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

Anonymous Referee #3

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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 gravita-*

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Discussion paper

tional 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.

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. 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. 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.

Second, it is perfectly valid to question the impact of mass source and sink on the framework of Laliberte etal., 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.

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.

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.

My recommendation here would be to simplify section 2 and 4, drop section3 and expand on section 5. Section 6 could be clarified as well.

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