Response to the referees' comments on "Quantifying the global atmospheric power budget" doi:10.5194/acp-2016-203

1 Referee 1 [doi:10.5194/acp-2016-203-RC1]

1.1 Comment 1

Summary: In this study, the authors attempt to examine the gap between the gravitational power of precipitation, which is estimated as the total atmospheric power – kinetic power from the MERRA dataset, and another independent gravitational power of precipitation estimated from the surface precipitation data. While I can see a good merit of this work, I found the paper contains several loopholes that need to be clarified before the manuscript can be accepted for publication. Also, the presentation of this work is somewhat confusing, and can be simplified substantially to make it clearer. My concerns are given as bellow:
1. The evaluation of the gravitational power of precipitation (GPP) as presented in Appendix A, which is used to verify the GPP estimated from the MERRA data, contains a significant source of uncertainties as it depends so much on different input parameters as listed in Appendix A. Likewise, the GPP estimated from MERRA also depends strongly on the data resolution, the number of vertical levels, or the numerical approximations. Before trying to explain the discrepancies between GPP obtained from GPCP data and the GPP obtained from the MERRA data, the authors should at least quantify the errors in all of your numbers. While the authors claim that the uncertainty of your estimated GPP from GPCP is 30%, there is no guarantee that the difference between the two GPP estimations will be statistical significance. Afterall, 30% of 1 W m^{-2} is 0.3, and so it could be anything from 0.7-1.3 W m^{-2}, which may be comparable to the GPP computed from the MERRA data;

Response [see also doi:10.5194/acp-2016-203-AC6]:

We agree with the above points and explicitly discuss the uncertainty of W_P in Appendix B, last paragraph. Specifically, we make two notes regarding our conclusion that W_P in MERRA is underestimated. First, as illustrated by the derivation of Eqs. (20)-(22) (see the footnote[1]), W_P *must* depend on data resolution. Indeed, W_P derives from the vertical air velocity and thus describes rainfall associated with air motions at the considered scale.

Meanwhile the theoretical estimate of W_P is based on the total observed rainfall and thus assesses cumulative gravitational power of precipitation at all scales. If W_P derived from MERRA coincided with theoretical W_P, that would mean that no rainfall is associated with the air motions at a scale finer than 100 km and six hours. Since the scale of convection is of the order of a few kilometers or less, apparently some rain must remain unresolved by the larger-scale motions. Therefore, the fact that W_P in MERRA is lower than its independent theoretical estimate does not indicate inconsistencies in the database. This note is included in Section 5.1, p. 15.

Second, the theoretical estimate in Appendix B illustrates how the various parameters entering the value of W_P impact its magnitude. The bottomline however is provided by the TRMM-derived estimate of Pauluis and Dias [2012], which is 1.5 W m^{-2} for the area between

[1]

$$W = -\frac{1}{\mathcal{S}}\int_{\mathcal{V}} \mathbf{v}\cdot\nabla p d\mathcal{V} \equiv W_K + W_P, \tag{20}$$

$$W_K \equiv -\frac{1}{\mathcal{S}}\int_{\mathcal{V}} (\mathbf{u}\cdot\nabla p) d\mathcal{V} + W_c \approx -\frac{1}{\mathcal{S}}\int_{\mathcal{V}} \mathbf{u}\cdot\nabla p d\mathcal{V},\ \ W_c \equiv -\frac{1}{\mathcal{S}}\int_{\mathcal{V}} \rho_c(\mathbf{w}\cdot\mathbf{g}) d\mathcal{V}, \tag{21}$$

$$W_P \equiv -\frac{1}{\mathcal{S}}\int_{\mathcal{V}} \rho\mathbf{w}\cdot\mathbf{g} d\mathcal{V} = -\frac{1}{\mathcal{S}}\int_{\mathcal{V}} gz\dot{\rho} d\mathcal{V} = Pg\mathcal{H}_P,\ \ P \equiv -\frac{1}{\mathcal{S}}\int_{z>0} \dot{\rho} d\mathcal{V}. \tag{22}$$

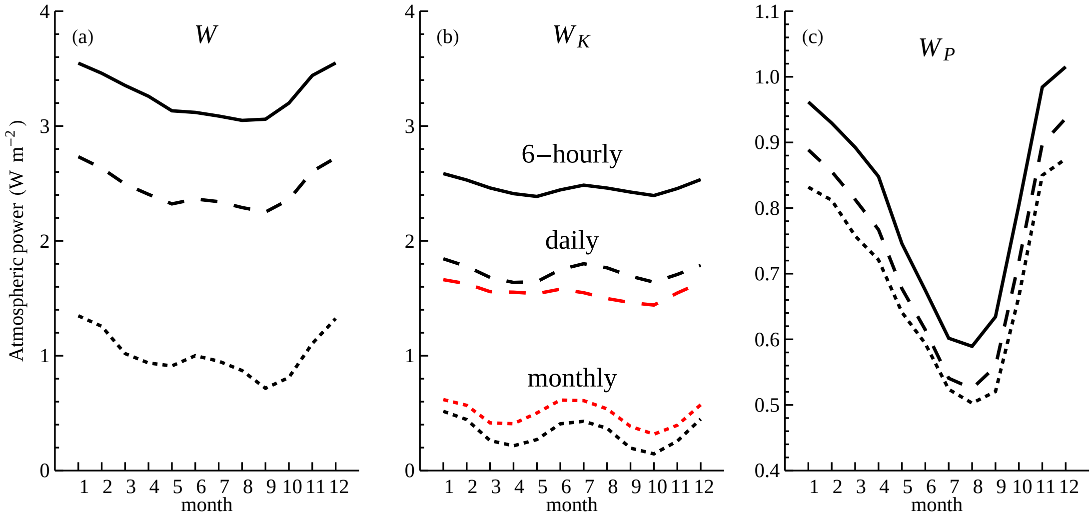

Figure c1: Long-term mean atmospheric power as dependent on temporal resolution: 6-hourly (solid curves), daily (dashed curves) and monthly (dotted curves). (a) total power W (20), (b) kinetic power W_K (21), (c) gravitational power of precipitation $W_P = W - W_K$. Black curves: MERRA; red curves: NCAR/NCEP.

30° N and 30° S. So, global W_P cannot be lower than 0.75 W m^{-2}. If it is 0.75 W m^{-2}, this means that there is no precipitation at all in the extratropics. However, since extratropical precipitation is significant (2.2 mm day^{-1} versus 3.1 mm day^{-1} in the tropics, see Fig. 5 in our manuscript), it will contribute to the global value of W_P. Even we assume that all extratropical rainfall precipitates from $\mathcal{H}_P = 1$ km (which is clearly an underestimate), global W_P will constitute 0.87 W m^{-2}. Therefore, the uncertainty of the lower limit of our estimate $W_P = 1$ W m^{-2} is about 10%. This note is included in Appendix B, p. 27.

We also emphasize in the new Section C4 of Appendix C that the second approach to estimating W_P (by extrapolation from the nearest pressure levels to the surface) produces an even lower estimate of W_P suggesting that our conclusion about W_P underestimated in MERRA is robust.

1.2 Comment 2

2. Estimations of the total atmospheric power W and W_K are subject to similar uncertainties as mentioned in my comment # 1 above. At resolution of 1.25 degree and 42 vertical levels, any global estimation of the total integrated energy and kinetic energy contains large variation, let alone the difference between two. Have the authors tried the NCEP reanalysis or ECMWF dataset at different resolutions to see how sensitive your estimations are? As long as we don't have reliable estimation of W, W_K, and GPP, explanation for the difference would provide little scientific value.

Response [see also doi:10.5194/acp-2016-203-AC6]:

We agree with the above comments and extended our analysis to include the NCAR/NCEP daily and monthly data for 1979-2015, as well as MERRA daily and monthly data. This yielded instructive results.

Two major conclusions emerged. First, the new data (Fig. c1) supported our original statement that estimated kinetic power W_K should grow with better resolution until all convective motions are resolved. Our analyses suggest that in this limit W_K should be about 4 W m^{-2}. This coincides with our previously published theoretical estimate of condensation-induced air circulation.

Second, we found that, unlike W_K, total power W and the gravitational power of precipitation W_P are not consistent across the re-analyses even if the zonal averages of local W are similar (Fig. c2); we explore the reason of these discrepancies and we have now suggested

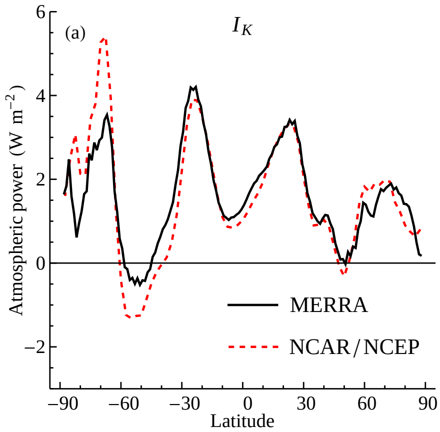

Figure c2: Long-term mean zonally averaged atmospheric power calculated from daily mean data for 1979-2015 in the MERRA versus NCAR/NCEP re-analysis as dependent on latitude (black solid curve: MERRA, red dashed curve: NCAR/NCEP). (a) $I_K \equiv -\int \mathbf{u} \cdot \nabla p dz$, (b) $I_\omega \equiv -\int omegadz$.

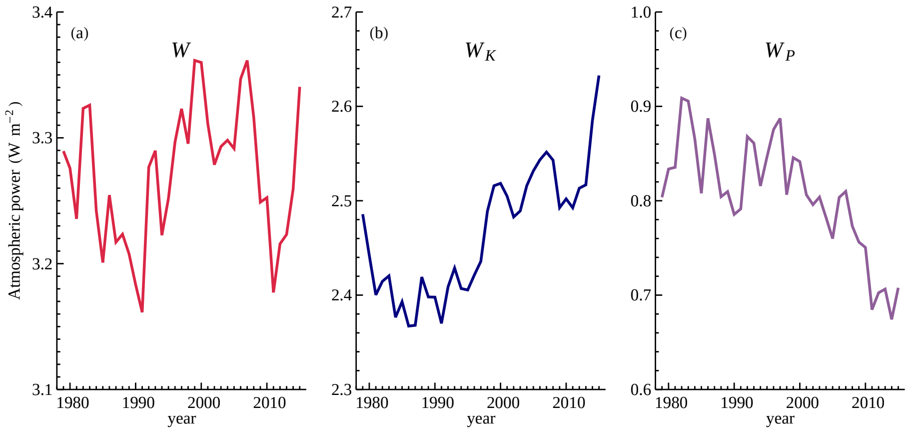

Figure c3: Trends in annual mean W, W_P and W_K derived from the 3-hourly instantaneous MERRA data.

how independent estimates of W_P might improve future estimates.

We added three new figures (Fig. c1, Fig. c2 and Fig. c3). The latter figure shows that W_P in MERRA is not correlated with global precipitation, which, according to recent analysis of Kang and Ahn [2015], rises in MERRA, while W_P, as we show, declines. We discuss how this may be an artefact of the correction procedures involved during the retrieval of the vertical velocities.

These new analyses are presented in the revised Sections 5.2 and 5.3.

1.3 Comment 3

3. The derivation of the total atmospheric power given by Eq. (7)-(8) is unnecessarily complicated. I can directly obtain Eq. (7) from Eq. (2) by noting simply that $\int pdV/dt = \int pd(\delta x \delta y \delta z)/dt = \int p(\nabla \cdot v)dV$. *Not sure why the authors present their argument in such a lengthy and confusing way.* The referee also noted in his general comments that *the presentation of this work is somewhat confusing, and can be simplified substantially to make it clearer.*

Following the suggestion of Referee 1, we unburdened Section 2 of the longer derivation of Eq. (7) from the continuity equation and the ideal gas law and derived the same result immediately from the consideration of the relative change of the air parcel's volume. However, this simpler derivation contains an implicit assumption, which necessitates our original longer derivation that uses the continuity equation and the equation of state for ideal gas (in the revised text it is moved to the new Appendix A). Specifically, this derivation assumes that any volume change occurs at the expense of the divergence of velocity $\nabla \cdot \mathbf{v}$ defined at an arbitrary scale. Since phase transitions involve gas velocities that are scale-specific, the plausibility of this assumption for this case requires a discussion. It is presented in the revised Section 2.

1.4 Comment 4

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 $H = \int hdV$, then $dH/dt = \int \partial h/\partial tdV$, since the total volume is fixed in time. As such, Laliberte et al.'s global stationary approximation is consistent with your local stationary approximation.

Response [see also doi:10.5194/acp-2016-203-AC5]:

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

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 $I_h \equiv (1/\mathcal{S}) \int_{\mathcal{M}} dh/dt d\mathcal{M} = 0$ and not $\partial(\int_{\mathcal{M}} hd\mathcal{M})/\partial t = 0$.

We also note that, to support their statement that the expression for total atmospheric 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 (see Eqs. 33 and 34), 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.

1.5 Minor concern

The practice of putting a dot on a variable to represent sources/sinks is too confusing, as the dot often denotes time derivation. Equation such $\dot{N} = dN/dt + N(\nabla \cdot v)$ is perplexing. The authors should replace all such dotted source/sink by different symbols to avoid the confusion.

Response [see also doi:10.5194/acp-2016-203-AC1, footnote 2]:

We agree with this comment, but ultimately chose to retain the dot over sources/sinks to ensure consistency in notations with the study of Laliberté et al. [2015], which we examine in detail.

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

2.1 Comment 1

This manuscript looks at the power budget in the MERRA reanalysis over the last 7 years. It is generally poorly written and way too long for the arguments being made. In its current state, it does not stand up as a contribution worthy of the high standards of publication for ACP. Based on my comments (to be found below), I do not recommend this manuscript for publication at ACP.

Key Comments:

1. Section 2 is both way too complicated and appears to be wrong. Following Vallis' (2006) notation:

$$W = \int_{\mathcal{V}} p \frac{d\alpha}{dt} \rho d\mathcal{V} = \int_{\mathcal{V}} p(\partial_t(\rho\alpha) + \nabla \cdot (\rho\alpha\mathbf{v}) - \alpha S_\rho) d\mathcal{V},$$

where $S_\rho = \partial_t(\rho) + \nabla \cdot (\rho\mathbf{v})$ *is the local sources and sinks of mass. Now,* $\alpha\rho = 1$ *so*

$$W = \int_{\mathcal{V}} p(\nabla \cdot (\mathbf{v}) - \alpha S_\rho) d\mathcal{V} = \int_{\mathcal{V}} (\nabla \cdot (p\mathbf{v}) - \mathbf{v} \cdot \nabla p - \alpha p S_\rho) d\mathcal{V}.$$

This is the same form as in equation (8). But it depends explicitly on S_ρ*, contrary to the authors' claim. Why this contradiction? The problem in the authors' derivation comes in part from equation (3). While it is true that* $\sum_i d\tilde{N}_i/dt = 0$*, it is not true that* $\sum_i T_i d\tilde{N}_i/dt = 0$*, unless the atmosphere is isothermal. But it is exactly what's used to convert the last term in equation (2) to the last term in equation (4).* $\tilde{V} = \tilde{N}/N$ *has units of* m^3 *(parcel*$^{-1}$*). To compute work, however, we need the specific volume with units of* m^3 *(kg*$^{-1}$*). So we have to introduce a new quantity, the mass per parcel* $\tilde{m}$ *so that the specific volume is* $\tilde{V}/\tilde{m}$*. Then the expression for work (equation 4) with the same units as in Vallis (2006) reads:*

$$W = \frac{1}{\mathcal{S}} \int_{\mathcal{V}} p \frac{\tilde{m}}{\tilde{V}} \frac{d(\tilde{V}/\tilde{m})}{dt} d\mathcal{V}.$$

But the continuity equation (6) also requires fixing. Since, N *has units of mol* m^{-3} *then equation (6) is an equation for mass conservation only if the molar mass* $\tilde{m}/\tilde{N}$ *is constant. But here the authors are, among other things, concerned about the effect of moisture on the work and moist air, unlike dry air, has an inhomogeneous in molar mass. The continuity equation (6) should then read:*

$$\partial_t((\tilde{m}/\tilde{V})) + \nabla \cdot (\mathbf{v}(\tilde{m}/\tilde{V})) = \dot{\tilde{m}}(N/\tilde{N}) + \tilde{m}/\tilde{N}\dot{N} - (\tilde{m}N/\tilde{N}^2)\dot{\tilde{N}}$$

where the right hand side is the local sources and sinks of mass. With these fixes, the expression for work will look exactly like in Vallis (2006) and will depend on the sources and sinks of mass.

Response [see also doi:10.5194/acp-2016-203-AC3]:

In our revised work we consider the entire MERRA data span from 1979 to 2015 at several temporal resolutions; plus additionally NCAR/NCEP data for the same period.

The discrepancy between our Eq. (7) and the referee's derivation results from the incorrect definition of work per unit mass. In the presence of phase transitions it is not $pd\alpha$ as clarified in our revised Section 2, see Eq. 11. Indeed, consider an air parcel of mass $\tilde{m}$ and volume $\tilde{V}$, such that $\alpha \equiv \tilde{V}/\tilde{m}$. Then work of this parcel is $pd\tilde{V}$, while work per unit mass is $(1/\tilde{m})pd\tilde{V} = (1/\tilde{m})pd(\alpha\tilde{m}) = pd\alpha + p\alpha d\tilde{m}/\tilde{m} \neq pd\alpha$. Therefore, $W \neq W_{IV} \equiv (1/\mathcal{S}) \int_{\mathcal{V}} p(d\alpha/dt)\rho d\mathcal{V}$.

We also note that in our derivation we did not assume either $\sum_i d\tilde{N}_i/dt = 0$ or $\sum_i RT d\tilde{N}_i/dt = 0$. This misunderstanding might have arisen because the derivation was presented in a very compact form. The revised more detailed text (new Appendix A) makes it clear that the resulting expression for work does not depend on the temperature term discussed by the referee.

We have striven to present our arguments as succinctly as possible in the revised manuscript. But the need to identify and discuss the various inconsistencies that surround the topic we examine stands in the way of a radical shortening.

2.2 Comment 2

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. However, the Makarieva et al. (2013) analytical derivation is somewhat meaningless when applied to re-analysed data: this can be evaluated directly. This is exactly what I have done for the purpose of this review. Using the 1 hourly vertically integrated budgets provided from the data archive, one can compute the integral $\int_{\mathcal{S}} \overline{\dot{h}}\overline{\dot{\rho}} d\mathcal{S}$, *where the overline indicates vertically integrated fields. In the reanalysis,* $\overline{\dot{\rho}} \neq 0$ *because of the analysis step. In MERRA, this is provided directly. In the MERRA documentation it is indicated that this* $\overline{\dot{\rho}}$ *includes both the effect of* $E - P$ *and adjustments needed to represent the observed surface pressure field accurately. It therefore includes the effect described by the authors. This quantity for 1980-1985 is 0.2* W/m^2. *Adding the vertical dependence would likely be a second order effect since* $E - P$ *is mostly driven by horizontal and temporal variability. This simple analysis performed using the output from the MERRA product seems to show that Appendix A is likely to be inaccurate (0.2 is not within 30% of 1.6). In any case, this issue was discussed at length by Trenberth (see his papers in the 1990's) and the proposed solution is to modify the winds so that the continuity equation does not have a source term. I had a hard time finding this but you mention that Laliberte et al (2015) might have done something like this. In this case, I do believe that* $\int_{\mathcal{M}} dh/dt d\mathcal{M} = 0$ *makes sense since it is an exact derivative.*

Response [see also doi:10.5194/acp-2016-203-AC2]:

We thank Referee 2 for their effort to numerically check our results. However, as we show below, the estimate obtained by Referee 2 appears to result from a misunderstanding (a confusion of the mean of a product $\overline{f_1 \cdot f_2}$ for the product of means $\overline{f_1} \cdot \overline{f_2}$, which is fatal when $\overline{f_1} = 0$). As such, this estimate neither disproves our theoretical result nor justifies the omission of I_h by Laliberté et al. 2015. As we clarify below, we have demonstrated in our work that I_h is not proportional to the vertical integral of the source term $\int \dot{\rho} dz$ and does not vanish when the latter is zero.

We presume that the referee's agreement with our Eq. (15) pertains to the equality

$$I_h \equiv \int_{\mathcal{M}} \frac{dh}{dt} d\mathcal{M} = -\int_{\mathcal{V}} h\dot{\rho} d\mathcal{V} \equiv -A. \qquad \text{(c1)}$$

The referee proposes to estimate A as

$$A \approx B \equiv \int_{\mathcal{S}} \widehat{\dot{h}}\widehat{\dot{\rho}} d\mathcal{S}. \qquad \text{(c2)}$$

suggesting that $\widehat{\dot{h}}$ and $\widehat{\dot{\rho}}$ are available from the MERRA dataset (we replaced the overline by $\widehat{}$ in B to preserve the overline for the averages to appear below).

We need first to resolve an inconsistency between the units of our A and the referee's B. First, we note that the dot over enthalpy h in B may be a misprint since an *enthalpy source* $\dot{h}$ appears to be an unspecified variable out of context. Next, if following the referee's indication that $\widehat{}$ in B denotes *vertically integrated fields* we assume that $\widehat{h} \equiv \int hdz$ and $\widehat{\dot{\rho}} \equiv \int \dot{\rho} dz$, then B has the units of [J s^{-1}m], while A has the units of [J s^{-1}]. So expression B needs some "fix" before it could be compared with A.

Keeping $\widehat{\dot{\rho}} \equiv \int \dot{\rho} dz$, the only way we can see to remedy B is to assume that $\widehat{h} \equiv \int hdz / \int dz$, units [J kg^{-1}] is the mean enthalpy in the air column (not the vertically integrated enthalpy [J kg^{-1} m]). In this case the units of A and B coincide and what the referee proposes reads

$$A \approx \int_{\mathcal{S}} \left(\frac{\int hdz}{\int dz} \int \dot{\rho} dz \right) d\mathcal{S}. \qquad \text{(c3)}$$

Noting that $d\mathcal{V} = dzd\mathcal{S}$, this implies the following replacement in A

$$\int h\dot{\rho}dz \approx \frac{\int hdz}{\int dz} \int \dot{\rho}dz. \tag{c4}$$

By dividing both parts of (c4) by $\int dz$ we find that (c4) relates the columnar mean of $h\dot{\rho}$ to the product of columnar means of h and $\dot{\rho}$. The two expressions are not equivalent, since, as is well-known:

$$\overline{h\dot{\rho}} = \overline{h} \cdot \overline{\dot{\rho}} + \overline{(h - \overline{h})(\dot{\rho} - \overline{\dot{\rho}})}, \tag{c5}$$

where $\overline{X} \equiv \int Xdz / \int dz$. The second term in the right-hand part of (c5) represents the covariance of the two variables. Indeed, we know that the enthalpy and the rate of phase transitions in the atmosphere are spatially correlated: h is higher at the surface where evaporation occurs and $\dot{\rho} > 0$ and lower in the upper atmosphere where condensation occurs and $\dot{\rho} < 0$. Therefore, $\overline{(h - \overline{h})(\dot{\rho} - \overline{\dot{\rho}})}$ in (c5) is not zero.

When, as proposed by the referee, $\int \dot{\rho}dz \to 0$ and $\overline{\dot{\rho}} \to 0$, the first term in (c5) disappears. The relative error of estimating $\overline{h\dot{\rho}} \neq 0$ by $\overline{h} \cdot \overline{\dot{\rho}}$ tends to infinity. For this reason B carries no information about the real value of A and, hence, I_h (c1).

Note also that since the enthalpy of an ideal gas is defined to the accuracy of an arbitrary constant, the absolute magnitude of $\overline{h} \cdot \overline{\dot{\rho}}$ for $\overline{\dot{\rho}} \neq 0$ does not have any physical meaning as it explicitly depends on that constant. The second term in the right-hand part of (c5) is constant-invariant.

In our work we have estimated I_h assuming that evaporation and condensation are localized at, respectively, the surface $z = 0$ and the mean condensation height $z = H_P$. This approximation allows one to explicitly specify $\dot{\rho}$ via the Dirac delta function

$$\dot{\rho} = E(x,y)\delta(z) - P(x,y)\delta(z - H_p), \quad \int \dot{\rho}dz = E(x,y) - P(x,y), \tag{c6}$$

from which I_h can be explicitly evaluated.

Putting $E(x,y) = P(x,y)$ in Eq. (15), such that $\int \dot{\rho}dz = E(x,y) - P(x,y) = 0$, one obtains from our Eq. (15) that the integral I_h is proportional not to the (zero) difference between evaporation and precipitation, but, as one might have expected, to the intensity of the water cycle, i.e. to $E(x,y) = P(x,y)$ multiplied by the difference in air enthalpy between $z = 0$ and $z = H_P$. (This clarification is added to the revised manuscript, see Section 4.). Since no global observational data exist on the local values of $\dot{\rho}$, our theoretical estimate is currently the only available estimate of I_h (c1).

In revised Section 5.2 we discuss the correction procedure proposed by Trenberth [1991]. Strictly speaking, this procedure does not modify the winds such that the continuity equation does not have sources or sinks. Rather, to achieve mass conservation this correction does take the non-zero sources and sinks into account, but only in the vertically integrated form, since local values of $\dot{\rho}$ are unknown. We suggest that this procedure might be responsible for the physically unreasonable seasonal cycle and multiyear trend of W_P in MERRA, whereby W_P is uncorrelated with precipitation.

2.3 Comment 3

3. Computing the work from MERRA data. As mentioned before, the MERRA product has many vertically integrated budget variables that allow one to quantify each one of the term in the energy equation. For this review, I've looked at the kinetic energy generation 1980-1985 and the yearly average gives 3.40-3.48 W/m^2 for the integral of $\omega\alpha$ and 3.6-3.8 W/m^2 when including the kinetic energy generation from the analysis step. The kinetic energy generation is balanced by damping from the numerical dissipation, the dynamical remapping and the physically parametrized frictional dissipation. This means that the estimates provided in section 5.1 are substantial underestimates.

Response [see also doi:10.5194/acp-2016-203-AC6]:

We agree with the referee that our estimates of W are substantial underestimates of the real atmospheric power; indeed, it is our major point. We do not consider the kinetic energy generation from the analysis step. (Neither did Laliberté et al. [2015]). We explicitly address how the global atmospheric power can be estimated using the re-analysis pressure and velocity at their face value, based on our Eqs. (20)-(22). In our revised manuscript we present an analysis of the entire period 1979-2015. We note that for W_K our results practically coincide with those of Huang and McElroy [2015], who reported $W_K = 2.46$ W m^{-2} for 1979-2010. Our calculations for the same period give $W_K = 2.45$ W m^{-2}.

Our annual estimates of W for 1980-1985 range from 3.20 to 3.28 W m^{-2}. This is 6% smaller than the referee's. The discrepancy may stem from two sources. First, the dataset with the vertically integrated $\omega\alpha$ derives from the 1-hourly surface dataset (presumably MAT1NXINT [tavg1_2d_int_Nx]), while our estimate derives from the 3-hourly dataset (MAI3CPASM [inst3_3d_asm_Cp]). As we have shown that W increases with finer temporal resolution, see Fig. c1a, this may explain the 6% discrepancy. Second, the discrepancy may stem from a difference in the boundary condition for ω at the surface.

It is not explicitly indicated in MAT1NXINT how the integration was performed. We have described in detail how we treated the surface layer to make our analysis tractable and comparable to other studies. Furthermore, we investigated the impact of the surface boundary condition on our analysis for each variable and showed that the associated uncertainty is about 6%.

For W and W_K we estimated the value of ω and $\mathbf{u} \cdot \nabla p$ at the surface in two ways (see Appendix C in our revised manuscript). One is to assume that air velocity at the surface is zero, $\mathbf{v} = 0$, another is to linearly extrapolate ω and $\mathbf{u} \cdot \nabla p$ from the nearest pressure level to the surface. Our increased attention to the boundary layer is justified by the fact that horizontal velocity experiences significant non-uniform changes along the vertical. In the limit of an infinitely precise vertical resolution the two approaches should give the same value. In the real atmosphere they produce somewhat different results.

Specifically, the extrapolated W_K turns out to be higher than W_K calculated assuming $\mathbf{v} = 0$. This has to do with the vertical profile of W_K shown in Fig. c4. Kinetic energy generation grows with increasing pressure in the lower atmosphere. Extrapolation of this dependence to the surface yields a positive surface value for kinetic energy generation. Thus, W_K obtained from this extrapolation is higher than when we assume that $\mathbf{v} = 0$, such that no kinetic energy is generated at $z = 0$

In contrast, the estimate of total power W is smaller when extrapolated than when assuming zero velocity at the surface. This has to do with a different distribution of pressure velocity over pressure levels, Fig. c4. Here the lowest layer between 975 hPa and the surface makes a large negative contribution to the total W. This is because the air predominantly descends in the regions of higher surface pressure. Therefore with one and the same ω at 975 hPa, the layer where the air descends and surface pressure is about, say, 1020 hPa is thicker than where the air ascends and surface pressure is about 1000 hPa. Since W is proportional to $-\omega$, in the result the net contribution of the lower layer to global W is negative.

The difference between the two estimates for W and for W_K is about 10%. The difference between W_P values obtained by the two means is greater. W_P obtained by interpolation is considerably smaller than W_P obtained assuming that zero velocity at the surface. This suggests that our conclusion about W_P being underestimated in MERRA is robust.

2.4 Comment 4

4. In section 5.1, I do not see the use for W_1. And why not use $\omega_s = \partial_t p_s + \mathbf{v}_s \cdot \nabla_H p_s$, with ∇_H being the horizontal gradient? The p_s and $\mathbf{v}_s$ are both available and this is the right expression. Maybe that could fix their underestimate of W.

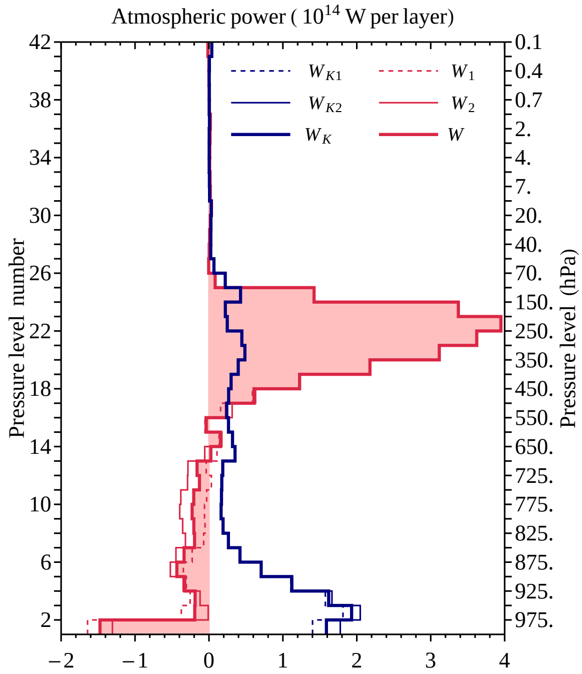

Figure c4: This figure was added to the revised text, see the new subsection C3 in Appendix C. Atmospheric power within the 41 pressure layers enclosed by the 42 pressure levels in the MERRA dataset MAI3CPASM in 1979-2015. Each bar of the histogram contains the contribution from the corresponding pressure layer (p_i, p_{i+1}), where i is pressure level number, plus the contribution from layer (p_s, p_i) if $p_i \leq p_s$ in the considered cell is the pressure level nearest to the surface. For example, the lowest bar of the histograms corresponds to the layer with pressure less than 975 hPa (i.e. the layer from $p_1 = 1000$ hPa to $p_2 = 975$ hPa plus the layer from p_s to p_1). Sum of the histogram values over all layers gives the global values of W and W_K. Subscripts 1 and 2 refer to the two ways of estimating W and W_K, see Table 2 for details.

Response [see also doi:10.5194/acp-2016-203-AC6]:

The use of W_1 has been discussed in our reply to Comment 3 of Referee 2. W_1 and W_{K1} were used to investigate the uncertainty associated with insufficient data resolution in the boundary layer.

One cannot use $\omega_s = \partial_t p_s + \mathbf{v}_s \cdot \nabla_H p_s$, because the horizontal gradient of surface pressure $\nabla_H p_s$ only exists if the surface is horizontal (i.e. has invariant geopotential height). Since the geopotential height of the real surface varies, surface pressure is much more affected by this variability in the vertical plane than by any effects in the horizontal plane, which prevents the use of p_s for a reliable determination of ω_s.

Furthermore, since the term $\mathbf{v}_s \cdot \nabla_H p_s$ is present in the surface values of both ω and $\mathbf{u} \cdot \nabla p$, even if this term were added, this would not change the difference between the global W and W_K.

To simplify the presentation, in the revised text we everywhere use W_2 and W_{K2} and discuss W_1 and W_{K1} only in a separate Section C3 in Appendix C.

2.5 Comment 5

5. The way I see it, there are approximately three manuscripts in this study. The first one, sections 2 and 3 as well as Appendix A, consist mostly of derivations that are either flawed or mostly useless for this study. The second paper is more akin to a white paper and comprises *sections 6.1 and 6.2. Now, sections 1, 4, 5 and the very beginning of section 6 as well as Appendix B and C are self-contained and describe an original treatment of reanalysis data. Appendix C could be moved up after section 4. If they wish to submit their results to another publication, I would recommend that the authors focus on these sections and perform their analysis on the whole of MERRA (1979-2015).*

Response:

We thank the referee for this kind word about our data analyses. We followed their recommendation to analyze the whole of MERRA (1979-2015) in the revised text, see Section 5. However, as we argued in our response to Comments 1 and 2 of the referee, we disagree that Sections 2 and 3 (now 2 and 4) are not relevant to these data analyses. We find that the literature on our topic, the definition and estimates of global atmospheric power, lacks clarity (see, for example, our Response to Comment 2 of Referee 4 below). Before setting out to analyze numerical data, it is essential to define on clear physical grounds what we are going to measure and constrain. In the revised text we overview the various formulations for global atmospheric power and show why and how they differ and which can be applied to a moist atmosphere (see Introduction, especially Eqs. (1)-(4), and Sections 2 and 3).

We appreciate the referee's mentioning of Appendix C (now D), as we believe that it does indeed contain some interesting results. In particular, in Fig. 10 in the revised manuscript (see Fig. c5) we show that the integral of pressure tendency $\Psi \equiv \frac{1}{S}\int_{\mathcal{V}} \frac{\partial p}{\partial t} d\mathcal{V}$ is comparable to global atmospheric power W on a seasonal scale and that the formulation of the continuity equation with use of hydrostatic equilibrium prevent a consistent account of this term. If we formally add Ψ to $\Omega \equiv -\frac{1}{S}\int_{\mathcal{V}} \omega d\mathcal{V}$ (which should give W) we obtain an unreasonable result that total atmospheric power W during certain months is smaller than kinetic power W_K.

It is also notable that Ψ can be estimated using a very simple formula for a periodically warming and cooling hydrostatic atmosphere from the observed rate of global temperature change.

2.6 Comment 6

6. Finally, I'm not sure the following sentence is logically true: "The fact that W_{Kc} is likewise higher than our MERRA-derived kinetic power, testifies in favor of the theoretical estimate". All it means is that W_{Kc} is potentially a right upper bound. The only way to check whether it is the right upper bound would be to either verify if it holds on other Earth-like planets or using simulations with increasing resolution and seeing that it describes the scaling. As I said before, the last two sections of this manuscript are really too speculative in their current form and they are dragging down the original results described in sections 5.

Response [see also doi:10.5194/acp-2016-203-AC6]:

We explained in our original manuscript in what sense we consider our results supportive of our theoretical estimate W_{Kc}: since we expect that kinetic power W_K should grow with better resolution, it is a good news for W_{Kc} that is is higher than W_K observed at current resolution. If, instead, W_K were higher than W_{Kc}, that would testify against W_{Kc}.

In our revised manuscript we attempted to estimate how W_K changes with temporal resolution by analyzing additionally daily and monthly mean MERRA and NCAR/NCEP data for W_K (see Section 5.3 and Fig. c1 above). These results indicate that W_{Kc} does indeed represent a plausible upper limit for the kinetic power of convective motions resolved at the scale of about 1 hour. But we do agree with the referee that further analyses are needed to improve reliability of this result.

Section 6, using the results obtained in the previous sections, shows that condensation-driven circulation corresponds to a Carnot cycle with a temperature difference ΔT coinciding with the mean temperature difference between evaporating and condensing water vapor. We believe that this new result is quite specific.

This work evolved from a short technical comment that we made on the work of Laliberté et al.(2015) in February 2015. This comment and the review we received from Science is

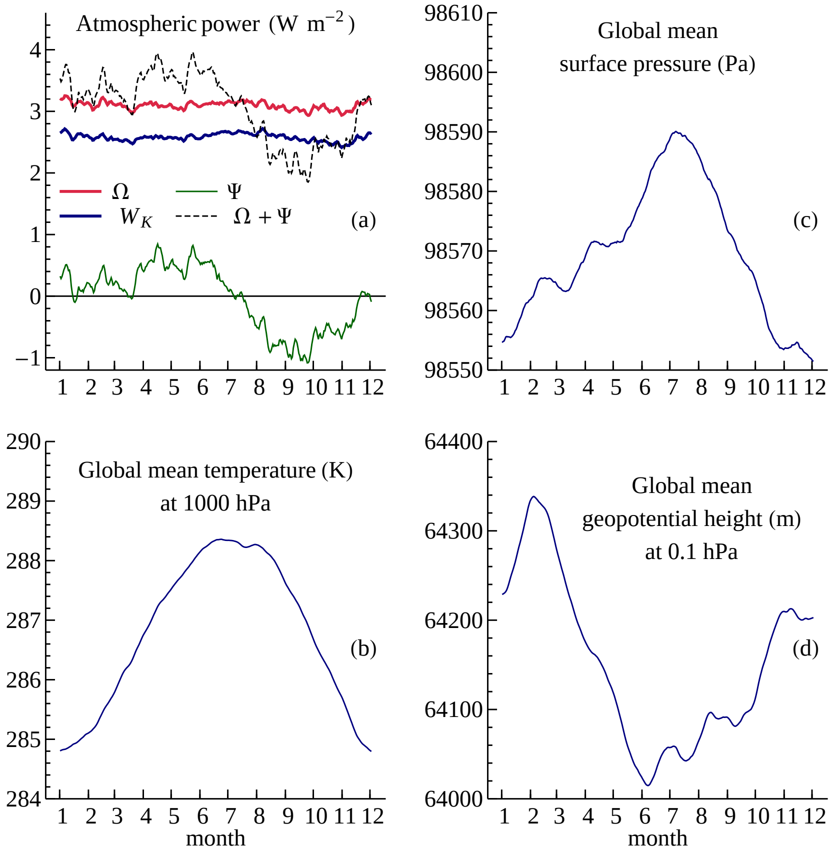

Figure c5: Time series (30-day running mean of daily values for the year 2010) of (a) the global integral of the pressure tendency Ψ, the omega integral Ω and kinetic power W_K (21); global mean surface temperature (b), global mean surface pressure (c) and global mean geopotential height at $p_T = 0.1$ hPa (d). This pressure level moves with vertical velocity w_T of about 300 m in half a year, $w_T \sim 2 \times 10^{-5}$ m s^{-1}, which corresponds to $I_T \sim p_T w_T \sim 10^{-4}$ W m^{-2} $\ll W$. Ticks on the horizontal axes correspond to the 15th day of each month.

available from http://www.bioticregulation.ru/ab.php?id=he. In particular, one referee of this short comment refuted our suggestion that air circulation on Earth can be powered by condensation by noting that the models of a dry atmosphere display the same atmospheric power as does the real atmosphere – hence no need for alternative drivers. Assessments of our work by other anonymous colleagues showed that this idea is common. Thus, in Section 6 we explain why models of dry atmospheres cannot indicate whether or not global atmospheric circulation is condensation-driven.

3 Referee 3 [doi:10.5194/acp-2016-203-RC3]

3.1 Comment 1

The manuscript is poorly written and requires substantial improvement before publication. The authors misrepresent part of their results as a new analysis, while they have been previously discussed in the literature.

Main comments:

1. Appropriation in the main result:

The manuscript states pretty explicitly that the main contribution here is

"Starting from the definition of mechanical work for an ideal gas, we present a novel

derivation linking global wind power to measurable atmospheric parameters. The resulting expression distinguishes three components: the kinetic power associated with horizontal motion, the kinetic power associated with vertical motion and the gravitational power of precipitation."

as it is stated in the abstract. This claim is repeated on multiple occasions. I assume that this specifically refer to the equation (20-22), which the authors claim that "Equations Eqs. (20)-(22) and their derivation have not been previously published."

These equations are presented in Pauluis etal. (2000) (See equations (2), (4), (8) and (10). See also equations (4) and equation (6) of Pauluis and Held (JAS, 2002)). It is very troublesome that the authors fail to mention that equations (20-22) are presented in Pauluis etal. (2000) despite the fact that this pa

The appropriation is not limited to the equations, but extends to some of the arguments presented. For instance, the authors relate the claim

"The meaning is that hydrometeors perform work at the expense of their potential energy. To acquire this energy, a corresponding amount of water vapor must be raised by air parcels. We can also see that WP does not depend on the interaction between the air and the falling hydrometeors. This term would be present in the atmospheric power budget even if hydrometeors were experiencing free fall and did not interact with the air at all (such that no frictional dissipation on hydrometeors occurred)."

This points is made previously (and more clearly) in Pauluis etal. JAS (2000, p. 991):

"The dissipation by precipitation can be thought as proceeding in two steps. First, water is lifted by the atmospheric circulation, increasing its potential energy. Then, during precipitation, the potential energy of condensed water is transferred to the ambient air where it is dissipated by molecular viscosity in the microscopic shear zone around the hydrometeors."

To put it bluntly, the authors are presenting as their own an analysis that was done by others, and in doing so, are misleading their reader.

Response [see also doi:10.5194/acp-2016-203-AC4]:

Presumably there is some misunderstanding involved so we have revised the text clarifying how our results relate to previous work. In particular, we now show that Eqs. 20-22 could not *in principle* be formulated by Pauluis et al. 2000, because their basic assumptions are not consistent with either Eqs. 20-22 or with Eq. 4 of Pauluis and Held 2002. We acknowledge the value in making this claim clear and explicit as it is precisely because Eqs. 20-22 were not published previously that the global gravitational power of precipitation W_P could also not be estimated from re-analyses until now.

We revised the text having added a separate "Section 3.3 Our results compared to Pauluis et al. 2000". Right below Eqs. (20)-(22) we explain why in our view these equations are original. Furthermore, we also explicitly refer the readers to Section 3.3 where these results are compared with Pauluis et al. 2000 by noting: "Equations (20)-(22) and their derivation have not been previously published (see the next section)." Readers can judge our claims for themselves. Reference to Pauluis et al. 2000 is also made already in the revised Introduction: "In Section 3 we discuss how global atmospheric power can be represented as a sum of three distinct physical components. Two components dominate in the atmosphere of Earth: the kinetic power of the wind generated by horizontal pressure gradients and the gravitational power of precipitation generated by the ascending air. We compare our results with the previous assessments of the atmospheric power budget by Pauluis et al. [2000].".

As a separate point, we note that Eqs. (20)-(22) make it clear that W_P can be estimated from the data on air velocity and pressure gradient with no information required about moist processes. As can easily be verified by examining the texts in question, this message is absent from the works cited by the referee (or indeed in any previous publications of which we are aware). To facilitate this comparison we list the equations mentioned by the referee below together with our Eqs. 20-22 from the submitted manuscript.

Pauluis et al (2000), Eqs. (2), (4), (8) and (10), respectively:

$$W_p = \int_\Omega g\rho_c v_{\rm T}, \tag{c7}$$
$$W_p = \int_\Omega g\rho_t w, \tag{c8}$$
$$W_D = \int \bar{\rho} g w \left[\frac{\Theta'}{\overline{\Theta}} + \left(\frac{R_v}{R_d} - 1\right)\frac{\rho_v}{\overline{\rho}} - \frac{\rho_c}{\overline{\rho}}\right], \tag{c9}$$
$$W_{\rm tot} = \int wg\left[\overline{\rho}\frac{\Theta'}{\overline{\Theta}} + \rho_v\frac{R_v}{R_d}\right], \tag{c10}$$

where $\rho_t = \rho_c + \rho_v$.

Pauluis and Held (2002), Eqs. (4) and (6), respectively:

$$W = \int_\Omega p\partial_i V_i, \tag{c11}$$
$$D_p = \int_\Omega g\rho_c V_T = \int_\Omega \rho q_t g w, \tag{c12}$$

where V_i is the ith component of the velocity, $\partial_i = \partial/\partial x_i$ is the partial derivative in the i direction, ρ_c is the mass of falling hydrometeors per unit volume, q_t is mass of total water per unit mass of moist air, V_T is the terminal velocity of the falling hydrometeors, and w is the vertical velocity of the air.

Equations (20)-(22):

$$W = -\frac{1}{\mathcal{S}}\int_\mathcal{V} \mathbf{v}\cdot\nabla p d\mathcal{V} \equiv W_K + W_P, \tag{c13}$$
$$W_K \equiv -\frac{1}{\mathcal{S}}\int_\mathcal{V} (\mathbf{u}\cdot\nabla p)d\mathcal{V} + W_c \approx -\frac{1}{\mathcal{S}}\int_\mathcal{V} \mathbf{u}\cdot\nabla p d\mathcal{V},\ \ W_c \equiv -\frac{1}{\mathcal{S}}\int_\mathcal{V} \rho_c(\mathbf{w}\cdot\mathbf{g})d\mathcal{V}, \tag{c14}$$
$$W_P \equiv -\frac{1}{\mathcal{S}}\int_\mathcal{V} \rho\mathbf{w}\cdot\mathbf{g}d\mathcal{V} = -\frac{1}{\mathcal{S}}\int_\mathcal{V} gz\dot{\rho}d\mathcal{V} = Pg\mathcal{H}_P,\ \ P \equiv -\frac{1}{\mathcal{S}}\int_{z>0} \dot{\rho}d\mathcal{V}. \tag{c15}$$

Note that $\rho = \rho_d + \rho_v \neq \rho q_t$; $\mathbf{v} = \mathbf{u} + \mathbf{w}$ is air velocity (horizontal and vertical).

3.2 Comment 2

2. Discussion of Laliberte etal. (2015)

The discussion of Laliberte etal. (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 etal.

Response [see also doi:10.5194/acp-2016-203-AC5]:

We agree with the referee that Laliberte et al. assume a steady atmosphere. 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 Appendix C (now D) where it is shown that this term may be considerable on a seasonal scale thus influencing estimates of global atmospheric power, see Fig. 10a in the revised manuscript or Fig. c5a above. As discussed later in the paper (see revised Section 5.2), 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. To shorten the presentation, we removed Section 3.2 from the revised paper, as all the necessary information is contained in Appendix D.

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.*

Response:

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, see line 28 on p. 11.

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 etal. 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.*

Response:

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 air as a whole

$$\frac{\partial \rho}{\partial t} + \nabla \cdot (\rho \mathbf{v}) = \dot{\rho} \tag{c16}$$

together with the continuity equation for water vapor

$$\frac{\partial \rho_v}{\partial t} + \nabla \cdot (\rho_v \mathbf{v}) = \dot{\rho}. \tag{c17}$$

Noting that $q_T \equiv \rho_v/\rho$ (Laliberté et al. [2015] neglect the tiny condensate content) we find from Eqs. (c16) and (c17) that

$$\dot{q_T} = \frac{\dot{\rho}}{\rho}\left(1 - \frac{\rho_v}{\rho}\right). \tag{c18}$$

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 can be assumed in the MERRA database and could also be assumed by Laliberté et al. [2015] (although we see no grounds for such an assumption), is that the vertically integrated continuity equation has negligible sources or sinks, that is $\int \dot{\rho} dz \approx 0$. 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. As we discuss in revised Section 5.2, the barotropic correction to wind velocities used by Laliberté et al. [2015] is not equivalent to setting the sources and sinks 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.

3.3 Comment 3

3. Overal structure:

The paper is poorly constructed. It is mainly three separate studies. Sections 2-4 attempt a theoretical discussion of the issues that mostly reprise previous work. It is unnecessarily confusing. Section 5 is the main 'new' result. The computation done are fairly routine, and the result in line with what we know. The inability of the authors to produce a consistent figure for Wp is distressing and should be better addressed in the revision. Section 6 is a lengthy disgression which is mostly a repeat of the authors previous work.

Response [see also doi:10.5194/acp-2016-203-AC6]:

We believe that the revised paper is now much clearer and presents a coherent theme. We underline that the entire literature on this subject is somewhat confusing and it is the need to identify and examine the inconsistencies in other studies that leads to difficulties. Our revision is attentive to these difficulties (e.g. the different formulations of W).

We note that published approaches to W_P suffer important inconsistencies. Pauluis et al. [2000] estimated, on theoretical grounds, that tropical W_P (between 30N and 30S) should be between 2 and 4 W m^{-2}. Pauluis and Dias [2012] analyzed TRMM data to conclude that tropical W_P is, rather, 1.8 W m^{-2}. Makarieva et al. [2013] likewise on theoretical grounds, suggested that Pauluis et al. [2000] overestimated tropical W_P by around one-hundred percent. Their results led Pauluis and Dias to revise their calculations and publish a revised TRMM-based estimate of 1.5 W m^{-2} for the tropics as a corrigendum to their 2012 work. On the other hand, Makarieva et al. [2013] suggested that global W_P should be around 0.8 W m^{-2}; in our present work we show that the true value is around 1 W m^{-2} and we address the associated uncertainties.

As we discussed in our reply to Comment 2 of Referee 1, we show in the revision that the inconsistency in the estimates of W_P as well as of total power W is an inherent property of the re-analyses. We disagree that this is already known, since we find no estimates of global W_P from re-analyses or otherwise. This is indeed surprising given recent emphasis on the thermodynamic aspects of the water cycle [see, e.g., Pauluis, 2015]. We hope that our revised work brings greater clarity to this matter.

In particular, our results suggest that if Laliberté et al. [2015] used NCAR/NCEP rather than MERRA data for their analysis, they would have obtained a negative value for total atmospheric power (and, hence, W_P). Key to their result is the procedure of zeroing pressure velocity at the surface between the modelling steps. If a different procedure were used, the results would be different as well.

3.4 Recommendation

My recommendation here would be to simplify section 2 and 4, drop section 3 and expand on section 5. Section 6 could be clarified as well.

Response:

We revised Sections 2 and 3 (former 4) to present a coherent overview of the available formulations for W, W_K and W_P and their physical meaning. We followed the recommendation of the referee to expand on section 5 having included more extensive analyses not only of MERRA but also of NCAR/NCEP for monthly, daily and 3-hourly resolution for 1979-2015 with four new figures. We would be willing to clarify Section 6 but since the referee provided no guidelines we just double-checked our messages for consistency.

Regarding our analysis of Laliberté et al. [2015] (former Section 3, now Section 4), we showed in the revised text that the omission of I_h stems from the same reasoning that led to Comments 1 and 3 of Referees 2 and 4 concerning the definition of work. The reason is a misinterpretation of dh/dt (or $d\alpha/dt$, where α is mass-specific volume) as the change per unit time of, respectively, enthalpy and volume per unit mass of a material element (air parcel). This is not correct in the presence of phase transitions, because the parcel's mass is not constant. The revised text clarifies this issue and should reduce future confusion.

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

4.1 Comment 1

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.

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.*

Response:

In the revised Section 1 we list many expressions used by various researchers to refer to global atmospheric power. Currently there is no consistency in terminology. This situation may reflect some confusion, which, as we show in our work, surrounds the definition and estimate of the power of atmospheric circulation in a moist atmosphere. In our work we chose the word "power" because it is work per unit time, while our focus is on estimating the work output of the atmospheric circulation – in agreement with the thermodynamic definition of work. We discuss the physical meaning of all the terms of the atmospheric power budget in the revised Section 3 right below Eqs. (20)-(22) on p. 8.

4.2 Comment 2

it 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.

Response [see also doi:10.5194/acp-2016-203-AC6]:

The referee's account of the work of Laliberté et al. [2015] appears to be a misunderstanding. There are three relevant quantities: the power of a Carnot cycle W_C, the kinetic atmospheric power W_K and the total atmospheric power W. The focus of Pauluis et al. [2000] was indeed to show that W_K is lower than W because, using the referee's words, solar power is "lifting water vapour against the gravity field, part of which is then removed through precipitation, leaving only the residual to power the atmospheric circulation". However, Pauluis [2011] advanced a different statement: that total power W is lower than Carnot power W_C because of the irreversible processes like water vapor diffusion. Laliberté et al. [2015] were likewise concerned about why W is smaller than W_C and did not assess the gravitational power of precipitation.

This misunderstanding might have stemmed from the comment of Pauluis [2015] on the work of Laliberté et al. [2015], where the two statements, $W_K < W$ and $W < W_C$, became mixed. To provide some context, an ideal atmospheric Carnot cycle consuming heat flux $F = 100$ W m^{-2} at surface temperature $T_{in} = 300$ K and releasing heat at $T_{out} = T_{in} - \Delta T_C$ with $\Delta T_C = 30$ K, would generate kinetic energy at a rate of $W_C = F(\Delta T_C/T_{in}) = 10$ W m^{-2}. Laliberté *et al.* (2015) estimated total atmospheric power W at around 4 W m^{-2}. Comparing their result with W_C, Pauluis [2015] noted that "estimates for the rate of kinetic energy production by atmospheric motions are about half this figure". Here confusion has apparently arisen between total atmospheric power W and kinetic power W_K (because Laliberté et al. [2015] assessed only W but not W_K, the latter being about 2.5 W m^{-2}, i.e. a quarter rather than half of W_C). Indeed, Pauluis [2015] continued that "the difference is very likely due to Earth's hydrological cycle, which reduces the production of kinetic energy in two ways", one of which is the gravitational power of precipitation W_P and the other is the irreversible diffusion processes. However, from our Eqs. (20)-(22), W_P reduces W_K compared to W but it does not reduce W compared to W_C, since $W_K + W_P = W < W_C$.

4.3 Comment 3

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

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

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.

Response [see also doi:10.5194/acp-2016-203-AC3]:

The referee uses the same incorrect expression for work as Referee 2 in their Comment 1, with the same resulting discrepancies from our derivation. Total power is not equal to $W_{IV} \equiv (1/\mathcal{S}) \int_{\mathcal{V}} p(d\alpha/dt)\rho d\mathcal{V}$. This is clarified in the revised Section 2, see Eq. 11. Moreover, since $\nabla \cdot (\rho \mathbf{v}) = \dot{\rho} \neq 0$, the second equation of the referee contradicts the first one.

We note that our four referees appear to disagree on how the correct expression for atmospheric power W should look like. Referee 1 (and implicitly Referee 3) agree with our Eq. (7), which shows that W does not explicitly depend on the rate of phase transitions. Meanwhile, Referees 2 and 4 opine, respectively, that our results either *appear to be wrong* or are *unrelated to the standard view* suggesting two derivations of their own. However, as we have discussed, both derivations assume that work per unit mass is equal to $pd\alpha$, which is not a valid assumption in the presence of phase transitions. The resulting expressions contradict not only our Eq. (7) but also the identical Eq. (4) of Pauluis and Held (2002) endorsed by Referee 3. We hope that our revised text clarifies this topic.

4.4 Comment 4

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 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 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.

Response [see also doi:10.5194/acp-2016-203-AC5]:

As was stated in our manuscript (see Eq. 5 on p. 3) and is perhaps better emphasized in our revision (first paragraph in Section 2, p. 3 and lines 16-18 on p. 4), 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.

In the revised Section 4 we explain the physical meaning of this result (see Eqs. 33 and 34). The integral of dh/dt over mass is not zero simply because it does not represent changes of enthalpy per unit mass of a material element.

4.5 Comment 5

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

$$\rho \frac{D}{Dt}\frac{u^2}{2} + u \cdot \nabla p = \rho F \cdot u \tag{c19}$$

so that in equilibrium

$$\int_V u \cdot \nabla p dV = Friction, \tag{c20}$$

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

$$\int_V \rho g w dV = 0 \tag{c21}$$

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

$$\frac{d(GPE)}{dt}\Big|_{gas} = \underbrace{\int_V \rho g w dV}_{-SW_P} + GAS\ DESTRUCTION = 0, \tag{c22}$$

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.*

Response [see also doi:10.5194/acp-2016-203-AC6]:

The decomposition of total atmospheric power W into the kinetic power of winds W_K and the gravitational power of precipitation W_P is useful in several ways. First, as we discuss below in response to Comment 6 of Referee 4 (see also revised Section 5.1, last paragraph on p. 16), W_P and W_K in re-analyses are characterized by substantially different uncertainties, so it is useful to keep a separate record for them. Second, W_P can be estimated independently from wind velocities using observed precipitation; this information can be used to constrain vertical velocities. Third, since thermodynamics constrains total power W and not kinetic power W_K or W_P separately, it is necessary to clearly differentiate between W, W_K and W_P from a theoretical viewpoint. Distinguishing these components can help avoid confusions when comparing results from different studies (see also above our reply to Comment 2 of Referee 4). For example, given the modern concern about renewable energy resources it is necessary to understand that the so-called "wind power" [Marvel et al., 2013] as well as the river hydropower (which is part of W_P) are not the total power of the atmosphere.

We also note that in the presence of condensate the vertical distribution of gaseous air is not hydrostatic; the condensate loading term describes the generation of kinetic energy of the vertical air motions and is not zero. Furthermore, the integral of the left-hand part of the referee's equation (c19) is not zero in the presence of phase transitions, so Eq. (c20) does not hold. This is discussed in detail in the revised section 3, see p. 8 and Eq. 29 on p. 10.

4.6 Comment 6

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.

Response [see also doi:10.5194/acp-2016-203-AC6]:

In the meteorological literature it is common to refer to the re-analyses data as to *observations* using which models outputs could be verified – see, for example, the study of Boer and Lambert [2008] devoted to the atmospheric energy cycle. This is because the re-analyses aim to systematize available observations of air pressure, velocity, temperature, humidity etc. in a coherent form. Air pressure and velocity are the basic observational parameters recorded. Likewise, precipitation is directly measured at the surface as well as assessed in the tropical atmosphere with use of satellites (the TRMM mission).

Since vertical velocities are small compared to horizontal velocities, they cannot be derived directly from observations. It is in this sense that the term $\mathbf{u} \cdot \nabla p$ is observable with a good accuracy, while the term $\rho w g$ responsible for the gravitational power of precipitation is not. This latter term can only be derived from observations using additional assumptions. Because of this difference, we estimate W_K with less uncertainty than W_P. These uncertainties are estimated in the revised Section 5.1, see the last paragraph on p. 16.

5 Summary of revisions

Since the original manuscript has undergone a lot of changes, including some restructuring (Section 3 became Section 4 and vice versa), we do not provide a marked PDF with all the changes to avoid confusion. We list the changes made to the manuscript below.

1. Section 1 was revised to include an overview of various formulations of W in the literature.

2. Section 2 was revised following the recommendation of Referee 1 to use an alternative derivation of W. Our original derivation became new Appendix A.

3. Section 3 (former Section 4) was revised by adding new subsections 3.1 (discussing the boundary condition for velocity) and 3.3 discussing previous work by Pauluis et al. The text of Section 3.2 remained relatively intact.

4. Section 4 (former Section 3) containing analysis of Laliberté et al. [2015] was shortened following the recommendation of Referee 2. Additionally, it was explained how the omission of the enthalpy integral is related to the incorrect definition W_{IV} for W in a moist atmosphere.

5. Section 5 was considerably revised following the recommendations of Referees 1, 2 and 3 to include new analyses. Three new figures were added.

6. Section 6 was slightly shortened and double-checked for clarity.

7. New Appendix A contains the derivation of W for ideal gas.

8. Appendix B (former A) remains largely intact, but we added one paragraph on p. 27 to discuss the uncertainties of our precipitation-based W_P estimate – as recommended by Referee 1.

9. Appendix C (former B) remains largely intact, but we added a new subsection C3 with a new figure explaining the impact of boundary values of ω and $\mathbf{u} \cdot \nabla p$ on the resulting estimates of W, W_K and W_P.

10. Appendix D remained intact.

11. The abstract was modified to reflect the revisions made.

References

G. J. Boer and S. Lambert. The energy cycle in atmospheric models. *Climate Dynamics*, 30:371–390, 2008. doi: 10.1007/s00382-007-0303-4.

J. Huang and M. B. McElroy. A 32-year perspective on the origin of wind energy in a warming climate. *Renewable Energy*, 77:482–492, 2015. doi: 10.1016/j.renene.2014.12.045.

S. Kang and J. Ahn. Global energy and water balances in the latest reanalyses. *Asia-Pacific J. Atmos. Sci.*, 51:293–302, 2015. doi: 10.1007/s13143-015-0079-0.

F. Laliberté, J. Zika, L. Mudryk, P. J. Kushner, J. Kjellsson, and K. Döös. Constrained work output of the moist atmospheric heat engine in a warming climate. *Science*, 347:540–543, 2015. doi: 10.1126/science.1257103.

A. M. Makarieva, V. G. Gorshkov, A. V. Nefiodov, D. Sheil, A. D. Nobre, P. Bunyard, and B.-L. Li. The key physical parameters governing frictional dissipation in a precipitating atmosphere. *J. Atmos. Sci.*, 70:2916–2929, 2013. doi: 10.1175/JAS-D-12-0231.1.

K. Marvel, B. Kravitz, and K. Caldeira. Geophysical limits to global wind power. *Nature Climate Change*, 3:118–121, 2013. doi: 10.1038/nclimate1683.

O. Pauluis. Water vapor and mechanical work: A comparison of Carnot and steam cycles. *J. Atmos. Sci.*, 68:91–102, 2011. doi: 10.1175/2010JAS3530.1.

O. Pauluis and J. Dias. Satellite estimates of precipitation-induced dissipation in the atmosphere. *Science*, 335:953–956, 2012. doi: 10.1126/science.1215869.

O. Pauluis, V. Balaji, and I. M. Held. Frictional dissipation in a precipitating atmosphere. *J. Atmos. Sci.*, 57:989–994, 2000. doi: 10.1175/1520-0469(2000)057<0989:FDIAPA>2.0.CO;2.

Olivier M. Pauluis. The global engine that could. *Science*, 347:475–476, 2015. doi: 10.1126/science.aaa3681.

Kevin E. Trenberth. Climate diagnostics from global analyses: Conservation of mass in ecmwf analyses. *Journal of Climate*, 4(7):707–722, 1991. doi: 10.1175/1520-0442(1991)004<0707:CDFGAC>2.0.CO;2.