**Atmospheric
Chemistry
and Physics
Discussions**

Interactive
Comment

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

Received and published: 10 November 2008

The discussion paper (DP) is vividly written, and its viewpoint that phenomena should be explained starting from the fundaments is attractive. Nevertheless the paper disappoints me in several ways: (1) the obscurity of some of the crucial steps, (2) the careless (and often erroneous, see specific remarks) way in which existing knowledge is handled, or neglected, and (3) the way in which much previous research is criticized without good reason (see specific remarks).

When reading the discussion, I was surprised that some (not all!) contributors praise the physical rigorousness of the DP, which is then opposed to the so-called lack of such a rigorousness with the authors which are attacked in the DP. It has even been

Full Screen / Esc

Printer-friendly Version

Interactive Discussion

Discussion Paper

suggested, based on the impression made by the DP, that current meteorology as a whole is not in tune with "deep fundamental science". The DP is a rather weak base for this accusation. From what I read there (see specific comments) I must conclude that the DP is itself not too well in tune with fundamental science.

On the other hand, I cannot recognize the picture, emerging in the discussion, of a chronic gap between current meteorological science and fundamental physics (these remarks I address to readers for whom they are not already obvious). The suggestion that everything would change if meteorology and theoretical physics were confronted with each other raises too great expectations. I am myself a theoretical physicist, and I have been doing meteorological modeling and peer-reviewing since many years, but I cannot say that the physics with which I was educated was superior (as far as it is useful) to the physics which is now already used in meteorology. The real problem of current meteorology is not that the basic laws are neglected, but that the basic laws are nothing but a very first step to understand complex systems.

The following specific remarks concentrate mainly on subjects which have not already found much attention in the discussion. Special attention will be paid to the new theory of the authors developed in section 4.

about section 2

The major statement of the DP is that the models come into conflict with the laws of thermodynamics (see abstract). For this I find no evidence in the paper. The proof would rely on section 3.4, "quantifying heat loss to space", where it is allegedly shown that the heat released by condensation cannot be balanced by the radiation to space since that would require T_0 on top of the atmosphere which would be higher than T_s at the surface, contradicting both observations as well as the theory of hurricanes as

Full Screen / Esc

Printer-friendly Version

Interactive Discussion

Discussion Paper

Carnot heat engines. However, this argument has been refuted correctly by Referee 1 (S7915, "energy loss to space"): the radiation to space comes from a cirrus cap which has a far greater horizontal extension than the region where condensation heat is released, so the cloud cap can yield sufficient radiation in spite of its low temperature. By this the most important claim of the paper has been refuted.

I will not comment on the discussion about the theory of the hurricane as a dissipative heat engine, which is interesting but subtle. It seems to me that the discussion is essentially about good book-keeping whereas definite flaws in any part of the conceptual model are not pointed out.

about section 3.1

The DP makes the odd claim that "from Eq. (E15), it can be concluded that hurricane cannot exist". This point is emphasized by the authors later on in the discussion (S8193, last paragraph). The authors are erring on this point. Their argument is that when Emanuel (1991) integrates the Bernoulli equation (E1) the contribution of the wind speed v is neglected in the result. However, this neglecting is acceptable as the contribution to the integral is $(v_c^2 - v_a^2)/2$, with c the hurricane center and a a point outside the hurricane. Such a contribution would be typically of order 10^2 m^2 s^{-2} or less. The contribution of the integral of αdp and Fdl is typically of order 10^4 m^2 s^{-2} for a hurricane, so neglecting the contribution of v is perfectly acceptable. This excludes by no means the existence of hurricane-force wind speeds within the trajectory, such speeds only make no contribution to the integral as they rise but fall again. (Possibly the error arose because the authors of the DP have read $d(v^2/2)$ as $(v^2/2)dl$, but I am not sure about this).

about section 3.3

Section 3.3 of the DP ("Estimating of dissipative heating") is a side-way, but still it looks rather harmful for the reputation of the researchers cited there. However, those authors have only applied a well-established principle: by combining wind speed gradients and eddy viscosity, one can calculate the production rate of the turbulent kinetic energy (TKE). Now, since this energy is never converted back to the kinetic energy of laminar flow, and since it is also not accumulated for a long time, it must be equal (in the long run) to the viscous dissipation of energy ("production equals dissipation": page 64 in Tennekes and Lumley 1990; equation 31.3 and the accompanying explanation in Landau and Lifshitz 1987; and many other handbooks). This implies that combining turbulent viscosity with (known) macroscopic wind speed gradients, yields the same dissipation rate as combining molecular viscosity with (unknown) microscopic gradients. Thus the approach of the attacked authors is well founded.

The DP claims that (1) turbulent friction forces are unrelated to dissipation into the thermal energy of chaotic molecular motion; (2) not the turbulent but the molecular viscosity should be combined with the macroscopic gradients. These errors in the DP lead to underestimating the dissipation rate by a factor 10^8. However, the DP now accuses Bister and Emanuel (1998), and subsequent papers, of over-estimating the dissipation 10^8 times. This is illustrative for the carelessness with which the authors of the DP criticize the work of other people.

about section 4

Here it is tried to propose a novel theory for (a.o.) hurricane formation. I am not convinced that the old theory is so bad, but let us look at the new theory.

Interactive
Comment

Full Screen / Esc

Printer-friendly Version

Interactive Discussion

Discussion Paper

This theory explains the origin of pressure differences by assuming that when condensation occurs, the local pressure drops with an amount equaling the partial pressure p_v of the water vapor that has condensed.

This theory is surely attractive, if it is true, and in that case there is enough reason to wonder why it has not been proposed earlier. But if you attach so much weight to agreement with thermodynamic laws as the authors do when discussing the work of other people, it would seem that the present theory should be also investigated from this viewpoint before further application. The following simple exercise (in which only the dominating terms are retained, as usual in a test calculation) casts serious doubt on the theory.

The notation is as follows: q = mixing ratio, p = air pressure, p_v = partial vapor pressure, L_v = latent heat of condensation = 2.5 x 10^6 J kg^{-1} , c_v = specific heat at constant volume = 717 J kg^{-1} K^{-1} , T = temperature in K, ρ = specific mass, R = gas constant for dry air = 287 J kg^{-1} K^{-1}. The employed equations are explained in Wallace and Hobbs (1977) and similar handbooks.

Let us start with a thought experiment with a super-saturated air parcel, and let it be contained in a vessel to rule out distorting volume-changes (which are also neglected in section 4). There is no heat exchange with the surroundings.

Now let a part of the water vapor condensate, such that the change of mixing ratio is $dq = -10^{-3}$ kg/kg. The question is: what happens to the pressure in the vessel?

According to section 4, the answer is easy: the change in partial pressure p_v corresponding to the condensed vapor is $dp_v = pdq$ /0.622, and dp must equal dp_v so that

Full Screen / Esc

Printer-friendly Version

Interactive Discussion

Discussion Paper

dp = - 160 Pa. (Discussion Paper theory)

My concern is that section 4 does not consider that a heat amount of $-L_v dq$ is released by the condensation. Does this make much difference? This heat will be used mainly to warm the air according to $c_v dT = -L_v dq > 0$. As a result, the pressure of the air rises according to the gas law $p = \rho RT$ (neglecting the water vapor), which implies at a constant volume: $dp = \rho R dT = (p/T)dT$, so that $dp = -(L_v/c_v)(p/T)dq$. This yields for standard pressure and T = 300 K:

dp = + 1160 Pa. (Thermodynamics)

This is a very different result: contrary to what the DP claims, condensation does not cause a decrease but an increase of the pressure. The "annihilation" of the water vapor causes by itself a decrease (neglected in the coarse calculation), but the opposite effect of the condensation heat on the pressure dominates strongly over this minor effect.

In reality, the air is not contained in a vessel, but the DP-model cannot be relied on for difficult atmospheric cases if it cannot yield the good sign for a simple laboratory case.

The conventional wisdom is that in the real atmosphere, the condensation heat causes expansion of the air, hence a lowering of the specific density, hence a lesser weight of the air column, and this (together with surface heat flux) causes the pressure drop in the lower layers. From the viewpoint of dynamic meteorology, condensation is not an "anti-explosion" or "black hole" as claimed in section 4, but rather a kind of explosion. 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.

Interactive
Comment

Full Screen / Esc

Printer-friendly Version

Interactive Discussion

Discussion Paper

I find this conventional wisdom better in agreement with fundamental laws of physics, and better suited for explaining the phenomena, than the novel theory of the DP. The idea of an "anti-explosion" proposed in section 4, does not seem to me very promising to explain the observed phenomena. Besides of being based on a neglect of elementary thermodynamics, it 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, resulting in a circulation which is the reverse of what we observe.

references

Bister, M., and Emanuel, K.A.: Dissipative heating and hurricane intensity. Meteorol.

Atmos. Physics 65, 233-240. 1998.

Emanuel, K.A.: The theory of hurricanes. Annu. Rev. Fluid Mech. 23, 179-196. 1991.

Landau, L.D., and Lifshitz, E.M.: Course of Theoretical Physics, volume 6, Fluid Mechanics. Pergamon Press, Oxford, 1987

Tennekes, H. and Lumley J.L. : A First Course in Turbulence. The MIT press, Cambridge, 1990.

Wallace, J.M., and Hobbs, P.V.: Atmospheric science, an introductory survey. Academic

Press, Orlando etc. 1977.

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

Interactive Comment

Full Screen / Esc

Printer-friendly Version

Interactive Discussion

Discussion Paper