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Atmospheric Chemistry and Physics Discussions

Interactive comment on "On the validity of representing hurricanes as Carnot heat engine" *by* A. M. Makarieva et al.

A. Meesters

antoon.meesters@falw.vu.nl

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This new short comment pertains to S8923 (Rep1) which is a reply to the part of my first short comment S8916 (SCM1) that deals with section 4 of the the discussion paper (DP). I much admire the astonishing pace with which Dr. Makarieva produces her replies. However, some of my statements are somewhat misrepresented in Rep1.

Let me first explain why I described a laboratory experiment. This was not done to mimic processes just as they happen in the atmosphere, but to fix some parameters in such a way that the relations between the non-fixed parameters come out more clearly. This is a usual way to demonstrate thermodynamic laws, and it has also been the way in which these laws (which were subsequently applied to the atmosphere) have been discovered. If it is a fundamental law that the condensation of water vapor causes a pressure drop equaling the partial vapor pressure of the part of the vapor that has condensed, then this law should hold not only in the free atmosphere but also in the laboratory. The law describes a relation between condensing vapor and falling pressure, and has been formulated without any reference to saturation-equilibrium (it is combined later with assumed saturation only to find out how much vapor is condensing). The question how the outcome of the experiment can be reconciled with the law assumed in the DP remains to be answered.

It is true that the condensation from supersaturated air is a non-equilibrium process, but this does not distort the computation as only the universal law of energy conservation and the gas law are applied. Concerning the remark about the universal Clausius-Clapeyron equation, that equation cannot be applied to super-saturated vapor.

I do not understand why the interpretation of c_v as the specific heat at constant volume would be wrong. The amount of gas does not change, except for the depletion of water vapor which has a minor, second order effect on the heat capacity. By the way, this does not seem to have consequences for the remainder of the discussion.

I assume that the accompanying remark about using c_p instead of c_v refers again to the supposed uselessness of the laboratory experiment.

Let us now turn to the free air. As air ascends, it cools (as the air expands and work is done at the expense of the internal energy). I fully agree that this also holds for wet air (this is also nowhere denied in SCM1). What I said in the last-after-one paragraph about the effects of condensation should be understood of course in differential sense: the difference with the effects in dry air is understood, since this is the critical point. I claim that pV (with V the parcel volume) tends to become larger with than without

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condensation, if air is lifted to some level. We agree that with condensation, the cooling goes slower than without, and that the difference is caused by release of latent heat in the former case. So T for wet air is higher than T for dry air lifted to the same height, and the more so as more vapor has condensed. I belief that it is instructive now to invoke the universal gas law

$$pV = NkT$$

in which V is the parcel volume, N the number of molecules in gas/vapor state, and k is the Boltzmann constant. When we compare rising parcels with and without condensation, we obtain from this

$$(pV)_{wet}/(pV)_{dry} = (N_{wet}/N_{dry})(T_{wet}/T_{dry})$$

As the parcels are lifted, N_{wet}/N_{dry} decreases (since in the wet parcel, water molecules are leaving the vapor state), whereas T_{wet}/T_{dry} rises by the release of latent heat in the wet air. Which effect will dominate? From a standard equation for the dry and the wet (saturated-adiabatic) lapse rate (e.g. Wallace and Hobbs section 2.6.3) we have

$$(dT/dz)_{wet} - (dT/dz)_{dry} = -(L_v/c_p)(dq_{sat}/dz)$$

It follows that for one g/kg of condensed water vapor ($\Delta q_{sat} = -10^{-3} kgkg^{-1}$) the rise of $\Delta T_{wet} - \Delta T_{dry}$ is 2.5 K, so T_{wet}/T_{dry} rises by 8.3 x 10^{-3} . On the other hand, with 10^{-3} of the mass condensed, the fraction of disappeared molecules is 10^{-3}/0.622 (accounting for the lower mass of the water molecules compared to dry air) so N_{wet}/N_{dry} falls by about 1.6 x 10^{-3}.

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It is seen from this somewhat cumbersome exercise that the rise of T_{wet}/T_{dry} goes faster than the fall of N_{wet}/N_{dry}. So pV becomes larger for the wet than for the dry parcel. Hence, either the pressure p or the volume V or both must be higher for wet air than for dry air (for the same lifting). And this shows that the effect of condensation, by latent heat release, has more the form of an "explosion" than of an "anti-explosion". This is qualitatively again the same result as what was already found from the closed-vessel calculation, where some artificial constraints were made just to lower the number of varying parameters.

Concerning the remark that it is a "physical misconception that the release of latent heat results in warming": it will be clear from the above that, first, the latent heat release does result in warming e.g. in simple laboratory experiment discussed in SCM1. (after all, the latent heat has not been called "heat" without a reason), and second, that if we apply it to the free atmosphere it is understood as differential warming, compared to what would occur if no condensation took place. The notion of equivalent temperature has played no role.

The paragraph about the effect of condensation on pressure is most interesting, as many people are curious about the implications of the new treatment proposed in section 4 of the DP. I am surprised that my formulation of the standard viewpoint "the pushing force in upper layers causes local thinning of the air, but this causes lower pressure and hence horizontal convergence in the lower layers" was immediately accepted as a fact (of course I meant that the lower pressure occurs in the lower layers). Few lines later it is also accepted that the mechanism proposed in the DP "would imply a horizontal contraction and densification in the upper layers, and hence (by the greater weight of the column) a larger pressure in the lower layers". This is just the converse of the preceding standpoint! It is also the converse of what one observes (hurricanes are accompanied with notoriously low surface pressure). Only the consequences for the direction of the air current are questioned.

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I do not understand much of the remainder of the paragraph (about densification etcetera), this will probably not be important for the discussion.

Interactive comment on Atmos. Chem. Phys. Discuss., 8, 17423, 2008.