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

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We thank the referee for the time spent on our work and useful comments. In our response below we want to focus on the physical misconception of interpreting negative work as dissipation (suggested by Dr. Tailleux and seemingly approved by Referee 2 in his comment No. 2).

The more subjective presentation arguments are never as clear cut as physical arguments; so they are more difficult to address. We respect the referee's concern about our insufficient succinctness and clarity. To be quite honest when submitting the current version we felt very good precisely because we thought that we had made our presentation as clear and detailed as possible. Lack of a positive feedback to any of our efforts came quite puzzling to us though; such that we do not have any guidance

as to in which direction to move to achieve more appeal to our potential readers in the future. But we will continue our efforts.

1. Length of the paper We agree that the paper is long and contains many equations. However, we thought that if all of them are correct, and the results valuable, the length and the number of equations are not by themselves a shortcoming. It is good sometimes rather than dealing with the so-called "salami slicing" to have all the material in one place. Such a text does not necessarily make an easy reading but it can be used as a convenient reference document where you can find all the essential information related to a particular topic of interest.

The referee says: *"In its current state, I don't understand what is the main point of the paper. Is it about the definition of "atmospheric power"? the role of condensation in this power? the evaluation of atmospheric power using the MERRA re-analyses ? or in fine the speculation that moisture accounts for most of the atmospheric power as suggested in the last part ? I must admit I am rather sympathetic with this final speculation, but as it stands, I cannot defend such a confusing manuscript."*

In fact, the referee quite clearly names the main parts of our paper: it defines atmospheric power; discusses how condensation impacts the formulation of this power; evaluates this power using the obtained formulation and MERRA **and NCAR/NCEP reanalyses** (not mentioned by the referee) and then presents a discussion about moisture accounting for most of the atmospheric power. We thought that dividing these materials into several papers would just lead to unnecessary repetitions in each of them.

2.1 The definition of atmospheric power

The referee notes (our emphasis) that *"The approach followed in this manuscript is to consider one term of the equations, the work of compression/expansion forces (W in eq. (9)), as the "power" system and, implicitly, viscous friction as "dissipation" or "end-user". This is a* ***legitimate choice****. But as noted by R. Tailleux in his review* *(acp-2017-17-RC1), this* ***might not be the most appropriate one*** *since compression/expansion forces also include negative terms that dissipate energy: consequently "work of compression/expansion forces" is NOT equivalent to "generation of mechanical energy from heat". In any case, this is a choice.*

We need to turn to the basics of thermodynamics. Consider a Carnot cycle – a textbook example of how mechanical energy is generated from heat. Carnot cycle consists of two adiabates and two isotherms, the warmer one and the colder one. At the warmer isotherm the air expands (positive work). Then it expands at the first adiabat (positive work). Then it compresses at the colder isotherm (negative work). Finally, it compresses at the second adiabat to return to the initial point (negative work).

This cycle, as is well-known, consists of reversible processes only; no dissipation occurs at any stage. Still, the mechanical energy produced by this cycle is equal to the net work performed by the working body (gas), i.e. to the sum of the negative and positive amounts of work performed at the different stages of the cycle. It is exactly the case when *"work of compression/expansion forces" IS equivalent to "generation of mechanical energy from heat".*

The same situation occurs in the atmosphere: the ascending air motions are associated with expansion and positive work, the descending air motions are associated with compression and negative work. Horizontal air motions can be of either type.

Therefore, if we aim to have a definition of atmospheric power (mechanical energy generated per unit time) that conforms to the constraints of the laws of thermodynamics, we have **no other choice** but to define it as the net work performed by both compressing and expanding air parcels in the entire atmosphere.

The suggestion of Dr. Tailleux to define atmospheric power just as the positive part of work performed in a thermodynamic cycle appears to be based on a misinterpretation of the process of compression – negative work which he understands as "thermal dissipation". In reality the descending and compressing air parcels do not dissipate en-

ergy. Indeed, compressing air parcels increase their pressure, which plays the role of potential energy in the Bernoulli equation and can be reversibly transformed to kinetic energy. Compression of the descending air parcels in a hydrostatic atmosphere, where $p = \rho g h$, represents a reversible conversion of the diminishing gravitational energy into the increasing ideal gas pressure. Interpreting this as a dissipative process is incorrect.

Consider a simple analogy: a non-dissipating pendulum, which reversibly transforms kinetic energy to potential energy and back. In the picture of Dr. Tailleux, when the kinetic energy diminishes being transformed to potential energy we would be dealing with dissipation, since the work of the gravity force (work = path times force) is negative.

We can formally define a power for such a system: it is the rate at which potential energy is transformed to kinetic energy (or vice versa), that is, the store of energy divided by the halfperiod of the pendulum. The power of such a non-dissipative system will not be constrained by the laws of thermodynamics in the sense that it can be arbitrary – depending on the store of energy in the system and its internal dynamics.

Meanwhile if the pendulum is a dissipative one and loses a bit of its energy with each cycle of energy transformation – such that we need to add some energy (either potential or kinetic) to it to keep it steady, then the power of this system will be equal to the rate at which dissipation occurs. This power will be constrained by the laws of thermodynamics. It is for this definition of power that we argue.

2.2 Let us not fully discount all previous literature on the subject

The referee says *"I strongly disagree with the author's point of view that "power" is a quantity defined a priori from the equations of the fluid. Power can only be defined as the mechanical energetic output of a "power system". Obviously, concerning the atmosphere, the "power system" is only an abstract part of the atmosphere, that needs to be specified first. I believe this misunderstanding largely explains why the introduction and the definition of "power" is so confusing in this manuscript.*

This appears to be a peculiar situation: there is a huge literature on atmospheric power both before and after Lorenz (1967) who identified quantifying the global atmospheric power as a major challenge for atmospheric sciences. In all of this literature, including Pauluis et al. and Laliberte et al. whom we discuss in detail, atmospheric power is consistently defined and interpreted as the net work of compression/expansion with viscous dissipation as the "end-user" (in the words of Referee 2). In our work we show how this understanding of atmospheric power is formalized in the presence of phase transitions; how the resulting formulations depend on the form of the continuity equations, boundary conditions, equations of motions etc.

Meanwhile Dr. Tailleux and Referee 2 appear to oppose the whole of this literature by noting, with a reference to two papers by Tailleux (2010) and Tailleux (2015), that all this previous understanding is either "not the most appropriate" in the words of Referee 2 (who however admits that the conventional view is "legitimate") or even "misleading" in the words of Dr. Tailleux. Even if the idea of Dr. Tailleux about compression=dissipation would not have been a misconception, denying our paper the right for existence on this basis would have been unjustified, in our view.

3. Pauluis et al. and manuscript structure

The referee mentions again the number of equations; also in their first comment the referee notes that while at its first submission *"the manuscript was 29 pages long. Now, in this new submission, the scientific content is to a large extent mostly the same, but the manuscript has been significantly expanded to 52 pages.*

We will continue to make efforts to be as clear as possible but as different reviewers and readers have difficulty with different issues and items, this has inflated the text. In fact, the content is not exactly the same as much new material was added as follows:

(1) Referee 1 of our first submission suggested that we should look at other datasets beyond MERRA and other spatial/temporal scales – we undertook an effort to include NCAR/NCEP (and also extended the analysis time from fifteen to thirty five years).

This NCAR/NCEP, because of no velocity correction, presented quite different results than MERRA – this warranted an additional discussion which also had implications for our criticisms of Laliberte et al. Unfortunately, Referee 1 never showed up again; other referees showed no interest in MERRA versus NCAR/NCEP. But the paper became longer.

(2) Then, in our first submission, we did not criticize the study of Pauluis et al., which we respect and consider valuable even if not consistent when it comes to the power budget. We just referenced their work. However, precisely because we were laconic on this matter, Referee 3, who admitted being in contact with Dr. Pauluis when evaluating our work, claimed that we had appropriated their results. (Those very results that Dr. Tailleux and Referee 2 now find unsatisfying.) Thus, in the revised version we briefly explained which issues we had with the approach of Pauluis et al. This, however, appeared insufficient, since Referee 3 misinterpreted our criticisms when evaluating our revised manuscript. Providing recommendations for a possible re-submission, our handling editor advised that we should account for the previous work as clearly as possible. We thus extended our analysis of Pauluis et al. and provided a one page figure showing how our approaches are similar and where they differ. The paper became longer again.

(3) Finally, a major revision was undertaken following the recommendation of Dr. Tailleux who advised as follows (see page 14 here http://www.atmos-chem-phys-discuss.net/acp-2017-17/acp-2017-17-EC1-supplement.pdf)

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 Atmospheric Power = 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?*

The Editor likewise suggested that we should pay attention to this comment of Dr. Tailleux and represent the various parts of the atmospheric power budget as a box diagram. Accordingly, we performed all the requested analyses and added the needed diagram (Fig. 1). This has required a considerable extension of the presentation, because there are many subtleties and controversies in the momentum equations for the moist atmosphere – we tried to accurately sort all of them out. The paper became yet longer again, but we felt it was worth it. In our opinion, the paper has greatly improved since the first submission.

Unfortunately, Dr. Tailleux in his new evaluation of our work appeared uninterested in how we followed his previous recommendations; instead, he switched to the idea that the atmospheric power should be defined along the ways outlined in his 2010 paper (where negative work is incorrectly interpreted as a dissipative process).

4. References to wind speed tendencies

The referee points out that we do not list references about "wind stilling" in the lower atmosphere on land. We would be willing to include such a discussion. Our logic was to demonstrate that there are discrepancies between model predictions and observations: these discrepancies are among those factors that justify an increased attention to the atmospheric power topic. Accordingly, we listed references to such discrepancies, i.e. to evidence in favor of atmospheric intensification. DeBoisseson et al. is not a single piece of evidence for such discrepancies, a number of references they cite support this intensification too. True that the study of Huang and McElroy 2015 is based on a re-analysis but it is all we have to evaluate the global atmospheric power. The fact that the re-analysis data do not conform to the GCM predictions is an interesting fact in itself which should not, in our view, be dismissed without a consideration.

We thank our referees again for their efforts and this discussion.

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