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Atmospheric Chemistry and Physics
Discussions

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

Anonymous Referee #1

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Review of paper "Quantifying the global atmospheric power budget" by Makarieva et al.

Summary: In this study, the authors attempt to examine the gap between the gravitational power of precipitation, which is estimated as the total atmospheric power - kinetic power from the MERRA dataset, and another independent gravitational power of precipitation estimated from the surface precipitation data. While I can see a good merit of this work, I found the paper contains several loopholes that need to be clarified before the manuscript can be accepted for publication. Also, the presentation of this work is somewhat confusing, and can be simplified substantially to make it clearer. My concerns are given as bellow:

Major concerns:

1. The evaluation of the gravitational power of precipitation (GPP) as presented in Appendix A, which is used to verify the GPP estimated from the MERRA data, contains a significant source of uncertainties as it depends so much on different input parameters as listed in Appendix A. Likewise, the GPP estimated from MERRA also depends strongly on the data resolution, the number of vertical levels, or the numerical approximations. Before trying to explain the discrepancies between GPP obtained from GPCP data and the GPP obtained from the MERRA data, the authors should at least quantify the errors in all of your numbers. While the authors claim that the uncertainty of your estimated GPP from GPCP is 30%, there is no guarantee that the difference between the two GPP estimations will be statistical significance. Afterall, 30% of 1 W m-2 is 0.3, and so it could be anything from 0.7-1.3 W m-2, which may be comparable to the GPP computed from the MERRA data;

2. Estimations of the total atmospheric power W and W_K are subject to similar uncertainties as mentioned in my comment # 1 above. At resolution of 1.25 degree and 42 vertical levels, any global estimation of the total integrated energy and kinetic energy contains large variation, let alone the difference between two. Have the authors tried the NCEP reanalysis or ECMWF dataset at different resolutions to see how sensitive your estimations are? As long as we don't have reliable estimation of W, W_K, and GPP, explanation for the difference would provide little scientific value.

3. The derivation of the total atmospheric power given by Eq. (7)-(8) is unnecessarily complicated. I can directly obtain Eq. (7) from Eq .(2) by noting simply that $\int p dV/dt=\int p d(\delta x \delta y \delta z)/dt=\int p(\nabla \cdot v)dV$. Not sure why the authors present their argument in such a lengthy and confusing way.

4. The authors criticize Laliberte et al. (2015)'s estimation of the integral of dh/dt, as they believe that it is not $dh/dt=0$ but should be $\partial h/ \partial t=0$ for a stationary budget. However, my understanding of Laliberte et al.'s study is that the total derivative that Laliberte et al used is in the context of global integration. So if you define $H=\int h dV$, then $dH/dt=\int \partial h/\partial t dV$, since the total volume

is fixed in time. As such, Laliberte et al.'s global stationary approximation is consistent with your local stationary approximation.

Minor concern:

The practice of putting a dot on a variable to represent sources/sinks is too confusing, as the dot often denotes time derivation. Equation such $\dot N =dN/dt+N(\nabla \cdot v)$ is perplexing. The authors should replace all such dotted source/sink by different symbols to avoid the confusion.

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