

Reply to comments of Referee 3 and Referee 4 on the revised manuscript **Quantifying the global atmospheric power budget** by A. Makarieva et al.

1. Editor's comments

I now have in hand two reviews of your revised manuscript. Unfortunately, neither of these is positive as they raise fundamental objections with the either the methodology or presentation. Reviewer 1 is concerned with either repetition or inaccurate characterization of Pauluis et al. (2000). Reviewer 2 is sympathetic with the effort and the contribution to the discussion but nonetheless disagrees with the methodological approach.

At this point, I believe any future consideration of the paper needs to be as a new submission. I would suggest that particular care be taken to accurately characterize and cite prior efforts given the field does appear to be contentious. Given there are widely varying views on what is essentially a simple question, I suggest that utmost care be given to making the paper as clear and transparent as possible. Box diagrams have been used in the past to illustrate relationships between APE and KE for example. Point 2 by Reviewer 2 might be helpful in this regard.

Response: We thank the referees and the Editor for their useful comments. During the revision, we have seen as our main task to show how our formulations relate to the equations of motion and continuity (as requested in Comment No. 2 of Referee 4) and to respond to the concerns of Referee 3 by clarifying the difference between our formulations and the formulations of Pauluis et al. (2000) (hereafter PBH).

To clarify the paper's structure, we have re-written the abstract and added a concluding section with a numbered list of the main results. Section 3 has been re-written completely to reflect the following major changes:

- As requested by Referee 4, we have presented an atmospheric power budget derived

from an explicit consideration of the equations of motion and equations of continuity for condensate and gaseous air.

- We performed a scale analysis of all the budget terms and showed that our formulation of the atmospheric power budget $W = W_K + W_P + W_c$ is valid with a significantly better accuracy $((w/u)^2)$ than hydrostatic equilibrium (w/u) . The power budget is illustrated with a new figure (Fig. 1). Our expression for total power $W = W_I$ is exact and universally valid (no assumptions or approximations are made).
- In Section 3.5 we analyzed the derivations of PBH in great detail and explicitly discussed all the equations of PBH listed by Referee 3. A detailed scheme comparing our derivations with those of PBH was added (new Fig. 2).

We show that PBH did not provide a consistent derivation of the atmospheric power budget as their key statement (Eq. 3) was incorrect; this lack of consistency is responsible for the error we identified in the approach of Laliberté et al. (2015), which based on the work of PBH.

Below we respond to the referees' comments in detail. The referees' comments are in italics. References to the changes made to the manuscript are marked in blue. The manuscript on which the referees commented can be found at <http://arxiv.org/abs/1603.03706v2> (note the version number "v2").

2. Referee 3

The authors present an analysis of the work done by the atmospheric circulation. This work is to a large extent an adaptation of the analysis of Pauluis, Balaji and Held (2000, PBH hereafter) to the global atmosphere. In my previous review, I pointed out that some of the results presented by the authors as an original derivation have already been discussed in PBH. The authors have not adressed this issue in their revision. Instead, they added a new

section 3.3 in which they make a number of false claims about PBH. As a result, the revised manuscript is substantially worse than the original submission and should not be published.

Response: We addressed the issue raised by the referee in good faith. In Section 3.3 (Section 3.5 in the present manuscript) we explained why we do not consider the approach of PBH to be consistent and how our own approach and results differ from those of PBH. The referee has apparently misunderstood our criticisms of PBH. To avoid such misunderstandings, we have thoroughly re-written Section 3.3 (new Section 3.5) and added a detailed scheme (new Fig. 2) to explain the difference between the approach of PBH and ours. In brief, while PBH obtained a valid expression for precipitation-related dissipation, they did not derive a general relationship for total atmospheric power as we did, because their key equation (Eq. 3 of PBH) was incorrect. Nor did PBH present a consistent derivation of the atmospheric power budget from an explicit consideration of the continuity equations and the equations of motion with correctly specified boundary conditions. We also showed that this lack of consistency was likely responsible for the error in subsequent analysis of the atmospheric power budget by Laliberté et al. (2015) that built on the work of PBH.

Main comments:

1. Appropriation and misrepresentation of the results of Pauluis, Balaji and Held (2000)

In my previous review, I pointed out that the set of equations (20-22) that discuss the mechanical energy generation by atmospheric motions are extremely similar equations equations 4, 8 and 10 in PBH:

$$W_p = \int_{\Omega} g \rho_t w, \quad (4)$$

$$W_D = \int \bar{\rho} g w \left[\frac{\Theta'}{\bar{\Theta}} + \left(\frac{R_v}{R_d} - 1 \right) \frac{\rho_v}{\bar{\rho}} - \frac{\rho_c}{\bar{\rho}} \right], \quad (8)$$

$$W_{\text{tot}} = \int w g \left[\bar{\rho} \frac{\Theta'}{\bar{\Theta}} + \rho_v \frac{R_v}{R_d} \right], \quad (10)$$

There are two differences between the PBH formulation and that of Makarieva. First,

PBH consider non-hydrostatic flows, in which case the generation of kinetic energy is given by the buoyancy flux (i.e. their equation 8) while Makarieva et al. only consider hydrostatic motion, in which case that the kinetic energy production can be expressed as equation (2) in the manuscript. Second, Makarieva et al. choose to reshuffle the water loading term (W_c) between equation (21) and (22). These are very minor differences, yet, on multiple occasions (see below), including the abstract and conclusions, Makarieva et al. present equations (20-22) as a new and original contribution.

Response: This summary is incorrect. Unlike equation 10 of PBH for W_{tot} , our equation 20 for total atmospheric power, $W = W_I = - \int_V \mathbf{v} \cdot \nabla p dV$ (now Eq. 37), is universally valid. **It neither assumes hydrostatic equilibrium nor depends in any other way on the particular form of the equations of motion.**

This result, $W = W_I$, requires the boundary condition $w_s = 0$ (discussed in detail in Section 3.1). PBH could not obtain $W = W_I$ because one of their basic equations, namely their Eq. 3, contradicts $w_s = 0$ (see new Fig. 2). This contradiction, which is key to our criticisms of PBH, was not mentioned by the referee. That PBH were not aware of $W = W_I$ is consistent with the fact that in a later publication Pauluis (2015) appears to have misinterpreted W_I (total power) for kinetic energy production (W_K).

Furthermore, the formulations for W_{tot} (equation 10) and W_D (equation 8) of PBH are not generally valid for non-hydrostatic flows (as it might appear from the referee's comment). These formulations derive from the model of Lipps and Hemler (1982) and are only valid in the so-called anelastic approximation.

Moreover, Eq. 8 of PBH only approximately describes "the flux of buoyancy" (this equation is valid neglecting the contribution of horizontal pressure differences to buoyancy). As we show in Section 3.5, an exact expression for total power of a buoyancy-driven circulation depends on two variables only ($w, \bar{\rho}$) rather than five ($w, \bar{\rho}, \bar{\Theta}, \Theta', \rho_v$) and in the notations of PBH simply equals $W_{\text{tot}} = \int_{\Omega} wg\bar{\rho}d\Omega$ (cf. eq. 10 above) (see the last two paragraphs in Section 3.5 and the trapezium in new Fig. 2).

To justify their refusal to acknowledge the contribution of PBH, the authors claim that the analysis of Pauluis et al. (2000) is incorrect in several ways:

”Here we compare our results with those of Pauluis et al. (2000) who likewise identified two distinct terms in the atmospheric power budget. Pauluis et al. (2000) were presumably aware of the fact that total atmospheric power is equal to W_{III} , since Eq. (3) was listed by Pauluis and Held (2002) albeit without a derivation or reference. However, as we show below, several inconsistencies in their basic assumptions did not permit obtaining results equivalent to Eqs. (20)-(22).”

(page 9, line 15-18) These so-called inconsistencies however are the direct result of the authors misrepresentation of PBH. Two specific claims are particularly egregious.

A. PBH do not provide any expression for the generation of kinetic energy.

Response: We disagree with this interpretation of our work. We never made this claim. We claimed instead that the expression for the generation of kinetic energy provided by PBH is inconsistent with their own assumptions.

The authors claim that PBH their approach is superior to that of PBH, because the later does not allow for the computation of the generation of kinetic energy. More specifically, they claim ”Instead, Pauluis et al. (2000) assumed that total mechanical work by resolved eddies W_{tot} is equal to the sum of the frictional dissipation associated with convective and boundary-layer turbulence W_D and the total dissipation rate due to precipitation W_P :...Since no general specification for turbulent processes exists, this formulation per se, unlike Eqs. (20)-(22), cannot guide an assessment of W_{tot} from observations.” (page 10, line 12-17)

Contrarily to the authors’ claim, PBH provide an explicit expression for the generation of kinetic energy (PBH equation 8) and total work (PBH equation 10). Equation (8) is clearly introduced as the rate of generation of kinetic energy: ”The kinetic energy generated on the scales resolved by the model is equal to the upward buoyancy flux”. Similarly equation 10 is clearly introduced as ” W_{tot} is the total mechanical work by the resolved eddies”. Both equation (8) and equation (10) in PBH were mentioned explicitly in my previous review, and

the authors appear to have deliberately chosen to ignore this part of PBH.

Response: Rather than ignoring equation (8) and equation (10) of PBH, immediately after the passage quoted by the referee we explained how PBH could have obtained their results **in their most general form** from the equations of motions. The referee’s misinterpretation of our work appears to have resulted from us having attributed to PBH more than they actually did. The formulations derived by PBH for total power and kinetic power are only valid for the particular model they considered under an additional assumption $\dot{K} = 0$ and clarification of the velocity notations and the form of the continuity equations.

On page 10, lines 17-18, we wrote: “Since no general specification for turbulent processes exists, this formulation per se, unlike Eqs. (20)-(22), cannot guide an assessment of W_{tot} from observations. **However, W_D can be retrieved from the equation of motion as the volume integral of $-\mathbf{F} \cdot \mathbf{v}$, where \mathbf{F} is turbulent friction force (cf. Lorenz, 1967, Eq. 101).**”

The last sentence, which concludes the paragraph, was omitted by the referee when quoting our text. The paragraph was followed by Eqs. (28)-(29), which explained how PBH could have obtained their formulation for W_D . We showed that W_D can be defined *only* by an explicit consideration of the equations of motions, which PBH did not present. Equation (29) gave the most general form of W_D under the assumption made by PBH (droplets having no acceleration); equation (8) of PBH is an approximation of Eq. (28).

Likewise, equations (30) corresponded to equations (10) and (4) of PBH under the assumption that $W_{\text{tot}} = W_I$ (which, as we discussed above, PBH did not show). Equations (30) highlight the remaining additional differences. First, PBH ignored \dot{K} , the dissipation of kinetic energy the droplets possess as they form. We note that \dot{K} is not equal to dissipation of the kinetic energy of droplets as they hit the ground (see footnote 1 on p. 991 in PBH). Second, PBH incorrectly specified the relationship between precipitation path length H_P and precipitation-related dissipation W_p (see below response to comment B).

In the revised text, as requested by Referee 4, we present a more detailed discussion of [how the atmospheric power budget relates to the equations of motion in Sections 3.2, 3.3 and](#)

3.4. Using these results, in Section 3.5 we explain more clearly how the derivations of PBH are affected by the particular form of the equations of motions and what their underlying assumptions are.

B. PBH do not compute the precipitation-induced dissipation and rely solely on a scaling argument to estimate W_p .

Response: We disagree with this interpretation of our work. We never made this claim. We claimed that PBH did not correctly specify the relationship between the precipitation-induced dissipation and precipitation path length and condensation rate.

The authors implies that PBH never compute the precipitation-induced dissipation, and instead use an incorrect approximation, e.g.

”Rather than using Eq. (18) and the continuity equation for water vapor similar to what was done in Eq. (19), Pauluis et al. (2000) further assumed that W_P is proportional to the precipitation rate P at the surface...”

Section 2 of PBH offers some scaling argument to offer a rough estimate of the precipitation-induced dissipation based on the precipitation scale height. Section 3 however compute the precipitatio-induced dissipation in a numerical model, e.g. ”In this simulation, the dissipation rate associated with precipitation W_p is $3.6 W m^{-2}$. Given the model’s precipitation rate, the corresponding precipitation path-length is H_f 9.3 km.” (p. 992) This makes it clear that the precipitation-induced dissipation (i.e. equation 2 in PBH) is used to compute H_f , not the other way round, as falsely claimed in Makarieva etal. In a personal communication, Prof. Pauluis confirmed that he indeed computed W_p explitlely for their paper.

Response: We agree that PBH estimated W_p from their Eq. 4 (see above), where $\rho_t = \rho_v + \rho_c$. This is a correct formulation of W_p (but not the gravitational power of precipitation $W_P \neq W_p$). However, this formulation, $W_p = \int_{\Omega} (\rho_v + \rho_c) w g d\Omega$, does not clarify how atmospheric circulation generates potential energy of droplets. Indeed, PBH suggested that water is first *”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.” However, vertical air velocity w in equation 4 of PBH can be either positive or negative; for example, for dry air $\int_{\Omega} \rho_d w g d\Omega = 0$ is unrelated to potential energy. So why Eq. 4 should reflect the potential energy of droplets remains unclear.

Potential energy per unit mass of droplets at height z is gz . To reveal the link between W_p and potential energy of condensate one needs to introduce the mean precipitation path length $H_P \equiv -\int \dot{\rho} z d\mathcal{V} / P$, where $P \equiv -\int_{z>0} \dot{\rho} d\mathcal{V}$ is global precipitation at the ground $z = 0$. In this case, since $\int \rho_v w g d\mathcal{V} = -\int \dot{\rho} g z d\mathcal{V}$ (which PBH did not show but it is shown in our revised Sections 3.2 and 3.3), we have $W_p = P g H_P + W_c \neq P g H_P$, where $W_c = \int \rho_c w g d\mathcal{V}$. We emphasize that in this relationship H_P is not a *rough scaling parameter* but a strictly defined variable. Since it depends on condensation rate $\dot{\rho}$, H_P can be estimated from the observed spatial distribution of absolute humidity.

PBH introduced a variable H_f , which they referred to as the precipitation path length but did not express it via condensation rate $\dot{\rho}$. However, PBH attempted to estimate their H_f from absolute humidity, claiming at the same time that $W_p = P g H_f$.

If H_f of PBH is indeed the mean precipitation path length, $H_f = H_P$, the statement of PBH that $W_p = P g H_f$ is incorrect (the correct relationship is $W_p - W_c = P g H_f$). If H_f is not the mean precipitation path length, $H_f \neq H_P$, the question arises what H_f is, how it relates to gravity and how it can be independently retrieved from observations if not from absolute humidity. In either case, PBH did not correctly specify the relationship between precipitation path length, condensation rate and thus the gravitational power of precipitation and their W_p . This relationship was specified in our Eq. (22) (in the revised text it is Eq. 39).

Minor comments: Here are some extracts from the manuscript in which the authors present the derivation of equation (20-22) as an important and original contribution, without

acknowledging any previous work on the issue. Page 1, line 5-8

”We discuss how estimates of W are affected by these differences. Starting from the thermodynamic definition of mechanical work, we present a novel derivation linking global atmospheric power to measurable atmospheric parameters. The resulting formulation distinguishes three components of W : the kinetic power associated with horizontal motion, the the kinetic power associated with vertical motion and the gravitational power of precipitation (W_P).”

Response: We consider our *derivation* to be novel precisely because it starts from the definition of mechanical work ($W = W_{III}$). Then, using the boundary condition $w_s = 0$, we obtain for total atmospheric power $W = W_I$. Using the equations of motions and continuity, we then decompose W into physically distinct terms (kinetic energy production and the gravitational power of precipitation). In contrast, PBH attempt to assemble the atmospheric power budget as a sum of precipitation-related dissipation and kinetic energy dissipation. In so doing, they do not demonstrate that their resulting expression for W_{tot} is consistent with the thermodynamic definition of mechanical work. (To do so would require adopting $w_s = 0$, which would contradict key equation (3) in the approach of PBH.)

In the revised text we added a scheme (new Fig. 2) illustrating our approach and all the assumptions we use in comparison with the approach and the assumptions of PBH.

We consider our resulting *formulation* to be novel as well. One reason is that it highlights the kinetic power associated with horizontal motion as the key constituent of the atmospheric power budget. This is essential for numerical assessments of atmospheric power budget, since only this term (and not the gravitational power of precipitation) turns out to be consistent among the re-analyses, and we explain why. In contrast, the formulations of PBH (see equations 4, 8 and 10 above) are all dependent on vertical velocity w and do not contain the above message.

We do acknowledge previous work on the issue. Immediately after the sentence quoted by the referee, we stated in the abstract: ”Unlike previous approaches, our formulation allows evaluation of W_P without knowledge of atmospheric moisture or precipitation.” This phrase, probably

unnoticed by the referee, clearly acknowledges that there *are* previous approaches. It also suggests quite unequivocally that these previous approaches allow evaluation of W_P albeit they require the knowledge of atmospheric moisture and precipitation. [In the revised text, the approach of PBH is considered in great detail in Section 3.5.](#)

page 3, line 13-14

”To our knowledge, these distinctions have not been previously highlighted and examined. Which definition, if any, represents the ”true power” of a moist atmosphere and is consistent with the thermodynamic interpretation of work?”

Response: If the referee is aware of a publication where the distinctions between the four definitions of atmospheric power, W_I , W_{II} , W_{III} , W_{IV} , when applied to a moist atmosphere were highlighted and examined, we would appreciate a citation and will revise our text accordingly. PBH did not perform such an analysis nor did they show that their formulation of W_{tot} is consistent with the thermodynamic definition of work (see our reply to Comment 1 above).

page 8, line 5-6 ”Equations (20)-(22) and their derivation have not been previously published (see the next section). These equations clarify the physical meaning of the atmospheric power budget.”

Response: Editors (and readers) often expect the authors to highlight what the authors consider novel in their work, and we did so in the first two versions of our manuscript. We have acknowledged all relevant preceding work of which we are aware and gave our reasons as to why consider our results to be distinct and, hence, novel. However, it is ultimately up to the scientific community to decide to what degree our formulations and their derivation are novel and valuable. We have removed all mentions of novelty from the revised text and let the readers to decide for themselves.

3. Referee 4

Summary and recommendation *The originally submitted version of this paper attracted somewhat divergent comments from a number of a reviewers, so that it was somewhat of a challenge to produce a revised version that would satisfy everyone. While I am not necessarily satisfied with the authors' response to my own comments, I acknowledge that they have made a decent attempt at improving the clarity of their manuscript. Moreover, I also recognise that the issue addressed by the authors, namely whether some constraints on the atmospheric energy budget can be obtained from considering the impact of condensation/evaporation on the divergence of the dry air+water vapour velocity, has not been really addressed previously, and hence that the authors should receive credit for it and their study published. Although I believe that the paper could be made considerably simpler and tidier, the paper is probably readable enough as it is for the reader to form an opinion of whether to believe the results or not.*

Response: We thank the referee for this overall positive evaluation of our work as well as for constructive critical comments. Should any specific recommendations be provided towards making our text simpler and tidier, we are ready to follow them.

While I believe that the theoretical developments proposed by the authors are not mathematically or physically self-consistent, they are nevertheless probably consistent with the current assumptions forming the basis for the equations solved by the MERRA system, and therefore probably acceptable for the present paper. Hopefully, the present study can stimulate discussion on these issues. One of the main consistency issue I have relates to assumption that the water vapour velocity coincides with the dry air velocity. While this assumption is commonly made, and underlies the present study, it seems nevertheless inconsistent with the different continuity equations satisfied by each component, namely:

$$\frac{\partial(\varepsilon_g \rho_d)}{\partial t} + \nabla \cdot (\varepsilon_g \rho_d \mathbf{v}_d) = 0, \quad \frac{\partial(\varepsilon_g \rho_v)}{\partial t} + \nabla \cdot (\varepsilon_g \rho_v \mathbf{v}_v) = -\dot{m}_w$$

where $\varepsilon_g = V_g/V$ is the volume fraction occupied by the gases (dry air + water vapour), \mathbf{v}_d

and \mathbf{v}_d the dry air and water vapour velocities respectively, $\rho_d = m_d/V_g$ and $\rho_v = m_v/V_g$ the densities of the dry air and water vapour defined relative to the gas volume V_g , and \dot{m}_v the condensation rate of water vapour into condensate. While molecular collisions may act to reduce the velocity difference $\mathbf{v}_d - \mathbf{v}_v$, condensation seems to act as a mechanism for increasing this velocity difference. Physically, one may argue that condensation only really directly affects only the water vapour velocity \mathbf{v}_v , whose momentum $\varepsilon_g \rho_v \mathbf{v}_v$ is generally regarded as small relative to dry air momentum $\varepsilon_g \rho_d \mathbf{v}_d$. It seems to me, therefore, that by assuming $\mathbf{v}_v = \mathbf{v}_d = \mathbf{v}$, one is now saying that condensation affects the full velocity, which might exaggerate the effect. This probably needs to be discussed and examined at a later stage possibly.

Response: Condensation does not affect the velocity of water vapor molecules; it affects their interaction. During condensation, collisions experienced by condensing molecules of vapor are no longer elastic (as they are for molecules of ideal gases). In contrast to an ideal gas, condensing vapor molecules once they collide remain together forming the condensate particle. Solely owing to the presence of these non-elastic collisions there is a non-zero component of mean velocity of water vapor molecules perpendicular to the liquid surface of the droplet. Otherwise actual velocities of all molecules, water vapor as well as dry air, remain as they were in the absence of condensation: there are simply fewer water vapor molecules going out of the droplet than those coming in. This mean non-zero velocity is manifested at a distance of about one free path length from the droplet surface. It does not have any impact on the macroscopic velocity of air parcels – the latter, in the atmospheric context, is defined at a scale of greater than a few hundred meters. [In our manuscript we have emphasized that atmospheric power \(work per unit time\) is a scale-specific phenomenon and that attempts to calculate macroscopic work on the molecular diffusion scale expectedly produces non-physical results \[see revised Section 3.1\].](#)

Apart from consistency issues, I have a number of additional comments or suggestions

for further clarification.

Response: We aim to present our work as consistently as possible, so any specific indications as to what else the referee finds mathematically or physically inconsistent in our theoretical developments or in the MERRA re-analysis that we used would be greatly appreciated. At the same time we note that any effort to realistically describe the atmosphere involves a number of approximations. Physics research to a large degree consists in making correct approximations. Approximations are not by definition inconsistencies unless they are shown to yield considerable errors. For example, all global circulation models treat water vapor as an ideal gas, thus neglecting $\varepsilon_g = V_g/V$ (the volume fraction occupied by the gases). While one can rightfully point out that this assumption is generally incorrect, since water vapor in fact condenses, such a remark would not automatically make all GCMs physically and/or mathematically inconsistent. Another example: we have argued in our work that the formulations of Laliberté et al. (2015) are physically inconsistent, since they neglect a major term in the atmospheric power budget they sought to assess. To this end, we estimated the neglected term and showed that it is indeed large. Had this neglected term been always negligible, our criticisms of Laliberté et al. (2015) would have been largely irrelevant even if formally true. We note that in this sense none of our own results has been so far shown to be inconsistent.

Main comments

1. *The authors review a number of possible definitions for atmospheric power, which are all mathematically equivalent in absence of condensation, but they fail to clearly emphasise that this requires assuming the atmosphere to be hydrostatic. I am pointing this out, because in a few places in their manuscript, the authors mention processes occurring at close the speed of sound, which require representing non-hydrostatic and compressible processes. As a result, they should discuss what is (are) the definitions of atmospheric power that is (are) also valid for a non-hydrostatic atmosphere.*

Response: In the Introduction we discuss four definitions of atmospheric power used for a dry atmosphere. Among them, only W_{II} assumes hydrostatic equilibrium; the other three – W_I , W_{III} and W_{IV} are universally valid, for hydrostatic and nonhydrostatic dry atmospheres. Likewise, our result that for a moist atmosphere $W = W_I = W_{III}$ is universally valid. It does not assume hydrostatic equilibrium. W_{IV} is not valid for a moist atmosphere at all.

In the revised text the atmospheric power budget is presented for the general case (see our response to the next comment below).

2. *The author claim that some definitions of atmospheric power are correct and that some others are not. While the authors' proposed definition is a plausible one, they do not really justify it other than by invoking their physical intuition. I would expect, however, that the definition of atmospheric power that one should use should be based on the full analysis of energetics, in particular, of the kinetic energy equation. However, the authors never make explicit what they assume the momentum equations to be, and it is unclear what their assumed global energy budget looks like. Physically, one would expect atmospheric power to satisfy a balance of the form*

$$\text{Atmospheric Power} = \text{DISSIPATION}$$

Can they form a closed energy budget? What does the DISSIPATION term include? Does it include viscous dissipation only, or viscous dissipation plus that due to the precipitation drag? Page 9, the authors write: 'The stationary power budget for a hydrostatic atmosphere can be written' followed by Eqs. (20) to (22), but what they write is not a budget, just a decomposition of W . They do not tell what would balance W in the real atmosphere.

Response: We have completely re-written Section 3.2 – now Sections 3.2, 3.3 and 3.4 – and presented a detailed derivation of the atmospheric power budget from an explicit consideration of the continuity equations for air and condensate and the equations of motion with explicitly specified interaction between air and condensate. A new figure (Fig. 1) was

added to illustrate the power budget for different formulations of the interaction between air and condensate. A scale analysis of all the terms in the atmospheric power budget was performed for horizontal scale of the order of 100 km corresponding to spatial resolution of the MERRA dataset. It was shown that in our decomposition of total power W into W_K , W_P and W_c , the kinetic power term W_K equals dissipation of kinetic energy of air with an accuracy of $(w/u)^2$, where w and u are the vertical and horizontal air velocities.

3. As in my previous comments, I dispute the fact the authors' contention that their formula can be estimated in terms of measurable parameters. Of course, the global velocity and pressure fields are measurable, but only in principle. The velocity and pressure fields used by the authors are obtained from a state estimate, which requires combining a numerical dynamical model of the atmosphere with sparse observations. In practice, if one wants to estimate the quantities discussed by the authors one needs to use inferred quantities rather than measured ones.

Response: We agree with the referee that the observations are sparse and that for this reason the MERRA and NCAR/NCEP data are imperfect. This appears to be a general understanding in the meteorological community. Our emphasis on measurability, as we attempted to clarify in our previous response, has a different focus.

Global velocity and pressure fields are measurable, at least in principle, because there are corresponding techniques and instrumentation developed. Had there been no such techniques, velocity and pressure would not have been measurable at all. In contrast, the local value of condensation rate $\dot{\rho}$ is not measurable, since there are currently no instruments that could assess it. Despite the fact $\dot{\rho}$ explicitly enters the power budget of a moist atmosphere (see Eq. 39 in the revised text), we showed nonetheless that this budget can be formulated in terms of *measurable parameters alone* like pressure and velocity.

Furthermore, since the techniques measuring air velocity have a finite resolution, these techniques perform well when velocities are considerable and poorly when they are small.

As far as vertical velocities are often several orders of magnitudes smaller than horizontal velocities, the vertical velocities, while still measurable in principle, are characterized by significantly greater uncertainties than horizontal velocities. Therefore, it is of essence that our formulation of the atmospheric power budget separates the more robust term W_K – kinetic energy production largely determined by horizontal velocity and pressure gradient – from the far less robust W_P , the gravitational power of precipitation, which depends on vertical velocity and, for that reason, should be independently estimated from theoretical considerations and precipitation-based estimates as those developed in Appendix B.

4. *In my previous review, I made the following comment: "Physically, the atmospheric energy budget is best understood by introducing some kind of available enthalpy $a_p = h(\eta; q_t; p) - h_r(\eta; q_t)$, 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 or radiation. The passage from the first term to the second term requires $\nabla(\rho\mathbf{v}) = 0$, and $\rho\mathbf{v}$ 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 \mathbf{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."

The authors seem to have misunderstood the point of my comment, which was raising the point that nowhere in their approach the authors discuss how one might express their expression of atmospheric power in the form of a thermodynamic efficiency times heat input. The authors dodged the issue by responding that I am using the wrong expression for atmospheric power. The same issue arises with the authors' formula. How do we write

$$W_K \approx -\frac{1}{S} \int_V \mathbf{u} \cdot \nabla p dV = \frac{T_{in} - T_{out}}{T_{in}} Q_{in}$$

in their approach? I can accept that this is not what the authors aim to do, but they could at least review some of the work that has been pursuing such an approach, e.g., Tailleux, ARFM, 2013. How to estimate the thermodynamic efficiency of the atmospheric heat engine seems to be an important question, which the authors do not address in their study.

As a step to that end, I would find it useful if the authors could provide a decomposition of their expression for W_K as the sum of a positive and negative term

$$W_K \approx -\frac{1}{S} \int_V \mathbf{u} \cdot \nabla p dV = -\int_1 S \int_{V+} \mathbf{u} \cdot \nabla p dV - \frac{1}{S} \int_{V-} \mathbf{u} \cdot \nabla p dV$$

where $V+$ (resp $V-$) are the part of the volume where $\mathbf{u} \cdot \nabla p > 0$ (resp $\mathbf{u} \cdot \nabla p < 0$). Physically, this idea was previously discussed in Tailleux (GRL, 2010, 'Entropy versus APE production: On the buoyancy power input in the oceans energy cycle) in the oceanic case, and the aim is to separate the total work into a part that corresponds to a production of kinetic energy, the other part corresponds to a dissipation of kinetic energy. That should be easy given that they have plotted the term in their Fig. 3.

Response: We agree with the referee that "how to estimate the thermodynamic efficiency of the atmospheric heat engine seems to be an important question". However, as the referee himself appears to agree, it is not the goal of our study. Our study focuses on finding a proper formulation for atmospheric work output that would be valid in the presence of phase transitions, as well as on illustrating how such a formulation could yield quantitative estimates from the available atmospheric data. To formulate work output is a necessary first step before the thermodynamic efficiency could be defined let alone estimated. Given

the widely spread confusions surrounding even the definition of atmospheric work, this task alone has required discussing many caveats and controversies, in particular, showing that using pdV for local work is in any case incorrect ($W \neq W_{IV}$). How to formulate thermodynamic efficiency is not less controversial (see, e.g., Makarieva et al. (2010)); this could well become a topic of a separate paper. We leave this topic to future studies.

We quote the work of Tailleux (2010) and Tailleux (2015) in a different context in the revised text. Regarding the decomposition of the expression of W_K as a sum of positive and negative terms, this was done in our recent publication, see Makarieva et al. (2017) (in press, currently available at <https://arxiv.org/abs/1505.02679>). The caveat here is that production of kinetic energy and work output of air parcels (or volumes of oceanic water in the consideration of Tailleux (2010)) is not the same; as we discuss in the Tellus paper, they can locally have different signs. [In the revised text, we briefly mention these issues in Section 5.1; however, a detailed consideration of these notions is beyond the scope of our present paper.](#)

5. Bottom of Page 6, top of page 7. I am surprised by the authors' statement that using the boundary condition $w_s = E$ at the surface would result in unphysical effects. Can the authors explain how they write the budget of water vapour in the atmosphere mathematically without a source term to represent the source of mass due to evaporation?

Response: [In the revised text we discuss the incorrect boundary condition \$\rho_{vs}w_s = E\$ in considerable detail in Section 3.1. As we write in the Summary of main results, item 3: "It is shown that assuming \$w_s \neq 0\$ results in errors, whereby the estimated atmospheric power may exceed the incoming solar radiation. A mathematically and physically consistent approach requires putting \$w_s = 0\$ and formulating the surface flux of water vapor \(evaporation\) as a point source \(Dirac's delta function\) at \$z = 0\$ and not as a vertical flux of water vapor \$\rho_{vs}w_s\$ with \$w_s \neq 0\$ \(see Section 3.1\)."](#)

6. Equation 17, page 7. The authors assert that in presence of condensate, we have

$$\nabla_z p = (\rho + \rho_c)\mathbf{g}$$

Where does that come from? Can the authors prove it from first principles? Given that condensate is unstable in the atmosphere, and is usually assume to fall at its terminal velocity, I find it hard to accept that this can be true. Is it compatible with formulation of the momentum equations derived by previous authors, such as Bannon or Ooyama?

Response: Yes this formulation of hydrostatic equilibrium is common in atmospheric modelling, see, e.g., § 26.3 "Governing equations" of Satoh (2014). In Section 3.3 of our revised paper it is shown which terms in the vertical equation of motion have to be neglected to obtain this equation. Note that the momentum equations developed by Ooyama (2001) and Bannon (2002) are incompatible with each other, with Bannon referring to some terms in Ooyama's formulation as "spurious". Despite both authors aimed to derive their formulations from the first principles but obtained contradictory results, in the fifteen years that followed since the two publications no attempts were made in the literature to clarify this discrepancy and establish which formulation is correct. (See Makarieva et al. (2013) for our own view.)

7. The expression for W_c does not make sense to me. Indeed, the authors write

$$W_c = -\frac{1}{S} \int_V \rho_c \mathbf{w} \cdot \mathbf{g} dV$$

I do not understand why the condensate should fall at the velocity characterising the gaseous component, not its own velocity, which is usually assumed to be the terminal velocity.

Response: The condensate is not falling with velocity of the gaseous component. Term W_c represents work per unit time of the force – weight of droplets $\rho_c g$ – exerted by the droplets on the air. This force is exerted on a unit air volume; thus its work per unit time is equal to the product of the force and air velocity. Terminal velocity is usually understood as the velocity \mathbf{w}_T of the condensate relative to the velocity \mathbf{v} of air, not the actual velocity $\mathbf{v}_c = \mathbf{v} + \mathbf{w}_T$ of the condensate. Section 3.3 in the revised text clarifies the relationships between these velocities and how they enter the equations of motion in considerable detail.

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