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

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We thank Dr. Garrett for his interesting comment (hereafter EC). The view that latent heat release is more important than the reduced number of gas molecules has been advocated several times during our research on condensation-induced dynamics. In EC this view is offered as a query within a clear physical context. This makes it easier to demonstrate why it is incorrect: in brief, it misinterprets heat as work. Below we also explain how the atmospheric responses of the two effects have distinct time scales and why this matters.

1 Heat vs work

To estimate *the rate of doing work* associated with condensation, Dr. Garrett presents two expressions for what he refers to as the *two microscopic components of work*:

$$\frac{dw_1}{dt} = R^*T\frac{d\ln\alpha}{dt}, \quad \frac{dw_2}{dt} = -L^*\frac{d\ln m}{dt}. \tag{1}$$

Here $\alpha \equiv \tilde{V}/m$ (m^3 kg^{-1}) is the specific volume, $\tilde{V}$ (m^3) and m (kg) are the volume and mass of the atmospheric gases, $L = 45$ kJ mol^{-1} is the latent heat of vaporization, $L^* = L/M_v$, $R = 8.3$ J mol^{-1} K^{-1} is the universal gas constant, $R^* = R/M_v$, $M_v = 18$ g mol^{-1} is molar mass of water vapor, T is temperature.

Since the total atmospheric volume $\tilde{V}$ does not change, we have $dm/m = -d\alpha/\alpha$ and obtain from (1)

$$\left(\frac{dw_2}{dt}\right) / \left(\frac{dw_1}{dt}\right) = \frac{L}{RT} \equiv \xi \approx 18. \tag{2}$$

This ratio is about an order of magnitide smaller than estimated in EC (which assumed $dm/m = -10d\alpha/\alpha$), but still it is much greater than unity. From this EC concludes that "it would seem that of these two microscopic elements of work, the one discussed in the paper is negligible", while latent heat release, which is given by dw_2/dt in (1), dominates.

The problem with this interpretation is that heat and work are two distinct concepts, with the efficiency ε of heat conversion to work central to thermodynamics. To assess how much work w_Q results from latent heat release, we write

$$\frac{dw_Q}{dt} = -\varepsilon L^*\frac{d\ln m}{dt}. \tag{3}$$

In contrast, dw_1/dt is not the rate of heat input; in agreement with *the definition of work*, it describes the expansion of air parcels with pressure $p = RT/V$, where $V = M\alpha$ is molar volume and M is air molar mass. In fact, dw_1/dt is equivalent to work performed per unit time by the air expanding into empty space freed by the condensation of water vapor.

Thus to build a general argument about latent heat being more important than the reduced number of gas molecules, one would need to demonstrate from first principles that $dw_Q/dt > dw_1/dt$, or, which is the same, to show that $\varepsilon > RT/L \approx 0.05$.

Such a proof does not exist. As illustrated by Fig. 2 of Goody (2003), under typical atmospheric conditions ε for heat-driven convection is of the order of 10^{-2} and can vary around zero; negative values are also possible. The reason is that any air circulation is not confined just to the rising portion (where latent heat contributes to the positive buoyancy of the ascending air). It also necessarily includes the descending portion, where the sinking air warms (and thus potentially becomes positively buoyant too). Therefore, if we consider the effect of latent heat alone (i.e. ignore the radiative heat exchange processes), the same heating that makes the rising air parcels positively buoyant, will be a drain on the circulation power when these warm air parcels will have to descend. The net effect of latent heat in a steady-state will be precisely zero ($\varepsilon = 0$, see Gorshkov et al., 2012).

In other words, since pushing warm positively buoyant air parcels down is costly, the power of any circulation based on temperature effects is ultimately limited not by the rate of latent heat release but by the rate at which heat can be disposed of by the descending component of the circulation (Goody, 2003). In particular, when the radiative cooling rates are low, latent heat release serves not as a driver but as a break prohibiting sustained circulation ($\varepsilon < 0$).

Previous versions of the argument about the greater significance of latent heat release, including the commentary of Dr. D. Rosenfeld[1] in 2008 and the more recent

[1]http://www.atmos-chem-phys-discuss.net/8/S12426/2009/acpd-8-S12426-2009.pdf, page S12436

commentary of Dr. A. Kleidon[2], were grounded in a modified relationship (2) of the type $L/(c_v T) \approx 7 \gg 1$, where $c_v = (5/2)R$. This implies warming the atmosphere with latent heat by $\Delta T = \Delta Q/c_v$ and calculating the associated buoyancy increase as $\Delta T/T$ to compare it with $\Delta\alpha/\alpha$. EC is distinct in addressing the ultimate question – circulation power (rate of doing work), rather than the buoyancy of the rising air which may or may not be associated with net positive work of the circulation as a whole.

To summarize, whatever the meaning of dw_1/dt and irrespective of its relevance to large-scale atmospheric dynamics, which, we emphasize, should be justified separately as we do in our article, dw_2/dt is not the rate of doing work. Thus neither $L/(RT) \gg 1$ nor, as a variant, $L/(c_v T) \gg 1$, prove *the greater importance of latent heat*.

2 Time scales

The distinct time scales of the different physical processes associated with phase transitions are crucial for describing moist dynamics (Makarieva et al., 2017). While the time scales of latent heat release and the reduction of the number of gas molecules are the same, the time scales of the atmospheric responses are not.

When water vapor condenses in rising air, the reduction in the number of gas molecules perturbs the vertical pressure distribution. Indeed, owing to the ideal gas law at constant temperature we have $\Delta\alpha/\alpha \sim -\Delta p/p$. Any pressure disturbance leads to an adjustment. Once the vapor is removed from the middle troposphere where condensation has occurred, the lower air must expand to fill the void.

This hydrostatic adjustment has two effects: (1) the pressure deficit shifts downward

[2]Pers. com. to Anastassia Makarieva 05 April 2016 and http://www.hydrol-earth-syst-sci-discuss.net/12/C4945/2015/hessd-12-C4945-2015.pdf, page C4946

producing a horizontal pressure gradient at the surface between the condensation area and the ambient environment and (2) air density is partially restored where condensation has occurred (since the air has risen during the adjustment and partially replaced the condensed water vapor). In the result of the hydrostatic adjustment (even if it is incomplete), the actual density change in the upper atmosphere becomes much less than $\Delta\alpha/\alpha$. In this sense it would be a misnomer to refer to the effect of the reduced number of gas molecules as a "density effect".

In contrast, the density change associated with temperature change, $\Delta T/T$, has a much longer relaxation time scale (governed by heat conduction which depends on radiative exchanges and turbulent air motions) than the hydrostatic adjustment.

Because of these distinct scales, the "slow" temperature effects are, via parameters for radiative exchange and turbulence, automatically included into the standard set of differential equations governing air motion. (In this aspect a moist atmosphere is formally identical to a dry atmosphere with a heat source equal to latent heat release.) The "instantaneous" dynamic effects of condensation must be incorporated manually, for example, via a corresponding constraint on the resulting circulation power.

(A similar situation, as we recently demonstrated, exists with the interaction of the air with condensate particles. If the time scale of this interaction is much smaller than the time scale of air motion, a specific term must be introduced into the equations of motion representing the drag resulting from this interaction (Makarieva et al., 2017). This term is absent in the standard set of equations and cannot be deduced from them. It is incorporated *ad hoc* based on a separate consideration of the relevant physical processes.)

We thank Dr. Garrett once again for interesting comments and would welcome any further opportunities to clarify these relationships. We address the query about cloud physics and models in a separate comment.

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