

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

Anastassia M. Makarieva et al.

ammakarieva@gmail.com

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Some referees opined that our article appears lengthy and/or confusing.¹ We admit that, despite our continuing efforts to make it as clear as possible, our text is not easy reading. At the same time, we feel that we are not entirely to blame. Since a prerequisite to resolving confusions is to describe them, a confusing field with many subtleties cannot be presented in a brief and straightforward manner. When readers are unaware of the controversies surrounding the formulation of atmospheric power and are suddenly exposed to them while reading our article, they may attribute some of their resulting confusion to our presentation.

¹<http://dx.doi.org/10.5194/acp-2016-203-RC1> p. C1, <http://dx.doi.org/10.5194/acp-2016-203-RC2> p. C4, <http://dx.doi.org/10.5194/acp-2017-17-RC1> p. C1, <http://dx.doi.org/10.5194/acp-2017-17-RC2> p. C2

C1

Our interest in atmospheric power grew out of the idea that we can use its magnitude to test predictions of the condensation-induced dynamics – a theoretical approach we have been developing for the last few years. At first we had only a rough idea of how the atmospheric power could be formulated for a moist atmosphere. But, since it was nearly seventy years ago that Lorenz proclaimed quantifying atmospheric power as a major challenge for atmospheric physics, we had expected this information to be readily available from the literature. This turned out not to be the case. Below we present a list of controversies that challenge any researcher trying to do what we did – to derive a physically consistent and comprehensive formulation of atmospheric power and its budget in the presence of phase transitions.

1. A major issue is that, contrary to what one can read in textbooks (e.g., Vallis, 2006), in the presence of phase transitions the mass-specific rate of doing work ($\text{J s}^{-1} \text{kg}^{-1}$) is not $p d\alpha/dt$. Mechanical work is proportional to the change of an air parcel's volume \tilde{V} (m^3), while $\alpha \equiv 1/\rho$ ($\text{m}^3 \text{kg}^{-1}$) is the volume of a constant unit mass of gas. When mass of the air parcel is not constant, $d\alpha/dt \neq (d\tilde{V}/dt)/(\rho\tilde{V})$ (see Section 2 in our article). In simple words, if gas is contained in a box and a certain part of the gas condenses, but the volume of the box does not change, no work is performed. In this case $d\alpha > 0$ but $d\tilde{V} = 0$. To our knowledge, the correct formula for mass-specific rate of doing work (see Eq. (11) in Section 2) cannot be found in any textbooks. That this misconception is wide-spread was illustrated by Referee 2 and Referee 4 (Dr. Tailleux) of our first submission, who both used $p d\alpha/dt$ in their comments.²

2. Another major controversy, closely related to the first one, pertains to the correct choice of velocity in the definition of material derivatives of variables related to atmospheric power (the rate of doing work). Using either the velocity of gaseous air or the mean velocity of air and condensate produces drastically different estimates. As the discussion of our two submissions demonstrated³, there is profound confusion as

²<http://dx.doi.org/10.5194/acp-2016-203-RC2> p. C2, <http://dx.doi.org/10.5194/acp-2016-203-RC3> p. C3

³<http://dx.doi.org/10.5194/acp-2017-17-AC5>

C2

to which expressions for atmospheric power are valid. In the revised manuscript we summarize these caveats in a questionnaire, see Fig. 1 in the revised manuscript (also shown in the end of this comment).

In the presence of phase transitions for a correctly defined material derivative of, say, mass-specific enthalpy of moist air h (J kg^{-1}) the following relationship *does not* hold: $d(\int_{\mathcal{M}} h d\mathcal{M})/dt = \int_{\mathcal{M}} (dh/dt) d\mathcal{M}$, where \mathcal{M} is atmospheric mass⁴. The reason is, again, that the air parcel for which the material derivative is defined does not have a constant mass. Thus, for a given air parcel the change, per unit mass, of its enthalpy $\tilde{h} = h/\tilde{m}$ (J) is *not equal* to the change of its mass-specific enthalpy: $d\tilde{h}/\tilde{m} \neq d(\tilde{h}/\tilde{m}) \equiv dh$ (see Eq. (57) in Section 4 of our article).

Again, to our knowledge, nowhere in the meteorological literature the importance of discriminating between gaseous air velocity and mean velocity of air and condensate has been highlighted. Elsewhere in physics such a recognition does exist (e.g., Brenner, 2009).

3. Likewise, nowhere in the meteorological literature can one find a discussion of the boundary conditions for velocity and how they matter for the atmospheric power budget (they do!). Hoping to gain some insight from the studies of Pauluis et al. (2000) and Pauluis and Held (2002), which are the only attempts to derive an expression for the atmospheric power budget for a moist atmosphere, one immediately notices that the boundary conditions adopted in their approach yield nonphysical results, with total atmospheric power exceeding solar power (see Section 3.1 and Fig. 3 in our article).

This inconsistency is due to the neglect of the scale-dependence of the definition of atmospheric power – another important subtlety that we emphasize (Sections 2 and 5.3). A non-zero vertical velocity for the evaporating water vapor exists at a microscopic scale of about one free path length from the evaporating surface. At a greater distance

⁴Cf. comment 4 on p. C2 of Referee 1 of our first submission, <https://www.atmos-chem-phys-discuss.net/acp-2016-203/acp-2016-203-RC1.pdf>

C3

due to collisions with dry air molecules any appreciable difference in gas velocities cease to exist. To get a consistent account of macroscopic atmospheric power one has to put this velocity equal to zero at the evaporating surface and introduce surface sources (e.g., of moist air enthalpy) as Dirac's delta functions, such that the total flow of a considered property into the atmosphere is finite.

4. Furthermore, if one now turns to the numerous publications evaluating the Lorenz energy cycle – for a clue to understanding moist atmospheric power – there, too, one finds *two contrasting formulations yielding different results (!)*. These are the so-called $v \cdot \text{grad } z$ and $\omega \cdot \alpha$ formulations (Kim and Kim, 2013). While Peixoto and Oort explained that the two formulations must be identical, they are not. Most surprisingly, there appears to be little concern about why this is so.

In our revised text we explain that this contradiction is due to the long tradition of using mathematically inconsistent continuity equations, with a zero mass sink for air as a whole and a non-zero mass sink for the mixing ratio of water vapor. This approach, uncritically adopted by Pauluis et al. (2000) and Laliberté et al. (2015), introduces relatively minor errors in the evaluation of the rate of kinetic energy generation. But it profoundly undermines assessments of total atmospheric power and the gravitational power of precipitation, since both depend significantly on the intensity of the mass sink (see Section 3.5 in the revised text for details).

5. Next, even if one has made one's way through all these contradictions, formulated a valid expression for atmospheric power and is willing to explore the power *budget* involving the equations of motion and continuity, one is challenged with another controversy. One suddenly finds out that while many atmospheric models explicitly proclaim a zero source/sink in the continuity equation, they then implicitly re-introduce a non-zero sink via a technical velocity correction, of which many analysts, including Dr. Tailleux and Referee 3 of our first submission, appear to remain entirely unaware. Again, how this correction matters for the atmospheric power budget (it does!) appears previously unexplored.

C4

6. Ultimately, turning to the equations of motion – i.e. to the atmospheric dynamics, which Referee 2 of our present submission has characterised as *"rather well studied and understood"* – one discovers that there co-exist two mutually inconsistent equations of motion for a moist atmosphere, of Ooyama (2001) and Bannon (2002), and that the seventeen years that passed since those formulations got published saw no attempts to resolve the controversy (but see Makarieva et al., 2017).

Thus, to be able to generate a reliable numerical estimate of atmospheric power for comparison with our theoretical predictions, we did our best to disentangle all these confusions, including the recent controversy involving the study of Laliberté et al. (2015). Our formulations simultaneously resolve all the six controversies in a self-consistent manner. Actually, we formulated an approach to handle atmospheric power in a moist atmosphere from scratch – to find that the obtained numbers are compatible with the predictions of the condensation-induced dynamics.

In the view of all the above, in our opinion, it would not be constructive to divide the paper in several parts as Referee 2 of our first submission and Referee 2 of our present submission would advise – we believe that there is an advantage in having a wholistic picture addressing all the existing inconsistencies in a single framework. In particular, cutting the quantitative MERRA – NCAR/NCEP part from the theoretical part would be unproductive. The comparison between MERRA (with its velocity correction to the continuity equations) and NCAR/NCEP (no velocity correction) is a direct illustration of how the form of the continuity equation matters for the atmospheric power estimate. In NCAR/NCEP, but not in MERRA, the total atmospheric power is negative! We would have hoped that some of our referees might be acquainted with these re-analyses well enough to comment on this result (Referee 2 from our first submission could perhaps do that but unfortunately he never showed up again).

Acknowledgements. We want to use this opportunity to thank our referees once again for their time and thought-provoking comments. We are particularly grateful to Dr. Tailleux (Referee 4 of the first submission and Referee 2 of the present submission)

C5

who spent a lot of time evaluating several versions of our text. Dr. Tailleux' objections helped us better understand how the atmospheric and oceanic power budgets and dynamics differ. We are grateful to Referee 1 of our first submission who urged us to look at other datasets beyond MERRA: for us NCAR/NCEP with its negative power was an eye-opener. We are grateful to Referee 2 of our first submission, who attempted to verify our quantitative results and even attributed to us *"an original treatment of re-analyses data"*. Comments of Referee 3 of our first submission helped us a lot in clarifying how our work relates to that of Pauluis et al.

Referee 2 of our present submission paid attention to the key Section 2, which led to a more comprehensive understanding of the expression of global atmospheric power that we obtained.⁵ Moreover, Referee 2 appeared sympathetic to our main idea – the key role of water vapor in driving atmospheric circulation. In this context, we want to briefly outline one more major controversy to which condensation-induced dynamics might provide a clue – monsoons.

7. People working with monsoons know that GCMs do not properly account for these abrupt seasonal shifts in the ocean-to-land atmospheric moisture transport (see, e.g., Acharya et al., 2011). To get around this problem, some time ago people proposed the so-called "moisture advection feedback" – a proposition that the atmosphere over land gets warmer and warmer as more and more latent heat is transported from the ocean. This warming, assumed to be proportional to the inflow of latent heat, is supposed to further facilitate the ocean-to-land transport thus providing a positive feedback on the circulation intensity. This concept was invoked to explain modern monsoonal climates (e.g., Levermann et al., 2009), make projections for Amazonian deforestation (e.g., Wright et al., 2017; Boers et al., 2017) and explain monsoon shifts in the geological past (Herzschuh et al., 2014).

However, this mechanism does not appear to be valid, because, in simple words, la-

⁵<http://dx.doi.org/10.5194/acp-2017-17-AC6>

C6

tent heat does not warm. That is to say, if the atmosphere is already moist adiabatic, a more rapid release of latent heat will not make it any warmer. Explained from a different angle, as we clarified in our response to Dr. Garrett, the release of latent heat per se cannot accelerate air circulation (and can even suppress it)⁶. A "moisture advection feedback" based on latent heat does not exist. This was recently clearly pointed out in an exchange of opinions in PNAS (Boos and Storelvmo, 2016a; Levermann et al., 2016; Boos and Storelvmo, 2016b). The abrupt monsoon transitions remain unexplained.

Condensation-induced dynamics, even if it is simplistically understood as a hydrostatic mass sink (but see p. C7 in <http://dx.doi.org/10.5194/acp-2017-17-AC4>), is principally different. Since the removal of gas by condensation lowers surface pressure, this pressure drop initiates an inflow of air towards the condensation area. If the arriving air ascends and is easily ventilated away from the condensation area in the upper atmosphere (i.e. when neither ascent nor ventilation is the limiting process of the circulation), then enhancing the inflow of water vapor will intensify the rate at which pressure is lowered by condensation thus providing a positive feedback to the circulation intensity. The intensity of condensation-induced air circulation is directly proportional to water vapor inflow and condensation rate (see Section 6 in our article) and thus provides the true "moisture advection feedback" necessary to explain monsoons. Forests moistening the atmosphere more efficiently than does the ocean, are able to win the "tug-of-war" with the ocean for atmospheric moisture (Makarieva et al., 2013, 2014).

Finally, we thank our Editor for making these discussions possible, for supervising them and for encouraging us to extend and re-submit our work after our first submission. We have learnt a lot while working on this topic.

Technical note. We will post the revised manuscript to arxiv at <https://arxiv.org/abs/1603.03706> as version 4 (v4). All other relevant information

⁶<http://dx.doi.org/10.5194/acp-2017-17-AC3> p. C3

C7

will be available at www.bioticregulation.ru/ab.php?id=he. Interested readers might prefer to read the article at arxiv.org (the present submission is version 3) and not in ACPD. For some reason, the ACPD publishing process eliminates all the hyperlinks from the text (hyperlinks are supported by the ACPD Latex template and are present in the original PDF file that we submit to the journal). This makes navigation between the many equations extremely complicated (here we do understand Referee 2 who complained about the large number of equations in our article).

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C8

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Interactive comment on Atmos. Chem. Phys. Discuss., <https://doi.org/10.5194/acp-2017-17>, 2017.

C9

<p>A. Which expression(s) describe(s) the global rate of generation of mechanical work in a moist atmosphere?</p> <p>B. Which expression(s) describe(s) the global rate of generation of mechanical work in a dry atmosphere only?</p> <p>C. Which expression(s) describe(s) the global rate of generation of kinetic energy of air in a moist hydrostatic atmosphere?</p>	<p>I</p> $-\int (\mathbf{v} \cdot \nabla) p dV$	<p>II</p> $-\int (\mathbf{u} \cdot \nabla) p dV$	<p>III</p> $\int p \nabla \cdot \mathbf{v} dV$
	<p>IV</p> $\int p (\mathbf{v} \cdot \nabla) \alpha dM$		
	<p>V</p> $-\int (\mathbf{v}_m \cdot \nabla) p dV$	<p>VI</p> $-\int (\mathbf{u}_m \cdot \nabla) p dV$	<p>VII</p> $\int p \nabla \cdot \mathbf{v}_m dV$

Figure 1. A questionnaire overviewing possible issues with the formulation of atmospheric power. Here p is the ideal gas pressure, ρ and ρ_c are the densities of gaseous air and condensate particles, respectively; \mathbf{v} is velocity of gaseous air, $\mathbf{v}_m \equiv (\rho \mathbf{v} + \rho_c \mathbf{v}_c)/(\rho + \rho_c)$ is the mean velocity of air and condensate (sometimes called "barycentric velocity"), \mathbf{u} and \mathbf{u}_m are the horizontal components of, respectively, \mathbf{v} and \mathbf{v}_m , $\alpha \equiv 1/\rho$, M is the mass of the gaseous atmosphere $M = \int \rho dV$, V is the total atmospheric volume. "Moist atmosphere" implies $\dot{\rho} \neq 0$ and $\mathbf{v}_m \neq \mathbf{v}$, "dry atmosphere" implies $\dot{\rho} = 0$ and $\mathbf{v}_m = \mathbf{v}$. Note that in the physics literature it has been recognized that the choice between \mathbf{v} and \mathbf{v}_m is not trivial (Brenner, 2009). Our response to questions A–C is given in the beginning of Section 7.

Fig. 1. New figure added to revised Introduction.