper). That is to say, if the *work per unit time* term in the first law of thermodynamics is integrated over the entire atmosphere, the result is not equal to the integral of $-\mathbf{u}\nabla p$.

1c. Even if the referee specified the "net" atmospheric power correctly, the statement that *"only the positive part [of this power] will matter"* does not have a scientific justification. The "net" atmospheric power certainly matters, which is why it has been the focus of so many researchers, both before and after Lorenz, Laliberté et al. (2015) included. It matters because it is this power that is constrained by the laws of thermodynamics. Therefore, it is important to have this power properly formulated for a moist atmosphere, which is done in our paper and has not been done before.

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1d. Finally, we note that Tailleux (2010) considers decomposition of $pd(1/\alpha)$, $\alpha \equiv 1/\rho$ into a postive and a negative component, while in the present review the referee proposes to decompose the integral of $-u\nabla p$. These are however different magnitudes and neither is "net" atmospheric power. The first is atmospheric power minus a term proportional to the rate of phase transitions (see Eq. (11) on page 5) and the second one is kinetic power (kinetic energy generated per unit time, see Eq. (38) on page 13). Indeed, work and kinetic energy generation are not equivalent: there are processes where work is positive while kinetic energy generation is negative and vice versa (for details, see Makarieva et al. (2017) Tellus http://www.tandfonline.com/doi/full/10.1080/16000870.2016.1272752).

The boundary condition

2a. The referee disagrees with the boundary condition $w_s = 0$ for the vertical velocity at the surface, which we argue is necessary for the formulation of atmospheric power to be physically consistent. The reason given is *"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."*

We see a logical flaw here. In Section 3.1 we showed that if we assume a non-zero vertical velocity in the formulation of Eq. 3 of Pauluis et al. (2000) (hereafter PBH), then their own expression for atmospheric power, which is indeed the compressible work term, yields a figure exceeding solar power (see Figure 2 on page 16, right column). Since atmospheric power defined as "compressible work" is constrained by the first law of thermodynamics (and thus cannot exceed solar power), this means that assuming $w_s \neq 0$ as PBH did is not valid. This physical inconsistency has not been previously exposed. Thus the referee's objection appears to be pointless. Irrespective of whether we consider *"the internal energy budget in its totality"* or not, the obtained conclusion about atmospheric power remains unchanged.

2b. Furthermore, PBH state (and we agree) that the atmospheric power is

$$W = \int_{\mathcal{V}} p\nabla \cdot \mathbf{v} d\mathcal{V}. \tag{1}$$

At the same time, Laliberté et al. (2015) endorsed by Pauluis (2015) state (and we agree) that the atmospheric power is

$$W = -\int_{\mathcal{V}} \mathbf{v} \cdot \nabla p d\mathcal{V}. \tag{2}$$

As discussed in Section 3.1 of our paper, these two expressions agree with each other if and only if

$$\overline{p_s w_s} = 0. \tag{3}$$

If the referee disagrees with this boundary condition, he must disagree with either PBH or with Laliberté et al. (2015) or both regarding their formulation of atmospheric power. The referee, however, avoids commenting on the formulation of W, which is a bit unexpected, since the entire paper under review revolves around this issue.

2c. How the surface enthalpy flux should be consistently taken into account without assuming $w_s \neq 0$ is discussed in Section 4, see in particular Eqs. (59) and (60) on p. 21.

3. Practical implications

3a. The referee attempts to defend Laliberté et al. (2015) in that the enthalpy term was neglected by them correctly. To do so he states that *"most models seem to assume that the velocity carried in the model satisfy a continuity equation of the form"* of Eq. 6 in the review, which has a zero source/sink term.

Indeed, there are models including that underlying MERRA, which assume a zero sink/source in the continuity equation. However, as is well-known, such models do

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not conserve mass (of the water vapor and dry air alike). Therefore, such models' output is not valid for the purposes of Laliberté et al. (2015) who intend to study how atmospheric power is affected by the water cycle (i.e. by mass non-conservation).

To get around this problem, Laliberté et al. (2015) adopt a velocity correction procedure which does take into account the sources and sinks in the continuity equation. This correction and its implications are discussed in our paper on page 27 but appear to have escaped the referee's attention. Moreover, we show using NCAR/NCEP data that atmospheric power calculated without such a correction (i.e. without taking the source/sink term into account) is unrealistic (negative). This illustrates that all quantitative conclusions of Laliberté et al. (2015) strongly depend on the correct account of the sink/source term in the continuity equation.

Our understanding of the situation is that Laliberté et al. (2015) following PBH just did not realize how crucially their conclusions depend on the correct form of the continuity equations. Indeed, it is unlikely that Laliberté et al. (2015) intended their approach to be only applicable to a model with a deficient representation of the water cycle (zero source/sink), as some colleagues are now trying to argue defending the omission. Rather, using the words of Pauluis (2015), they aimed to present an *elegant* physical approach to constrain the atmospheric power, an approach that would be generally valid such that using it one could conclude something about the real atmosphere. However, owing to an insufficient attention to the constraints imposed by mass conservation, the enthalpy term was incorrectly neglected in their formulations.

3b. The referee proposes that Laliberté et al. (2015) use Eq. 7 on p. c6 in the review for the mixing ratio. This is incorrect. The equation they use is given on p. 2 of their Supplementary Material; contrary to the referee's claim, this equation, $\partial q/\partial t + \mathbf{v} \cdot \nabla q = \dot{q}$, does not contain the diffusion term.

4. Section 6 The referee states that he has found an error in the biotic pump physics by noting, following Makarieva et al. (2009) that dry air has a vertical distribution deviating from the equilibrium in an opposite direction than the water vapor.

The referee states in his Eq. 8 on p. c7 that the non-equilibrium vertical pressure gradient of water vapor acts on the water vapor molecules alone. Then, considering that the velocity difference between water vapor molecules and dry air molecules is very small (in fact practically zero), he concludes that *"the force actually acting on the water vapour is considerably smaller than assumed by Makarieva et al., invalidating their idea."* This conclusion, as well as Eq. 8, is based on the incorrect premise that a pressure gradient chooses which molecules to act upon.

In reality, if there is a pressure gradient it will act on all air molecules irrespective of the gas that brought it in existence. It is a misconception to assume that a pressure gradient of water vapor drives water vapor, while a pressure gradient of dry air drives dry air. For example, in a hurricane if the air moves along an isothermal surface with a constant relative humidity, the pressure gradient is solely created by dry air molecules (since water vapor pressure is constant). Nevertheless, there is no doubt that the water vapor is also accelerated by the dry air pressure gradient. Thus nothing about atmospheric dynamics, including the biotic pump or indeed any other circulation driver, can be deduced from the consideration of the zero velocity difference between the different gases. So we feel that the referee's objection could have been better justified, which we would welcome very much. In any case, we are delighted that the referee has apparently found the biotic pump thought-provoking as it has kept the referee thinking *"for a very long time"*.

The biotic pump physics is about the non-equilibrium pressure gradient that forms as water vapor condenses. It is a vertical pressure gradient; however, because of a nearly instantaneous hydrostatic adjustment this gradient does not drive rapid vertical air motions. Instead, because of air re-distribution (which causes dry air to deviate from hydrostatic equilibrium), there appears a horizontal pressure gradient driving air motions with the same power as the non-equilibrium vertical gradient would. This theoretical proposition is, as we argue in a number of papers including the present one, confirmed by observations.

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