Atmos. Chem. Phys. Discuss.,
doi:10.5194/acp-2016-203-AC2, 2016


Atmospheric Chemistry and Physics
Discussions

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

**A. M. Makarieva et al.**

ammakarieva@gmail.com

Received and published: 4 July 2016

Here we reply to Comment 2 of Referee 2, who shows how, in their view, $I_h \equiv \int_{\mathcal{M}} dh/dt d\mathcal{M}$ neglected by Laliberté et al. 2015 can be directly estimated from MERRA – rendering useless our theoretical estimate of $I_h$ in Appendix A. The referee performs such an estimate, showing that it is significantly smaller than ours, and states that $I_h$ is zero when, as possibly assumed by Laliberté et al. 2015, $\int \dot{\rho} dz = 0$.

We thank Referee 2 for their effort to numerically check our results. However, as we show below, the estimate obtained by Referee 2 appears to result from a misunderstanding (a confusion of the mean of a product $\overline{f_1 \cdot f_2}$ for the product of means $\overline{f_1} \cdot \overline{f_2}$, which is fatal when $\overline{f_1} = 0$). As such, this estimate neither disproves our theoretical result nor justifies the omission of $I_h$ by Laliberté et al. 2015. As we clarify below, we



have demonstrated in our work that $I_h$ is not proportional to the vertical integral of the source term $\int \dot{\rho} dz$ and does not vanish when the latter is zero.

Comment 2 of Referee 2 [doi:10.5194/acp-2016-203-RC2] in full reads:

"*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.*"

We presume that the referee's agreement with our Eq. (15) pertains to the equality

$$I_h \equiv \int_{\mathcal{M}} \frac{dh}{dt} d\mathcal{M} = - \int_{\mathcal{V}} h \dot{\rho} d\mathcal{V} \equiv - A. \quad \text{(c1)}$$

The referee proposes to estimate $A$ as

$$A \approx B \equiv \int_{\mathcal{S}} \widehat{\dot{h}} \widehat{\rho} d\mathcal{S}. \tag{c2}$$

suggesting that $\widehat{\dot{h}}$ and $\widehat{\rho}$ are available from the MERRA dataset (we replaced the overline by $\widehat{}$ in $B$ to preserve the overline for the averages to appear below).

We need first to resolve an inconsistency between the units of our $A$ and the referee's $B$. First, we note that the dot over enthalpy $h$ in $B$ may be a misprint since an *enthalpy source* $\dot{h}$ appears to be an unspecified variable out of context. Next, if following the referee's indication that $\widehat{}$ in $B$ denotes *vertically integrated fields* we assume that $\widehat{h} \equiv \int h dz$ and $\widehat{\dot{\rho}} \equiv \int \dot{\rho} dz$, then $B$ has the units of [J s$^{-1}$m], while $A$ has the units of [J s$^{-1}$]. So expression $B$ needs some "fix" before it could be compared with $A$.

Keeping $\widehat{\dot{\rho}} \equiv \int \dot{\rho} dz$, the only way we can see to remedy $B$ is to assume that $\widehat{h} \equiv \int h dz / \int dz$, units [J kg$^{-1}$] is the mean enthalpy in the air column (not the vertically integrated enthalpy [J kg$^{-1}$ m]). In this case the units of $A$ and $B$ coincide and what the referee proposes reads

$$A \approx \int_{\mathcal{S}} \left( \frac{\int h dz}{\int dz} \int \dot{\rho} dz \right) d\mathcal{S}. \tag{c3}$$

Noting that $d\mathcal{V} = dz d\mathcal{S}$, this implies the following replacement in $A$

$$\int h \dot{\rho} dz \approx \frac{\int h dz}{\int dz} \int \dot{\rho} dz. \tag{c4}$$

By dividing both parts of (c4) by $\int dz$ we find that (c4) relates the columnar mean of $h\dot{\rho}$ to the product of columnar means of $h$ and $\dot{\rho}$. The two expressions are not equivalent, since, as is well-known:

$$\overline{h\dot{\rho}} = \overline{h} \cdot \overline{\dot{\rho}} + \overline{(h - \overline{h})(\dot{\rho} - \overline{\dot{\rho}})}, \tag{c5}$$



where $\overline{X} \equiv \int X dz / \int dz$. The second term in the right-hand part of (c5) represents the covariance of the two variables. Indeed, we know that the enthalpy and the rate of phase transitions in the atmosphere are spatially correlated: $h$ is higher at the surface where evaporation occurs and $\dot{\rho} > 0$ and lower in the upper atmosphere where condensation occurs and $\dot{\rho} < 0$. Therefore, $\overline{(h - \overline{h})(\dot{\rho} - \overline{\dot{\rho}})}$ in (c5) is not zero.

When, as proposed by the referee, $\int \dot{\rho} dz \to 0$ and $\overline{\dot{\rho}} \to 0$, the first term in (c5) disappears. The relative error of estimating $\overline{h\dot{\rho}} \neq 0$ by $\overline{h} \cdot \overline{\dot{\rho}}$ tends to infinity. For this reason $B$ carries no information about the real value of $A$ and, hence, $I_h$ (c1).

Note also that since the enthalpy of an ideal gas is defined to the accuracy of an arbitrary constant, the absolute magnitude of $\overline{h} \cdot \overline{\dot{\rho}}$ for $\overline{\dot{\rho}} \neq 0$ does not have any physical meaning as it explicitly depends on that constant. The second term in the right-hand part of (c5) is constant-invariant.

In our work we have estimated $I_h$ assuming that evaporation and condensation are localized at, respectively, the surface $z = 0$ and the mean condensation height $z = H_P$. This approximation allows one to explicitly specify $\dot{\rho}$ via the Dirac delta function

$$\dot{\rho} = E(x,y)\delta(z) - P(x,y)\delta(z - H_p), \quad \int \dot{\rho} dz = E(x,y) - P(x,y), \tag{c6}$$

from which $I_h$ can be explicitly evaluated.

Putting $E(x,y) = P(x,y)$ in Eq. (15), such that $\int \dot{\rho} dz = E(x,y) - P(x,y) = 0$, one obtains from our Eq. (15) that the integral $I_h$ is proportional not to the (zero) difference between evaporation and precipitation, but, as one might have expected, to the intensity of the water cycle, i.e. to $E(x,y) = P(x,y)$ multiplied by the difference in air enthalpy between $z = 0$ and $z = H_P$. Since no global observational data exist on the local values of $\dot{\rho}$, our theoretical estimate is currently the only available estimate of $I_h$ (c1). (This paragraph is to be added to the revised manuscript.)

Interactive comment on Atmos. Chem. Phys. Discuss., doi:10.5194/acp-2016-203, 2016.