Atmos. Chem. Phys. Discuss., 8, S8923–S8926, 2008
www.atmos-chem-phys-discuss.net/8/S8923/2008/

Atmospheric Chemistry and Physics Discussions

ACPD

8, S8923–S8926, 2008

Interactive Comment

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

A. M. Makarieva et al.

Received and published: 10 November 2008

We are grateful to Dr. Meesters for his critical comments (herefrom SCM) and will address them all. Here we would like to comment on the major issue concerning the novel physical approach that we propose. It is argued (SCM, p. S8920) that condensation of water vapor does not lead to any drop of air pressure because latent heat released during condensation warms the air and its pressure ultimately rises. So, according to the comment, condensation warms.

In the atmospheric context this conclusion is fundamentally incorrect. Following Clausius-Clapeyron equation (not mentioned in the comment), saturated partial pressure of water vapor decreases with decreasing temperature and increases with increasing temperature. The picture outlined in the comment is precisely the opposite: air has warmed, but it has a smaller amount of saturated water vapor, since it is de-

Full Screen / Esc

Printer-friendly Version

Interactive Discussion

Discussion Paper

manded that $dq < 0$ (q is water vapor mixing ratio). We would appreciate any further clarifications from Dr. Meesters as on whether the conventional meteorological paradigm implies that a warmer atmosphere should generally contain less saturated water vapor.

The notion of super-saturation pertains to a special case and refers to particular conditions. Indeed, maximum saturated concentration depends on whether water vapor finds itself in contact with spherical drops or planar water surface as well as on whether there are many/few condensation nuclei in the air. In this sense clean atmospheric air can contain more water vapor at a given temperature than dusty air. It is stated in the comment "now let a part of the water vapor condensate". However, condensation does not occur by itself. In order to initiate condensation in the supersatured water vapor without lowering temperature one has to apply some mechanical disturbance. Relaxation of the super-saturated state (e.g. by introducing extra condensation nuclei into a vessel filled with purified air saturated with water vapor) is an essentially non-equilibrium process, which cannot be described in the way it is done in (SCM) applying equilibrium thermodynamics. Importantly, defined for any particular set of conditions, the Clausius-Clapeyron dependence of maximum water vapor partial pressure on temperature retains its fundamental character.

In the approach that we propose it is considered what happens when the rising air parcel saturated with moisture enters the upper colder atmospheric layer, that is, when its temperature is ***already lowered***. (In this case $dT < 0$ from the very beginning, so in fact air pressure lowers by even a greater amount than dp_v, where dp_v is partial pressure of condensed water vapor.) As is well-known, the release of latent heat only changes the magnitude of the vertical lapse rate of air temperature (moist adiabatic lapse rate is smaller than dry adiabatic lapse rate), but it does not in any way lead to absolute warming of the atmospheric air in the upper atmosphere as compared to the lower atmosphere. The release of latent heat only changes the amount of heat that it is needed to take from the system to decrease its temperature by a given amount.

ACPD

8, S8923–S8926, 2008

Interactive Comment

Namely for this reason moist adiabatic lapse rate is smaller than the dry adiabatic one.

We note that Dr. Meesters incorrectly interprets c_v as "specific heat at constant volume" (p. S8920), which (volume), as the text of the comment presumes (p. S8920, third paragraph from bottom), is thought to be preserved by the constant volume of the vessel. The value of c_v refers to specific heat at constant ***molar*** volume, i.e. volume occupied by one mol (or one unit mass) of the considered gas. When vessel volume is constant, but the amount of gas in it changes, molar volume is not preserved, and there is no justification for using c_v instead of c_p (specific heat at constant pressure), provided that the considered pressure changes are small.

We believe that this physical misconception, that the release of latent heat ***results in warming*** (rather than ***partially*** offsets the cooling that initiated condensation), owes itself to the wide formal use of the notion of the so-called "equivalent temperature" T_e defined as "the temperature that an air parcel would have if all water vapor were condensed at constant pressure and the enthalpy released from the vapor used ***to heat the air***" (Glossary of Meteorology, amsglossary.allenpress.com). Notably, by definition one has $T_e = T + L_v q/c_p$, where q is the initial mixing ratio of ultimately condensed water vapor. With the account of the incorrect replacement of c_p by c_v in (SCM) as noted above, change of temperature $dT = -L_v dq/c_p > 0$ as described in (SCM) precisely corresponds to the definition of equivalent temperature, $dT = T_e - T$ (with $q = dq$). Although where equivalent temperature is defined one is warned that ***"This process is physically impossible in the atmosphere"*** (Glossary of Meteorology, amsglossary.allenpress.com), in practice this warning appears to be equally widely neglected (see also the authors' response to Dr. Barbosa (HESSD 2007 3: S1492) in the discussion of Makarieva, Gorshkov (2008) HESS 11: 1013)).

It is noted in (SCM) that "Also, the effect of condensation on pressure does not act merely locally, as the authors of the DP are inclined to see it: 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." This is indeed so. We refer to our comment

ACPD

8, S8923–S8926, 2008

Interactive Comment

Full Screen / Esc

Printer-friendly Version

Interactive Discussion

Discussion Paper

(Makarieva et al. (2008) ACPD 8: S8904) where it is shown that condensation and the related pressure gradient force induces horizontal circulation on a variety of scales. Another statement in (SCM) that the mechanism we propose "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" is also correct. But it does not in any way imply that it will result "in a circulation which is the reverse of what we observe". Densification in the upper layers will be present in any circulation pattern where air flows along a close trajectory. Namely this densification causes air in the upper layers to flow in an opposite direction compared to the surface air. This is a trivial consequence of the continuity equation, which bears no relevance to the driving mechanism of the circulation.

Once again, we thank Dr. Meesters for his critical comments. They are rewarding for the authors indicating quite unambiguously that even a very critically inclined observer considers our results as entirely new to "the conventional wisdom" (our quote of Dr. Meesters). They are also very valuable for us as the first specific criticisms of the physics of the proposed approach in the present discussion (see, however, discussion of Makarieva, Gorshkov (2007) HESS 11: 1013). We will be delighted to respond to any further concerns.

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