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Interactive comment

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

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Dr. Tailleux states on p. C5 [doi:10.5194/acp-2017-17-RC1]: "If the equations considered were formulated in terms of the full barycentric velocity, I would agree that this term is non-zero, and physically related to the exchange of freshwater between the land/ocean and the atmosphere, whereby the atmosphere gains freshwater at a higher temperature than it returns it to the land/ocean in the form of precipitation."

Here we, first, clarify the meaning of this statement and, second, explain why it is incorrect. It is related to one of the main points of our article (see abstract): *... confusion between gaseous air velocity and mean velocity of air and condensate ... results in gross errors despite the observed magnitudes of these velocities are very close.*" The

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Discussion paper

caveat, which has apparently escaped the referee's attention despite our repeated emphasis, pertains to the definition of the material derivative: depending on the velocity used in this definition, one obtains drastically different results. The choice is not arbitrary: in the atmospheric context using barycentric velocity as suggested by the referee violates the first law of thermodynamics.

1. Consider air and condensate with velocities $\mathbf{v}$ and $\mathbf{v}_c$ and densities ρ and ρ_c obeying the following steady-state continuity equations:

$$\nabla \cdot (\rho \mathbf{v}) = \dot{\rho}, \;\; \nabla \cdot (\rho_c \mathbf{v}_c) = -\dot{\rho}, \;\; \nabla \cdot (\rho_m \mathbf{v}_m) = 0. \tag{1}$$

Here $\rho_m \equiv \rho + \rho_c$ and the mean velocity of air and condensate $\mathbf{v}_m \equiv (\mathbf{v}\rho + \mathbf{v}_c \rho_c)/\rho_m$ is the so-called barycentric velocity.

If we define the material derivative of enthalpy via barycentric velocity as $dh/dt_m \equiv (\mathbf{v}_m \cdot \nabla)h$, then, using the third equation in (1) and the divergence theorem, for the integral of dh/dt_m over total atmospheric mass $\mathcal{M}$ we have

$$I_m \equiv \int_{\mathcal{M}} \frac{dh}{dt_m} d\mathcal{M} = \int_{\mathcal{V}} \frac{dh}{dt_m} \rho_m d\mathcal{V} = \int_{\mathcal{S}} h \rho_m \mathbf{v}_m \cdot \mathbf{n} d\mathcal{S}. \tag{2}$$

Interpreting $\rho_m \mathbf{v}_m$ in (2) as the mass flux of air and condensate across the planetary boundary and approximating the atmosphere as having a precipitating part where $\rho_m \mathbf{v}_m \cdot \mathbf{n} = P > 0$ and an evaporating part where $\rho_m \mathbf{v}_m \cdot \mathbf{n} = -E < 0$, one obtains that I_m is proportional to the difference in enthalpy of air at the surface between the two regions, $I_m = Ph_P - Eh_E$, where $E = P$ (kg year^{-1}) is the evaporation/precipitation flux[1].

[1] This result, albeit mistakenly with an opposite sign and h (enthalpy of moist air) replaced by the enthalpy of liquid water, was obtained by Dr. Tailleux during an earlier evaluation of our work, see www.bioticregulation.ru/offprint/he3-r2.pdf.

Note that if locally we have $\rho_m \mathbf{v}_m = 0$ (interpreted as local precipitation equals local evaporation), then $I_m = 0$. This runs counter to the reviewer's interpretation of I_m being related to the difference in temperatures of precipitating and evaporating water.

In contrast to I_m, the enthalpy integral I_h calculated in our article is not zero when local evaporation and precipitation coincide. Rather than being proportional to the difference in surface air enthalpies between regions of positive net evaporation and positive net precipitation, I_h is proportional to the difference in enthalpies of moist air at the surface and at the mean height where condensation occurs (see Eq. (60) on page 21).

This discrepancy alone should already provoke some thought. Indeed, as the enthalpy change is constrained by the first law of thermodynamics, apparently only one expression, either I_m or I_h, is correct.

2. The first law of thermodynamics relates mechanical work, heat increment and change in internal energy. Mechanical work in the atmosphere is expressed as $pd\tilde{V}$, where p is ideal gas pressure and $d\tilde{V}$ is the macroscopic expansion/contraction of the considered air parcel (control volume). Therefore, as discussed in Section 2 of our article, work per unit volume per unit time, $(p/\tilde{V})(d\tilde{V}/dt)$, can be expressed as $p\nabla \cdot \mathbf{v}$, where $\mathbf{v}$ is *the velocity of gas which performs work*. The relative change of the control volume occupied by condensate particles, $\nabla \cdot \mathbf{v}_c$, is not related to production of work by ideal gas with pressure p, thus $p\nabla \cdot \mathbf{v}_m$ *is not production of work*.

Since Laliberté et al. (2015) apply the first law of thermodynamics to the atmosphere considered as a mixture of ideal gases, their expression for atmospheric power output – the mass integral of $-\alpha dp/dt$ – must define the material derivative of p using the same velocity as in the expression for work, i.e. the velocity of ideal gas, $dp/dt \equiv (\mathbf{v} \cdot \nabla)p$. The other material derivatives in the first law of thermodynamics, including the material derivative of enthalpy $dh/dt \equiv (\mathbf{v} \cdot \nabla)h$, must be consistently defined using the same, i.e. gaseous, velocity. Using barycentric velocity for the material derivative of h simultaneously with gaseous velocity for work violates the first law of thermodynamics.

(We note in passing that while the referee attempts to defend the neglect of dh/dt by Laliberté et al. (2015), no derivation supporting this conclusion, i.e. that $\int (dh/dt) d\mathcal{M} = 0$, has been presented. The few arguments defending the neglect of dh/dt appear somewhat confusing and controversial. In particular, Eq. (6) on page c6 of the referee's comment could indeed yield $\int (dh/dt) d\mathcal{M} = 0$, but if and only if $\mathbf{v} \cdot \mathbf{n} = 0$. The latter (correct) boundary condition is, however, what the referee objects to, so he apparently cannot have this derivation in mind. Furthermore, as we pointed out in our earlier reply, see page c5 of doi:10.5194/acp-2017-17-AC1, equations (6) and (7) on page c6 of the referee's comment are not the equations actually used by Laliberté et al. (2015) (who applied a velocity correction to Eq. (6)). Ultimately, the referee avoids to specify whether v in his review is the gaseous or barycentric velocity.)

In the physics literature the proposition that the velocity associated with production of mechanical work is not necessarily identical to the mean flow velocity recently stimulated a rigorous discussion which is still on-going, see references in, and Google Scholar citations of, Brenner (2009). That the choice of an appropriate velocity crucially matters for the analysis of the atmospheric power budget is likewise a non-trivial issue, which apparently has never been discussed in the meteorological literature.

References

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