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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.

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We thank Referee 1 for his/her further comments (herefrom RC2). Before turning specifically to them, we would like to emphasize that we consider our critical statements made in the original discussion paper to be logically complete and self-sufficient. However, what we learnt from the referees' comments, what might seem sufficient from a physicist's viewpoint (e.g., Nefiodov 2008, ACPD 8: S8164) might be insufficiently detailed for a broader ACPD readership. It is for this reason that we undertook a further and wider analysis of the hurricane model that we criticize. In our view, so far some of our explanations have been persistently neglected, which, we believe, is the sole foundation of the ongoing argumentation in favor of the fundamentally incorrect concept of the dissipative heat engine by Referee 1.

We first mention another point. It is a misunderstanding that we accept that "there is cooling, albeit at a lower temperature than stated by Emanuel" (RC2). We maintain that detailed calculations of cooling rates are essential to decide whether hurricanes can be described as a Carnot cycle or not. That the problem of cooling rates is important was originally indicated by the large value of latent heat flux released within the hurricane, as estimated in the discussion paper. In our previous comment (Makarieva et al. 2008, ACPD 8: S7949, herefrom AC2) we indicated that, additionally, there is the problem of choosing the outflow temperature, which is on average 265 K for the tropics. This is the stationary value. Referee 1 indicates that in fact hurricanes are associated with cloudiness which emits to space radiation at 200 K. This raises further problems, because the greenhouse effect at high temperature (300 K) for the cloudy sky becomes very high corresponding to decrease of thermal radiation into space and net heating of the atmosphere. In the meantime, hurricane viewed as a Carnot cycle implies that, compared to the usual outgoing flux, some extra heat flux (presumably extracted from the sea) is lost to the atmosphere. We appreciate the referee's willingness to solve all these issues in his brief notes, but in our view these interrelated problems are not the ones to be handwaived but need to have been solved quantitatively and consistently by the model's author while building the model.

We now turn to the discussion of the dissipative heat engine which forms the basis of the hurricane model as presented by Emanuel (2003, Annu. Rev. Earth Planet. Sci., 31: 7). In (RC2, p. S8171) a statement is put forward that the cornerstone of the authors' presumed misunderstandings of Emanuel's framework is the use of the equality $\Delta S = \Delta Q/T$ instead of the inequality $\Delta S \geq \Delta Q/T$ as implied by the second law of thermodynamics for irreversible processes. In brief, this statement is in contradiction with the earlier comments of the referee and is irrelevant to the considered processes. Referee 1 stated in his/her first comment (RC1, p. S7916): "As frictional dissipation is an **irreversible process**, it corresponds to a net entropy production, given by A/T_A, where T_A is the temperature at which dissipation occurs." Taking into account that work A in the dissipative heat engine dissipates to heat $\Delta Q_A = A$, it is clear that in (RC1) the same formula $\Delta S = \Delta Q_A/T_A$ is used against which the referee opposes in (RC2).

ACPD

8, S8193–S8198, 2008

Interactive Comment

Further on, when formulating the second law of thermodynamics presumably including the **irreversible** process of frictional dissipation Referee 1 uses the equality (RC1, Eq. 2, p. 7916), rather than the inequality, which is again in contradiction with the statement of (Eq. 2, RC2).

We understand it such that the discussion of the two cylinders with gas (p. S8171-S8172 in RC2) is also meant to argue against the $\Delta S = \Delta Q/T$ relation. However, both examples strictly conform to this relation. In the first example, the cylinder is compressed adiabatically, $\Delta Q = 0$, so that the increase of internal energy of gas is equal to the work performed on gas, $\Delta U = A$. Indeed, entropy does not change, and this is consistent with $\Delta S = \Delta Q/T = 0$, since $\Delta Q = 0$. In the second example, one stirs gas in the cylinder imparting kinetic energy to gas in the same amount A. Then one waits until this kinetic energy dissipates to the energy of thermal motion of gas molecules, i.e. to heat, $\Delta Q = A$. For this reason, in the second case $\Delta S = \Delta Q/T = A/T > 0$, i.e. entropy increases. Here it is useful to recall that entropy of ideal gas is a state property, that is, it only depends on gas temperature and pressure. Thus, instead of imparting kinetic energy A first, then waiting until it dissipates to heat, one could simply warm the gas by the same amount $\Delta Q = A$, to obtain the same entropy change. Note that the final states in the two examples (adiabatic compression versus stirring gas) will be different. Dissipation of work (i.e. disappearance of ordered energy) is accompanied by heat increment. Heat increment (i.e. the increase of energy of thermal chaotic motion of gas molecules) is the product of work dissipation. In daily words, friction is associated with heating. The second process described by Referee 1 (gas stirred in the cylinder) is, in its second part when work undergoes dissipation, not adiabatic, as it involves heat increment $\Delta Q = A$.

For Carnot cycle the origin of the incoming heat is irrelevant and is never discussed (whether this heat originates from dissipation of work, heat conductivity, radiation decay etc., all of which are irreversible processes). Everywhere in the consideration of Carnot cycle one uses, as is well-known, exact equalities for entropy change of the type $\Delta S =$

ACPD

8, S8193–S8198, 2008

Interactive Comment

$\Delta Q/T$. For the dissipative heat engine (that is based on Carnot cycle) the only linkage with irreversible processes is the obvious statement that work A dissipates to an equal amount of heat $\Delta Q_A = A$ (energy conservation). Thus, the statement of Eq. (2) in (RC2) is, besides being contradictory to the earlier comments of the referee, irrelevant to the problem under consideration.

The referee characterizes as a "new claim" our statement that the regeneration of heat to work within the dissipative heat engine must involve a decrease of entropy. The referee states that the regeneration of heat ΔQ_A back to work A "occurs through a combination of (reversible) adiabatic expansion and compression, in which case the entropy is constant" (RC2, p. S8172). **This reflects the central misconception about the considered heat engine, which we have been trying to explain in several ways starting from the discussion paper (DP) and through comments (A1) and (A2).** We repeat (AC1, AC2) that Eqs. (1) and (2) in (RC1) (the stationarity of both energy and entropy in the dissipative heat engine) cannot be simultaneously satisfied:

$$\Delta Q_s - \Delta Q_0 = 0, \tag{1}$$

$$\frac{\Delta Q_s}{T_s} - \frac{\Delta Q_0}{T_0} + \frac{\Delta Q_A}{T_s} = 0. \tag{2}$$

In order to satisfy both, two conditions must be met. First, heat ΔQ_A resulting from dissipation of work A, as described by Eq. (2) (RC1), has to be converted back **to the same amount of work** A. Second, this must be done **with no change of entropy.** This is what the statement of Referee 1 quoted above implies. Only in this case Eq. (2) for entropy remains unchanged and Eq. (1) for energy becomes $\Delta Q_s - \Delta Q_0 + \Delta Q_A - \Delta Q_A = 0$, i.e. it remains unchanged as well. However, **it is impossible** to convert heat to an equal amount of work without any change in the environment (remember that the environment (= atmosphere) of the dissipative heat engine is stationary), this is

ACPD

8, S8193–S8198, 2008

Interactive Comment

prohibited by the second law of thermodynamics as formulated by Lord Kelvin. When there is no change of entropy, there must be loss of energy (heat) (Carnot cycle), but in this case the amount of regenerated work will be less than A and the energy conservation equation (1) will be broken. That is why the dissipative heat engine is a perpetual motion machine of the second kind.

This absolutely general statement can be illustrated more specifically by considering the particular processes that constitute the Carnot cycle. We repeat (AC1, S7331-S7332) that Carnot cycle consists of two isotherms and two adiabates. On the first, warmer, isotherm **all heat** that is added to the cycle is converted to work A_1 performed by the gas: $\Delta Q_s = A_1 > A$. (On the colder isotherm heat is lost and work is exerted on the gas, decreasing the net amount of useful work in the cycle down to $A = \varepsilon \Delta Q_s$). For the dissipative heat engine, where work A is converted to heat $\Delta Q_A = A$ and added to the cycle at the temperature of the warmer isotherm $T = T_s$, the equation for the warmer isotherm is $\Delta Q_s + \Delta Q_A = A_1$. This means that work A is first dissipated to heat Q_A and then converted back to work (as part of A_1) at one and the same temperature T_s. This physically impossible process makes the dissipative heat engine a perpetual motion machine of the second kind, irrespective of what processes might happen later in the cycle. (The mere presence of a temperature difference within the engine is not an automatic guarantee against the violation of the laws of thermodynamics, cf. (RC2, p. S8172, last paragraph)). So, the net entropy change that occurs due to dissipation/regeneration of work within this engine is zero at the warmer isotherm $T = T_s$, in accordance with what we said in our previous response (AC2, equation on p. S7949).

The above critique (appreciation of which demands from the reader no more than the knowledge of the bases of thermodynamics rather than any specialised literature) pertains to all publications where, according to Referee 1, the same concept of the dissipative heat engine was discussed.

Regarding Section 3.1, we noted in our previous response (AC2) that we consider it

ACPD
8, S8193–S8198, 2008

possible to drop some part of this section from the revised version of the paper. The reason is that we have shown that the central equation of Emanuel (1991) is incorrect, which makes any further specific discussion of this model redundant. Surprisingly, it is stated in (RC2) that "the authors can only argue that the Emanuel framework overestimate the efficiency." We repeat (AC1, p. S7328, AC2, p. S7953) that the central equation of the model of Emanuel (1991, Annu. Rev. Fluid Mech. 23: 179) is incorrect. It is obtained by an incorrect treatment of the processes of Carnot cycle and incorrect integration of Bernoulli's equation (DP, p. 17428). **In his/her three sets of comments Referee 1 has never mentioned these issues.** The model, incorrect as it was, was further synthesized with the dissipative heat engine concept that conflicts with the laws of thermodynamics. In our view, at this point there is no consistent "Emanuel framework" (our quotes of Referee 1) that could be further criticized. We believe that the focus of this discussion is hurricane physics; in our paper we have offered for discussion a physical concept that, to our understanding, could be productive.

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ACPD

8, S8193–S8198, 2008

Interactive Comment