

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

### **Anonymous Referee #2**

Received and published: 25 April 2017

I am not a specialist of the energetics of the atmosphere. As a physicist and climatologist, I first thought this paper could be, for me, a good opportunity to better understand this topic. Indeed, if the dynamics of the atmosphere appears rather well studied and understood, this is not quite true concerning its energetics. On this point, I must say I am quite disappointed. Indeed, the manuscript is long, not always consistent, and relies more on mathematical equations than on good physics. To summarize, it is more confusing than enlightening. I have looked through the rather numerous and lengthy discussions that this manuscript and its previously submitted version (in 2016) have generated. This did not help me to clarify what the main point of the paper actually is. I therefore do not recommend its publication.

1- Quite interestingly, Reviewer#2 of the 2016 submission said (point 5, acp-2016-203-RC2): “The way I see it, there are approximately three manuscripts in this study”. At

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this time, 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. The manuscript ends with a summary of the main results are: a bullet list with 14 points. I therefore strongly agree with the above review's quote, and suggest to split the content into several separate papers (at least three, possibly more. . .). 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.

2 - In the introduction (part 1) the authors claim that the definition of atmospheric power varies quite a lot in the literature, depending on authors, and relate this difficulty to phase transitions of a moist atmosphere. They insist in defining power through an integral over the atmosphere of local quantities like velocity and pressure (equation 9). The discussion is quite strange. For instance, line 108: "Considering that pressure, too, varies across the parcel as velocity does, [...] total atmospheric power  $W$  would invariably be zero, which does not make sense" If I understand well, the authors suggest that the spatial scale of pressure should be different than the scale of velocity (line 104), which looks a bit strange to me. On the other side, the fact that the true integral of the work of the atmosphere should be zero does perfectly make sense : the global volume and pressure of the atmosphere being almost constant, there is little work transferred from the atmosphere to other components (except for ocean waves or hydraulic gravitational energy over land) in the usual thermodynamic sense. Thermodynamics deals with integral quantities of macroscopic objects (pistons and engines), and transfers of energy between them (heat and work). Therefore the rather clumsy, not convincing discussion, on page 4 of the manuscript, where the authors introduce sums of virtual boxes (line 88) without explaining what they represent in the real world. The key point is that mechanical energy (or power) generated in some parts of the atmosphere can be

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dissipated in others. An (almost) null integral for the “net power” is therefore something quite natural and expected from simple thermodynamics. In physics, work is defined by force times length, with force being an interaction between TWO objects. Similarly, in thermodynamics, power implicitly involves TWO objects: an engine and an end-user that “dissipates the mechanical energy” and it is easy to define power through the forces acting between these TWO objects. The situation is obviously less clear when considering only ONE object. Here “atmospheric power” concerns the energetics of winds, or the generation of mechanical (kinetic and potential) energy WITHIN the atmosphere. 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. 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.

3 – The first half of the paper (parts 1, 2 and 3) is 20 pages long, with 54 numbered equations (. . . plus many more in the text). The final paragraph reads: “In summary, PBH obtained a valid expression for precipitation-related dissipation  $WP^* = WP + WC$  (44). I certainly understand that the authors disagree with Pauluis et al (2000) (PBH) on the way to derive this expression. But if the subject of the paper is to discuss this point, then this should be the end of the manuscript. If the subject of the paper is to estimate atmospheric power from reanalysis, then this should be the start.

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4 – As a motivation for examining atmospheric power, Makarieva et al. note a discrepancy between observations and models (lines 28-34). They mention a globally increasing intensity of observed winds, by citing de Boissésion et al. (2014) who are documenting such an increase in the equatorial Pacific only, and Huang & McElroy (2015) who are in fact using reanalysis, not wind data. In contrast, they do not mention the well documented phenomena of “global stilling”: the statistically most robust trend for (surface) winds is a decreasing one (at least over continents where we have direct data) and possibly an increasing one at higher altitudes. This looks like a blatant misrepresentation of current knowledge on climatological wind trends.

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

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