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

Anonymous Referee #2

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This manuscript looks at the power budget in the MERRA reanalysis over the last 7 years. It is generally poorly written and way too long for the arguments being made. In its current state, it does not stand up as a contribution worthy of the high standards of publication for ACP. Based on my comments (to be found below), I do not recommend this manuscript for publication at ACP.

Key Comments:

1. Section 2 is both way too complicated and appears to be wrong. Following Vallis' (2006) notation:

$$W = \int_{\mathcal{V}} p \frac{d\alpha}{dt} \rho d\mathcal{V} = \int_{\mathcal{V}} p \left(\partial_t(\rho\alpha) + \nabla \cdot (\rho\alpha\mathbf{v}) - \alpha S_\rho \right) d\mathcal{V},$$

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Discussion paper

where $S_\rho = \partial_t(\rho) + \nabla \cdot (\rho \mathbf{v})$ is the local sources and sinks of mass. Now, $\alpha\rho = 1$ so

$$W = \int_{\mathcal{V}} p \left(\nabla \cdot (\mathbf{v}) - \alpha S_\rho\right) d\mathcal{V} = \int_{\mathcal{V}} \left(\nabla \cdot (p\mathbf{v}) - \mathbf{v} \cdot \nabla p - \alpha p S_\rho\right) d\mathcal{V}.$$

This is the same form as in equation (8). But it depends explicitly on S_ρ, contrary to the authors' claim. Why this contradiction? The problem in the authors' derivation comes in part from equation (3). While it is true that $\sum_i d\tilde{N}_i/dt = 0$, it is not true that $\sum_i T_i d\tilde{N}_i/dt = 0$, unless the atmosphere is isothermal. But it is exactly what's used to convert the last term in equation (2) to the last term in equation (4). $\tilde{V} = \tilde{N}/N$ has units of m^3 (parcel^{-1}). To compute work, however, we need the specific volume with units of m^3 (kg^{-1}). So we have to introduce a new quantity, the mass per parcel $\tilde{m}$ so that the specific volume is $\tilde{V}/\tilde{m}$. Then the expression for work (equation 4) with the same units as in Vallis (2006) reads:

$$W = \frac{1}{\mathcal{S}} \int_{\mathcal{V}} p \frac{\tilde{m}}{\tilde{V}} \frac{d(\tilde{V}/\tilde{m})}{dt} d\mathcal{V}.$$

But the continuity equation (6) also requires fixing. Since, N has units of mol m^{-3} then equation (6) is an equation for mass conservation only if the molar mass $\tilde{m}/\tilde{N}$ is constant. But here the authors are, among other things, concerned about the effect of moisture on the work and moist air, unlike dry air, has an inhomogeneous in molar mass. The continuity equation (6) should then read:

$$\partial_t((\tilde{m}/\tilde{V})) + \nabla \cdot (\mathbf{v}(\tilde{m}/\tilde{V})) = \dot{\tilde{m}}(N/\tilde{N}) + \tilde{m}/\tilde{N}\dot{N} - (\tilde{m}N/\tilde{N}^2)\dot{\tilde{N}}$$

where the right hand side is the local sources and sinks of mass. With these fixes, the expression for work will look exactly like in Vallis (2006) and will depend on the sources and sinks of mass.

2. Section 3.1. This section is also way too complicated. After the first paragraph, one can jump directly to the top of page 5. Now equation (15) is not wrong

per se. However, the Makarieva et al. (2013) analytical derivation is somewhat meaningless when applied to reanalysed data: this can be evaluated directly. This is exactly what I have done for the purpose of this review. Using the 1 hourly vertically integrated budgets provided from the data archive, one can compute the integral $\int_{\mathcal{S}} \overline{h}\overline{\dot{\rho}} d\mathcal{S}$, where the overline indicates vertically integrated fields. In the reanalysis, $\overline{\dot{\rho}} \neq 0$ because of the analysis step. In MERRA, this is provided directly. In the MERRA documentation it is indicated that this $\overline{\dot{\rho}}$ includes both the effect of $E - P$ and adjustments needed to represent the observed surface pressure field accurately. It therefore includes the effect described by the authors. This quantity for 1980-1985 is 0.2 W/m^2. Adding the vertical dependence would likely be a second order effect since $E - P$ is mostly driven by horizontal and temporal variability. This simple analysis performed using the output from the MERRA product seems to show that Appendix A is likely to be inaccurate (0.2 is not within 30% of 1.6). In any case, this issue was discussed at length by Trenberth (see his papers in the 1990's) and the proposed solution is to modify the winds so that the continuity equation does not have a source term. I had a hard time finding this but you mention that Laliberte et al (2015) might have done something like this. In this case, I do believe that $\int_{\mathcal{M}} dh/dt d\mathcal{M} = 0$ makes sense since it is an exact derivative.

3. Computing the work from MERRA data. As mentioned before, the MERRA product has many vertically integrated budget variables that allow one to quantify each one of the term in the energy equation. For this review, I've looked at the kinetic energy generation 1980-1985 and the yearly average gives 3.40-3.48 W/m^2 for the integral of $\omega\alpha$ and 3.6-3.8 W/m^2 when including the kinetic energy generation from the analysis step. The kinetic energy generation is balanced by damping from the numerical dissipation, the dynamical remapping and the physically parametrized frictional dissipation. This means that the estimates provided in section 5.1 are substantial underestimates.

4. In section 5.1, I do not see the use for W_1. And why not use $\omega_s = \partial_t p_s + \mathbf{v}_s \cdot \nabla_H p_s$, with ∇_H being the horizontal gradient? The p_s and $\mathbf{v}_s$ are both available and this is the right expression. Maybe that could fix their underestimate of W.

5. The way I see it, there are approximately three manuscripts in this study. The first one, sections 2 and 3 as well as Appendix A, consist mostly of derivations that are either flawed or mostly useless for this study. The second paper is more akin to a white paper and comprises sections 6.1 and 6.2. Now, sections 1, 4, 5 and the very beginning of section 6 as well as Appendix B and C are self-contained and describe an original treatment of reanalysis data. Appendix C could be moved up after section 4. If they wish to submit their results to another publication, I would recommend that the authors focus on these sections and perform their analysis on the whole of MERRA (1979-2015).

6. Finally, I'm not sure the following sentence is logically true: "The fact that W_{Kc} is likewise higher than our MERRA-derived kinetic power, testifies in favor of the theoretical estimate". All it means is that W_{Kc} is potentially a right upper bound. The only way to check whether it is the right upper bound would be to either verify if it holds on other Earth-like planets or using simulations with increasing resolution and seeing that it describes the scaling. As I said before, the last two sections of this manuscript are really too speculative in their current form and they are dragging down the original results described in sections 5.

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