Atmos. Chem. Phys. Discuss., 8, S7947–S7953, 2008
www.atmos-chem-phys-discuss.net/8/S7947/2008/

Atmospheric Chemistry and Physics Discussions

ACPD

8, S7947–S7953, 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: 4 October 2008

We are grateful to Referee 1 for evaluation of our statements; we hope that this discussion will continue to be informative, because the issues raised are, in our view, fundamental.

We believe that the preliminary criticisms of Referee 1 have helped us a great deal in shaping our message; we are intending to use our response (Makarieva et al. 2008 ACPD 8: S7325, herefrom AC1) in the revised version of this paper. However, in our opinion, in the present comments of the referee (herefrom RC1) several key points of our paper, including those that we undertook much effort to clarify in (AC1), were not taken into account. This has led to the formulation of Eq. (2) in (RC1), which is fundamentally incorrect.

Re: Dissipative heat engine

Indeed, in the considered dissipative heat engine, which does not perform any work on its environment, the heat received ΔQ_s and heat lost ΔQ_0 coincide,

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

see Eq. (1) in (R1). We are explicit about it in Eq. (13) in (AC1). (Note that for brevity sake in (AC1) we did not use Δ symbols at heat Q terms, so in Eq. (13) in (AC1) $Q_{out} \equiv \Delta Q_{out} = \Delta Q_0 = \Delta Q_s$.)

Further on, work A produced by the engine dissipates to heat $\Delta Q_A = A$. Recalling that $dQ = Tds$, where dQ is heat increment and ds is entropy increment, if no other processes occurred in the environment where the engine operates, the change of entropy s would indeed be equal to $\Delta s = \Delta Q_A/T_s = A/T_s$, as is stated on p. 7916 in (RC1). But in this case heat would be accumulating in the environment, $\Delta Q = \Delta Q_A = A > 0$.

For this reason, as it is written in (RC1), equation (2) ($T_A = T_s$ is assumed)

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

which is meant to say that the change of entropy is zero, contradicts the energy balance equation, Eq. (1) above and in (RC1). Indeed, Eq. (2) contains a third heat increment term $\Delta Q_A = A$, which is not accounted for in Eq. (1). To agree with Eq. (2), Eq. (1) should have read $\Delta Q_s - \Delta Q_0 + \Delta Q_A = 0$. Note that in the environment which is demanded to be stationary (this is the justification for putting zero in the right-hand side of Eq. (2)), $\Delta Q = 0$ by definition and no heat accumulates.

Where does then the dissipated heat go? This issue is not discussed in (RC1). In the meantime, in the engine considered by Bister and Emanuel (1998) and related works, although work is first dissipated to heat, this heat (in exactly the same amount, so that the net heat increment is zeroed) is converted back to work. This is what is

ACPD
8, S7947–S7953, 2008

Interactive Comment

missed in Eq. (2), which only takes into account the first of the two processes, namely the dissipation of heat from work, but not the (unphysical yet essential for the model!) regeneration of work from heat, which is the key process in the considered approach of Bister and Emanuel (1998) and related works.

We emphasize that this regeneration, which violates the second law of thermodynamics (see pp. S7329-S7332 in (AC1); again, this statement of ours is not commented upon in (RC1)), is the key process in the considered model, because in its absence the environment would not be stationary, with heat accumulating at a rate of $\Delta Q = \Delta Q_A = A > 0$ per cycle.

Thus, the equation for entropy change of the considered engine that takes into account all the essential processes should have been (cf. Eq. (2) above)

$$\Delta Q_s/T_s$$

[increase of entropy due to heat input at the surface]

$$-\Delta Q_0/T_0$$

[decrease of entropy due to loss of heat to space]

$$+\Delta Q_A/T_s$$

[increase of entropy due to dissipation of work to heat $\Delta Q_A = A$ at the surface]

$$-\Delta Q_A/T_s$$

[decrease of entropy due to regeneration of heat Q_A back to work]

ACPD

8, S7947–S7953, 2008

Interactive Comment

$$= \Delta s.$$

The physically contradictory nature of the engine is immediately clear, because the last two terms cancel each other, while the first two ones are not equal (ΔQ_s and ΔQ_0 coincide, while T_0 and T_s do not, see Eq. 15 in AC1). So, despite the claimed stationarity, $\Delta s \neq 0$ and the entropy of the presumably stationary environment decreases! (see item III in AC1 for a detailed discussion).

To summarize, the dissipative heat engine considered in the works Bister and Emanuel (1998, Meteorol. Atmos. Phys., 65: 233), Emanuel (1999, Nature, 401: 665), Emanuel (2003, Annu. Rev. Earth Planet. Sci., 31: 7), Emanuel (2005, Divine wind: The history and science of hurricanes, OUP), Emanuel (2006, Physics Today, 59: 74), namely, the engine that does not receive any net flux of energy from the environment, does not perform any work on the environment, but eternally recycles heat to work within itself, physically classifies as a perpetual motion machine of the second kind.

Re: Energy loss to space

The second point of the comments (RC1) comes against our assertion that the heat released within the hurricane cannot be radiated to space by the atmosphere, as the heat flux exceeds by 20 times the mean flux of outgoing thermal radiation. It is stated (RC1) that in reality heat is released not from a small area near the hurricane center, where actually the highest wind speeds are observed, but from a large area 500-1000 km wide. It is also noted that "cooling and heating rates themselves are irrelevant to the Carnot cycle. Only the total heating and cooling integrated along the trajectory are taken into consideration. The fact that the cooling occurs over a larger area or longer period of time than the heating has no impact on the thermodynamic cycle."

In the theory of Carnot cycle it is presumed that there is a heat source at $T = T_s$ and a heat sink at $T = T_0$, these are the two essential premises. It is not correct that cooling and heating rates are not considered, because it is assumed that all heat that is released per cycle is given away to the heat sink during the same cycle, otherwise extra heat will be accumulating in the environment and the basic equations of Carnot cycle will not hold. Moreover, as we specify on p. S7332, 2nd paragraph from top, in (AC1), consideration of actual heating and cooling rates *irrespective of the theory of Carnot cycle* is absolutely essential to decide *whether* the real hurricanes can be considered as a Carnot cycle, i.e. *whether* there are effective heat sources and heat sinks at the desired temperatures $T_s \approx 300$ K and $T_0 \approx 200$ K.

Our calculation of the high heat release rates in the hurricane area is meant to emphasize the critical importance of such a consideration, which, crucial as it is, cannot be found in any of the works where the hurricanes were described as Carnot heat engine. The above crude estimate suggested in (RC1), which is, to our knowledge, the first one in the literature, is not sufficient to resolve the problem. Assuming that the incoming flux of solar radiation at the top of the atmosphere in the tropics is about 400 W m^{-2} or twice the value absorbed at the surface (Hatzianastassiou et al. 2005 ACP 5: 2847) and taking planetary albedo to be 30%, the outgoing flux of longwave radiation in the tropics estimates as $F_{out} \approx 280$ W m^{-2}. Brightness temperature of the upper radiative layer in the atmosphere is calculated from Stephen-Boltzmann law $F_{out} = \sigma_B T_b^4$, where $\sigma_B = 5.67$ W m^{-2} K^{-4} is Boltzmann's constant, to be $T_b \approx 265$ K. This is the mean temperature of the longwave radiation emitted by the Earth to space and the effective temperature of the atmospheric heat sink, $T_b \approx T_0$. As far as atmospheric pressure drops exponentially with height, radiation from the upper, coldest atmospheric layers with $T \approx 200$ K makes a negligible contribution to the outgoing heat flux in a manner similar to as the high temperatures of the stratosphere are also irrelevant for the effective temperature of the outgoing longwave radiation.

For this reason choosing $T_0 \approx 200$ K as the temperature of the atmospheric heat sink is not plausible. The value of $T_0 \approx 265$ K corresponds to Carnot efficiency $\varepsilon = (T_s - T_0)/T_s = 0.12$, which is three times lower than the mean value of $\varepsilon = 1/3$ used in the considered hurricane models on the basis of assuming $T_0 = 200$ K.

Another important issue is that since, as it is implicitly admitted in (RC1), heat fluxes

F in the hurricane area are dozens of times greater than the absorbed flux of solar radiation, it is easy to show that the main formulae of the dissipative heat engine model produces unrealistically high wind velocities. Although it is stated in (RC1) that in "only the total heating and cooling integrated along the trajectory are taken into consideration", in fact the main formula for hurricane velocity (formula (8) in Emanuel (2003)) operates with heat fluxes per unit surface in the hurricane area. Roughly estimating velocity V_{max} from $F \sim C_D \rho V_{max}^3 \sim 10^4$ W m^{-2}, where $C_D \sim 10^{-3}$ is drag coefficient, $\rho = 1.3$ kg m^{-3} is air density, gives $V_{max} \sim 200$ m s^{-1}, which is unrealistic.

This is to illustrate that a detailed quantitative consideration of heat and cooling rates (currently completely missing in the considered hurricane models) is fundamental for the understanding of the nature of hurricanes. As we argue, the available estimates indicate that apart from the outlined inconsistencies in the theoretical account of Carnot cycle in the current models, hurricanes, quantitatively, *are not a Carnot cycle.*

Re: Efficiency in the Emanuel framework

Thirdly, it is argued in (RC1) that the consideration in Section 3.1 are based on the authors' assumption that $\varepsilon = 1$, which is justified in Section 3.4 by the impossibility of a sufficiently high cooling rate, as discussed above. It is stated (RC1) that since the cooling rate argument is incorrect, the statements that we make in Section 3.1 remain unsupported.

In the previous section of our response we have shown that our cooling rate arguments are directly relevant to the problem under discussion. What is not mentioned in (RC1) is that in Section 3.1 we state that the main formulae in the framework of Emanuel (1991, Annu. Rev. Fluid Mech. 23: 179) are obtained by incorrect integration of Bernoulli's equation. This is also discussed in detail under item II in (AC1). In the revised version of the paper we believe it might be constructive to drop the closing part of Section 3.1 starting from line 17 on p. 17428 and ending with line 20 on p. 17429, and to replace this part by items I and II from (AC1), where the correct formulae for work are derived

ACPD

8, S7947–S7953, 2008

Interactive Comment

and compared with that of Emanuel (1991). Noteworthy, from the correct formula for work (which, prompted by the initial criticisms of Referee 1, we went into great details to derive in (AC1), see Eq. (7) in (AC1)), the central sought-for relationship between p_c and p_a (air pressures within and outside the hurricane), see formula (16) in Emanuel (1991), cannot be derived; formula (16), a major one in the framework of Emanuel (1991), is incorrect. Since in our critique (AC1) we argue in great details that the model of Emanuel (1991) is internally inconsistent, the discussion of the particular values of efficiency used within the model, as it is currently presented in Section 3.1, might indeed be redundant. (Note, however, that the efficiency values of 0.25-0.5 are not realistic in any case, see the cooling rate arguments above that suggest a maximum efficiency in the order of 0.1.)

Returning to the major issue in this response, if the concept of the dissipative heat engine, which, as argued above and in (AC1), represents a perpetual motion machine of the second kind, is discussed in the meteorological literature beyond the aforementioned works of Kerry Emanuel and colleagues (as illustrated already by the literature sources listed in (RC1), p. S7917), the current discussion should be interesting to a wide audience of meteorologists and physicists as well. Since the hurricane problem grows in its daily importance, we believe that an open, interested and responsible discussion of the physical foundations of the atmospheric processes, as made possible by the unique EGU ACPD platform, can be very fruitful in fostering further research. This can be by far more important than the success of any particular critique. Again, we thank Referee 1 for his/her specific and focused attention to our arguments.