

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

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Here we reply to Comments 4, 2, 2 and 4 of, respectively, Referees 1, 2, 3 and 4 that pertain to our analysis of Laliberte et al. 2015. Specifically, Referee 3 suggested that Laliberte et al. assumed no sources and sinks in the continuity equation. We thus show that this suggestion contradicts what Laliberte et al. themselves state.

Referee 3 suggests we should omit our discussion of Laliberte et al. But given that Laliberte et al. analyzed the same database and reached different conclusions – the discrepancy requires clarification. We found that Laliberte et al. omitted a crucial term, the global integral of the material derivative of enthalpy. Our analysis clarifies the scale of this omission and can, we hope, reduce future confusion. Thus we believe this contribution is constructive and is essential to placing our work in context.

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Below the referees' comments are listed each followed by our reply. Then there is revised and significantly shortened Section 3 (now Section 4), which contains our analysis of Laliberté et al. (2015). We correct two misprints in our previous comment [doi:10.5194/acp-2016-203-AC4, p. C10]: the third term in the right-hand side of Eq. (28) is  $\rho_c \mathbf{w} \cdot \mathbf{g}$  and the definition of  $\overline{K}$  in Eq. (29) misses the minus sign ( $\overline{K} \equiv -(1/S) \int_V K \rho dV$ ).

Comment 4 of Referee 1 [doi:10.5194/acp-2016-203-RC1]: 4. *The authors criticize Laliberte et al. (2015)'s estimation of the integral of  $dh/dt$ , as they believe that it is not  $dh/dt = 0$  but should be  $\partial h/\partial t = 0$  for a stationary budget. However, my understanding of Laliberte et al.'s study is that the total derivative that Laliberte et al used is in the context of global integration. So if you define  $H = \int h dV$ , then  $dH/dt = \int \partial h/\partial t dV$ , since the total volume is fixed in time. As such, Laliberte et al.'s global stationary approximation is consistent with your local stationary approximation.*

Laliberté et al. (2015) aim to estimate the global mean value of atmospheric power  $-(1/\alpha)(dp/dt)$ . They cannot therefore follow the above described procedure integrating the first law of thermodynamics first over mass  $\mathcal{M}$ ,  $d\mathcal{M} = \rho dV$ , and then taking its derivative over time. This procedure for  $-(1/\alpha)(dp/dt)$  would yield  $-\int_V \partial p/\partial t dV = 0$ .

Indeed, Laliberté et al. (2015) explicitly define  $dh/dt$  as the material derivative of enthalpy [see p. 540, middle column, 7th line from top], not the partial derivative over time. They state that they average the first law of thermodynamics taking the mass-weighted annual and spatial mean of all the terms in the equation, including  $dh/dt$  [p. 540, middle column, 7th line from bottom]. They denoted this mean as  $\{\cdot\}$ . The mass-weighted spatial mean of the material derivative of  $h$ , which is enthalpy per unit wet air mass, consists in taking its integral over total atmospheric mass and then dividing by the planet surface area. This means that stating that  $\{dh/dt\} = 0$  Laliberté et al. (2015) meant  $I_h \equiv (1/S) \int_{\mathcal{M}} dh/dt d\mathcal{M} = 0$  and not  $\partial(\int_{\mathcal{M}} h d\mathcal{M})/\partial t = 0$ .

We also note that, to support their statement that the expression for total atmospheric

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power does not contain the enthalpy term, Laliberté et al. (2015) refer to Eq. 4 of Pauluis (2011) [p. 540, right column, 12th line from top]. This link does not recognize that Eq. 4 of Pauluis (2011) [ref. 10 of Laliberté et al. (2015)] refers to atmospheric power defined *per unit dry air mass*. As we note in the revised text, the material derivative of any variable integrated over *total mass of atmospheric dry air* is zero (because of zero sources or sinks of dry air). In contrast, the material derivative of any variable integrated over *total atmospheric mass* is in the general case not zero, because of the non-zero sources and sinks in the continuity equation. This point, which follows from the previous derivations in the paper, is essential for understanding the atmospheric power budget and also for estimating it.

Comment 2 of Referee 2 [doi:10.5194/acp-2016-203-RC2]:

*2. Section 3.1. This section is also way too complicated. After the first paragraph, one can jump directly to the top of page 5. Now equation (15) is not wrong per se. ...*

Following the referee's suggestion, we revised the text to immediately obtain equation (15) for  $I_h$  (currently Eq. 32) after the first paragraph. The remaining part of the referee's comment was addressed separately in our second Author Comment [doi:10.5194/acp-2016-203-AC2].

Comment 2 of Referee 3 [doi:10.5194/acp-2016-203-RC3] (note it comes in several parts):

*2. Discussion of Laliberte et al. (2015)*

*The discussion of Laliberte et al. (2015) is very esoteric and does not pertain much to the rest of the discussion. Section 3.2 is a very minor point. It is fairly well-known that the integral of  $dp/dt$  is only equal to the work performed for a steady system, an assumption that is clearly stated in Laliberte et al.*

Section 3.2 did not mention Laliberte et al. and did not question their steady state assumption. This section drew attention to the  $\partial p/\partial t$  term and made a reference to

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Appendix C where it is shown that this term may be considerable on a seasonal scale, see Fig. 6a. As discussed later in the paper, this fact can account for the discrepancy between the seasonal changes of global mean precipitation  $P$  and  $W_P$  derived from mean atmospheric  $dp/dt$ . We removed Section 3.2 from the revised paper but extended the discussion of this matter in Section 5.

The referee continues: *As for section 3.1, there are several problems with the authors analysis. First, it should be clearly stated that the global integral of  $dh/dt$  is indeed zero in the absence of mass source and sink in the continuity equation.*

We see no problem here, as this statement immediately follows from the obtained expression for  $I_h$ . We have included the suggested statement in the revised text.

The referee continues: *First, it should be clearly stated that the global integral of  $dh/dt$  is indeed zero in the absence of mass source and sink in the continuity equation. This is the assumption made in Laliberte et al. It is also the continuity equation used in the MERRA Reanalysis. Hence, the authors should explicitly acknowledge that the claim that the integral of  $dh/dt$  is indeed correct within the assumptions made in the MERRA Reanalysis.*

The absence of mass source and sink in the continuity equation is equivalent to the absence of a water cycle. Laliberté et al. (2015) focus was on thermodynamic aspects of the atmospheric water cycle. They could not and did not assume the absence of mass source and sink in the continuity equation.

Specifically, on p. 2 in their Supplementary Materials, Laliberté et al. (2015) state: "In the atmosphere, the moist entropy  $s$  and the specific humidity  $q_T$  satisfy  $\partial_t s + v \cdot \nabla s = \dot{s}$  and  $\partial_t q_T + v \cdot \nabla q_T = \dot{q}_T$ , where  $\dot{s}$  and  $\dot{q}_T$  are their respective sources and sinks." (Note that the latter equation is equivalent to  $dq_T/dt = \dot{q}_T$ .)

To make it clear that this statement is incompatible with the assumption of "absent sources and sinks in the continuity equation", we consider the continuity equation for

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air as a whole

$$\frac{\partial \rho}{\partial t} + \nabla \cdot (\rho \mathbf{v}) = \dot{\rho} \quad (\text{c1})$$

together with the continuity equation for water vapor

$$\frac{\partial \rho_v}{\partial t} + \nabla \cdot (\rho_v \mathbf{v}) = \dot{\rho}. \quad (\text{c2})$$

Noting that  $q_T \equiv \rho_v / \rho$  (Laliberté et al. (2015) neglect the tiny condensate content) we find from Eqs. (c1) and (c2) that

$$\dot{q}_T = \frac{\dot{\rho}}{\rho} \left( 1 - \frac{\rho_v}{\rho} \right). \quad (\text{c3})$$

Thus, if Laliberté et al. (2015) had assumed  $\dot{\rho} = 0$ , they would have omitted not only the enthalpy term in their first law of thermodynamics but also the term proportional to  $dq_T/dt = \dot{q}_T$ . The latter term was the focus of their analysis though. Thus, the referee's suggestion that Laliberté et al. (2015) assumed  $\dot{\rho} = 0$  is not valid.

Neither is this assumption made in the MERRA database. What *is* assumed in the MERRA database and could also be assumed by Laliberté et al. (2015), as explained by Referee 2, see also Bosilovich et al. (2011), is that the vertically integrated continuity equation has no sources or sinks, that is  $\int \dot{\rho} dz = 0$ . However, as we discussed in detail in a previous comment [doi:10.5194/acp-2016-203-AC2], this relationship does make  $I_h$  equal to zero.

The referee continues: *Second, it is perfectly valid to question the impact of mass source and sink on the framework of Laliberte et al., but this should be done clearly. In particular, The Bernoulli equation is an equality with 4 different terms. Changing the mass conservation does not only affect the global integral of dh/dt, but also that of ds/dt and dq/dt. The authors here assume -without proof- that the change in the enthalpy integral would be reflected solely in the work output.*

If the referee's assumption about absent sources and sinks in the analysis of Laliberté et al. (2015) were correct, we would agree with this statement. For example, if Laliberte et al. defined  $h$  as enthalpy per unit dry air mass, then, as shown in our revised manuscript, the integral of  $dh/dt$  over *total dry air mass* would be zero. The other terms in the first law of thermodynamics would look different, too, if taken per dry air mass.

However, Laliberté et al. (2015) defined  $h$  as enthalpy per unit wet mass and, as is clear from their approach, integrated it over the entire mass of the atmosphere in the presence of mass sources and sinks. In this case the integral of  $dh/dt$  is not zero and its omission is not justified.

The referee continues: *The broader issue here is that the discussion of section 3.1. and 3.2. is presented without context and incomplete. It could only be understood by very few potential readers. It makes the paper unnecessarily confusing and should be removed.*

The work of Laliberte et al. 2015 is published in a journal aimed at a broad readership. Their account is clear: the authors present the first law of thermodynamics and set out to integrate it over atmospheric mass. All the terms in the corresponding equation are explicitly defined. Then they state that the global integral of one of the terms is zero [p. 540, right column, 3rd line from top]. We evaluate this integral and show that it is not zero and that its omission significantly impacts the paper's quantitative conclusions.

If we submitted our present manuscript without discussing Laliberte et al., a referee would rightly advise us to acquaint ourselves with the current literature and address the discrepancy between our results and those of Laliberté et al. (2015) (who analyzed the same MERRA database). We thus believe that our analysis of Laliberte et al. 2015 is an essential part of our study and have striven to present it as clearly as possible in the revised manuscript.

Comment 4 of Referee 4 [doi:10.5194/acp-2016-203-RC4]

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4. Sections 2 and 3. The whole point of the exercise of this exercise seems to establish that the term  $\int_V dh/dt \rho dV$  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 standard integration by parts

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

How to estimate this term depends on how the velocity  $v$ , the density  $\rho$  and enthalpy  $h$  are defined. If  $v$  is the fully barycentric velocity, and  $\rho$  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  $\rho v$  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  $h \nabla \cdot (\rho v)$  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  $h \nabla \cdot (\rho v)$  involves the dry air enthalpy, however, which is part of the definition of  $h$ .

As was stated in our manuscript (see Eq. 5 on p. 3) and is perhaps better emphasized in our revision (p. C4, first paragraph in Author Comment 3 doi:10.5194/acp-2016-203-AC3), velocity  $v$  is the velocity of the gaseous component of moist air (i.e. of the substance that actually performs work). Enthalpy  $h$  is defined per unit mass of wet air (i.e. dry air mass plus water vapor mass). There is thus nothing unphysical in the resulting expression for the integral of  $dh/dt$  over total mass of dry air and water vapor depending on parameters of *both* dry air and water vapor.

Revised Section 4 (former Section 3) follows<sup>1</sup>.

<sup>1</sup>This section follows revised Section 3 from our previous comment doi:10.5194/acp-2016-203-AC4. The rele-

## 4 Practical implications of the obtained relationships

In a recent effort to constrain the atmospheric power budget, Laliberté et al. (2015) used the thermodynamic identity

$$T \frac{ds}{dt} \equiv \frac{dh}{dt} - \alpha \frac{dp}{dt} + \mu \frac{dq_T}{dt}, \quad (31)$$

where  $s$  is entropy,  $h$  is enthalpy,  $\mu$  is chemical potential (all per unit mass of wet air),  $\alpha$  is specific air volume and  $q_T$  is the mass fraction of total water<sup>2</sup>. Laliberté et al. (2015) neglected, as we do, the atmospheric liquid and solid water content<sup>3</sup> and approximated  $q_T = q_v$ , where  $q_v$  is the mass fraction of water vapor.

Equations from the previous sections are as follows:

$$\frac{dX}{dt} \equiv \frac{\partial X}{\partial t} + \mathbf{v} \cdot \nabla X \quad (5)$$

$$\frac{\partial \rho}{\partial t} + \nabla \cdot (\rho \mathbf{v}) = \dot{\rho} \quad (6)$$

$$W_p \frac{\tilde{V}}{\tilde{m}} = W_p \alpha = p \left( \frac{d\alpha}{dt} + \frac{\alpha}{\tilde{m}} \frac{d\tilde{m}}{dt} \right) = p \left( \frac{d\alpha}{dt} + \alpha^2 \dot{\rho} \right), \quad (11)$$

$$W = W_{III} = \frac{1}{S} \int_{\mathcal{V}} p(\nabla \cdot \mathbf{v}) d\mathcal{V} = -\frac{1}{S} \int_{\mathcal{V}} (\mathbf{v} \cdot \nabla p) d\mathcal{V} + I_T + I_s \equiv W_I + I_T + I_s, \quad (12)$$

$$I_T \equiv \frac{p_T}{S} \int_{z=z(p_T)} (\mathbf{v} \cdot \mathbf{n}) dS = 0, \quad (13)$$

$$I_s \equiv \frac{1}{S} \int_S p_s (\mathbf{v} \cdot \mathbf{n}) dS = 0. \quad (14)$$

<sup>2</sup>The unconventional sign at the chemical potential term follows from  $\mu$  being defined in Eq. (31) relative to dry air: hence, when the relative dry air content diminishes this term is negative. For details see p. 8 in the Supplementary Materials of Laliberté et al. (2015).

<sup>3</sup>This assumption corresponds to an instantaneous removal of the non-gaseous water from the atmosphere by precipitation.

When integrating Eq. (31) over atmospheric mass, Laliberté et al. (2015) assumed that the enthalpy term vanishes,  $\int_{\mathcal{M}} (dh/dt) d\mathcal{M} = 0$ . This assumption was justified by noting that the atmosphere is approximately in a steady state. However, using the definition of material derivative (5), the steady-state continuity equation (6), the divergence theorem and the boundary conditions (13), (14) and noting that  $\rho \mathbf{v} \cdot \nabla h = \nabla \cdot (h\rho \mathbf{v}) - h\nabla \cdot (\rho \mathbf{v})$  and  $d\mathcal{M} = \rho d\mathcal{V}$ , we obtain

$$I_h \equiv \frac{1}{S} \int_{\mathcal{M}} \frac{dh}{dt} d\mathcal{M} = -\frac{1}{S} \int_{\mathcal{V}} h \dot{\rho} d\mathcal{V} \neq 0. \quad (32)$$

We can see that  $I_h$  is zero if only there are no sources and sinks of water vapor in the atmosphere, i.e. when  $\dot{\rho} = 0$ .

The physical meaning of this result is as follows. Enthalpy change per unit time in all air parcels (material elements) in a steady-state atmosphere is indeed zero. However,  $dh/dt$  is *not* equal to enthalpy change per unit mass of a given air parcel. (Likewise  $p d\alpha/dt$  is not equal to work per unit time per unit mass, see Eq. (11) above.) Indeed, for an air parcel of mass  $\tilde{m}$  total enthalpy of the parcel is  $h_p \equiv h\tilde{m}$ ; its change per unit mass is  $(dh_p/dt)/\tilde{m} = dh/dt + (h/\tilde{m})d\tilde{m}/dt \neq dh/dt$ . Therefore, the integral of  $dh/dt$  over total atmospheric mass is not zero. The same reasoning shows that if enthalpy were defined per unit dry air mass, such that  $h_p \equiv h\tilde{m}_d$ , then the integral of  $dh/dt$  per total *dry air mass* would be zero. Thus, we note the following general relationship. For any scalar quantity  $X$  in the view of the steady-state continuity equation (6) and Eqs. (13), (14) we have

$$\int_{\mathcal{V}} \frac{dX}{dt} \rho d\mathcal{V} = - \int_{\mathcal{V}} X \dot{\rho} d\mathcal{V}. \quad (33)$$

$$\int_{\mathcal{V}} \frac{dX}{dt} \rho_d d\mathcal{V} = 0. \quad (34)$$

Equation (34) follows from the continuity equation for dry air  $\nabla \cdot (\rho_d \mathbf{v}) = 0$ .

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The magnitude of  $I_h$  (32) can be roughly estimated assuming that evaporation and condensation are localized at, respectively, the surface  $z = 0$  and the mean condensation height  $z = \mathcal{H}_P$ . This approximation allows one to explicitly specify  $\dot{\rho}$  in (32) via the Dirac delta function  $\delta(z)$ :

$$\dot{\rho} = E(x, y)\delta(z) - P(x, y)\delta(z - \mathcal{H}_P), \quad \int \dot{\rho} dz = E(x, y) - P(x, y). \quad (35)$$

Here  $E(x, y)$  and  $P(x, y)$  are local evaporation and precipitation at the surface ( $\text{kg m}^{-2} \text{s}^{-1}$ ) with global averages  $E = P$ .

From (35) we have

$$I_h \approx -Eh_s + Ph(\mathcal{H}_P) \equiv -P\Delta h_c, \quad \Delta h_c \equiv h_s - h(\mathcal{H}_P), \quad h = c_p T + Lq_v. \quad (36)$$

Here  $c_p = 10^3 \text{ J kg}^{-1} \text{ K}^{-1}$  is heat capacity of air at constant pressure,  $L = 2.5 \times 10^6 \text{ J kg}^{-1}$  is latent heat of vaporization. We can see that  $I_h$  is proportional not to the difference between evaporation and precipitation (which can be locally arbitrarily small), but to the intensity of the water cycle  $E = P$  multiplied by the difference in air enthalpy between  $z = 0$  and  $z = \mathcal{H}_P$ .

For  $\mathcal{H}_P \approx 2.5 \text{ km}$  (Makarieva et al., 2013) and  $q_v(\mathcal{H}_P) \ll q_{vs}$  we have  $-P\Delta h_c = -P(c_p \mathcal{H}_P \Gamma + Lq_{vs}) \approx -1 \text{ W m}^{-2}$ . Here  $q_{vs} = 0.0083$  corresponds to global mean surface temperature  $T_s = 288 \text{ K}$  and relative humidity 80%; mean tropospheric lapse rate is  $\Gamma = 6.5 \text{ K km}^{-1}$ . Global mean precipitation  $P$  (measured in a system of units where liquid water density  $\rho_w = 10^3 \text{ kg m}^{-3}$  is set to unity) is equal to  $P \sim 1 \text{ m year}^{-1}$ , which in SI units corresponds to  $P = 3.2 \times 10^{-5} \text{ kg m}^{-2} \text{ s}^{-1}$ . A more sophisticated estimate of  $I_h$  (36) presented in Appendix B yields  $-1.6 \text{ W m}^{-2}$  with an accuracy of about 30%.

These estimates show that the enthalpy term cannot be neglected in Eq. (31) on either theoretical or quantitative grounds. By absolute magnitude the integral (36) is greater

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than one third of the total atmospheric power  $W \approx 4 \text{ W m}^{-2}$  estimated by Laliberté et al. (2015) for the MERRA re-analysis ( $3.66 \text{ W m}^{-2}$ ) and the CESM model ( $4.01 \text{ W m}^{-2}$ ).

Laliberté et al. (2015) appear to have first calculated the mass integral of  $T ds/dt$  from the right-hand side of Eq. (31), then calculated  $\mu dq_T/dt$  from atmospheric data and then used the obtained values and again Eq. (31) to estimate the total atmospheric power as  $-(1/S) \int_{\mathcal{M}} \alpha(dp/dt)d\mathcal{M}$ . In such a procedure, putting  $\int_{\mathcal{M}} (dh/dt)d\mathcal{M} = 0$  should have overestimated  $W$  by about  $1.6 \text{ W m}^{-2}$ . Since the omitted term is proportional to the global precipitation rate, it is crucial not only for a correct estimate of the mean value of  $W$ , but also for the determination of any trends related to precipitation.

Note also that even in the correct form, with the enthalpy term retained, Eq. (31) does not provide a theoretical constraint on  $W$ . This equation is an identity: it essentially defines  $ds/dt$  in terms of measurable atmospheric data. As is clear from Eq. (12),  $W$  can be estimated from the same data directly without involving entropy, as we discuss in the next section.

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