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Atmospheric Chemistry and Physics Discussions

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

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Here we briefly describe the major revisions made, add a few comments on Section 6 which received relatively little attention of the referees and attach our replies to all the referees' comments. The revised manuscript will be available from http://arxiv.org/abs/1603.03706v2 (version 2).

We consider the major criticisms against our work to be Comments 1 and 3 of, respectively, Referees 2 and 4, who suggested that the expression we derived for atmospheric power is incorrect. In our response [doi:10.5194/acp-2016-203-AC3] we showed that in both cases this suggestion was based on the same incorrect definition of work per unit mass. We revised our text to clarify the various formulations for work of atmospheric circulation; how and why they differ and which is applicable to a moist atmosphere.

The next major criticism was in the second comments of Referees 2 and 3. Our conclusion that in their assessment of global atmospheric power Laliberté et al. (2015) neglected a major term (the global integral I_h of material derivative dh/dt of mass-specific enthalpy h) was deemed invalid. In our response [doi:10.5194/acp-2016-203-AC3 and 10.5194/acp-2016-203-AC5] we showed that these criticisms misunderstand the dependence of I_h on moisture sources and sinks. In particular, we showed that I_h is not zero when the vertically integrated continuity equation has a zero source/sink.

Importantly, we also showed in the revised text that the omission of I_h stems from the same reasoning that led to Comments 1 and 3 of Referees 2 and 4 concerning the definition of work. The reason is a misinterpretation of dh/dt (or $d\alpha/dt$, where α is mass-specific volume) as the change per unit time of, respectively, enthalpy and volume per unit mass of a material element (air parcel). This is not correct in the presence of phase transitions, because the parcel's mass is not constant. The revised text clarifies this issue and should reduce future confusion.

Referee 1 suggested that we should check our results across different resolutions and databases. We followed this suggestion [10.5194/acp-2016-203-AC6], which resulted in major changes in Section 5 where the atmospheric power budget is assessed from re-analyses. While our original manuscript considered 3-hourly MERRA dataset for 2009-2015, in our revision we analyze 3-hourly, daily and monthly MERRA datasets and NCAR/NCEP daily and monthly datasets for 1979-2015.

Two major conclusions emerged. First, the new data supported our original statement that estimated kinetic power W_K should grow with better resolution until all convective motions are resolved. Our analyses suggest that in this limit W_K should be about 4 W m^{-2}. This coincides with our previously published theoretical estimate of condensation-induced air circulation. Second, we found that, unlike W_K, total power W and the gravitational power of precipitation W_P are not consistent across the re-analyses and we have now suggested how independent estimates of W_P might improve future estimates.

Finally, we disagree with Referee 3 who characterized our Section 6 as a repetition of our previous work. This section shows that condensation-driven circulation corresponds to a Carnot cycle with a temperature difference ΔT coinciding with the mean temperature difference between evaporating and condensing water vapor. In the revised text we explain that this result is new.

This work evolved from a short technical comment that we made on the work of Laliberté et al. (2015) in February 2015. This comment and the review we received from Science is available from http://www.bioticregulation.ru/ab.php?id=he. In particular, one referee of this short comment refuted our suggestion that air circulation on Earth can be powered by condensation by noting that the models of a dry atmosphere display the same atmospheric power as does the real atmosphere – hence no need for alternative drivers. Assessments of our work by other anonymous colleagues showed that this idea is common. Thus, in Section 6 we explain why models of dry atmospheres cannot indicate whether or not global atmospheric circulation is condensation-driven.

We thank our referees for their contribution. A complete list of all the referee's comments and our replies – updated in accordance with the final revised text – is attached to this Author Comment as a supplement PDF file.

Please also note the supplement to this comment:
http://www.atmos-chem-phys-discuss.net/acp-2016-203/acp-2016-203-AC7-supplement.pdf

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