

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

Anonymous Referee #1

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Review of **On the validity of representing hurricanes as Carnot heat engine** by A. M. Makarieva, V. G. Gorshkov, and B.-L. Li.

I appreciate the authors last comment, and would like to briefly respond to it. As I said in my review, I believe that most of the criticisms of the Emanuel framework arises from the authors misunderstanding of the framework, and not from any intrinsic problem with the original work by Emanuel. Several of the issues raised by the authors have been discussed in the existing literature. While I realize that the authors may not necessarily be from the meteorological community and may not have been aware of all the previous discussions, this does not excuse them from properly discussing the existing literature in their paper. Furthermore, given the nature of the authors' criticism - that several of Emanuel's papers, written over 2 decades, are fundamentally flawed, their paper

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should be held to a fairly high standard, which, in my opinion, has not been met so far.

The dissipative heat engine framework

The authors seem to be switching their criticism from Emanuel's framework to a wider argument the notion of dissipative heat engine. While the authors are free to do so, they should realize that this is a significant departure from their original paper, and would require at least a major revision.

While the authors and I at least agree on the formulation of the first law of thermodynamics for a dissipative heat engine,

$$\Delta Q_s - \Delta Q_0 = 0, \quad (1)$$

we disagree on the second law. The origin of this disagreement lies, I think, in one of the authors statements in their second comment (AC2) "Recalling that $dQ = Tds$, where dQ is heat increment and ds is entropy increment,..." (S7948). This is a common mischaracterization of the second law. Indeed, Clausius' formulation only states that the change of entropy must be greater or equal to the external heating divided by the temperature:

$$\Delta S \geq \frac{Q}{T}. \quad (2)$$

The equality only occurs for reversible processes. Physical processes are usually irreversible, and associated with a net entropy production. In several statement through their comments, the authors imply that change in entropy are equal to the heating divided the temperature $\Delta S = \Delta Q/T$. In the risk of repeating myself, the second law of thermodynamics states only that the change of entropy must be greater or equal to $\Delta S \geq \Delta Q/T$. The version of the second law used by the authors - in which the inequality in (2) is replaced by an equality, is only valid for reversible processes.

In the dissipative heat engine framework, the work produced by the engine is dissipated internally through frictional dissipation. Frictional dissipation increases entropy, not because it is associated with any external energy source, but because it is an irreversible

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conversion of kinetic energy into thermal energy. To illustrate this, we can consider the two following examples. First, we take cylinder and compress it adiabatically. In this case, the internal energy of the gas in the cylinder increases by the amount of work that was exerted, but is entropy remains constant. Now, we take the same cylinder, but, rather than compressing it, we stir the gas inside the cylinder, exerting the same amount of work as in the first example. We now let the internal motions dissipates. The internal energy increases by the same amount as in the first example, but now the entropy also increases. We have two adiabatic processes in which the same amount of work has been exerted on the cylinder. Yet, we end up with two different final states, with different value of the entropy. For more on this, I recommend the excellent book by de Groot and Mazur "*Non-equilibrium thermodynamics*" (1961).

(The authors also make a new claim that the "regeneration of work from heat" should corresponds to a "decrease of entropy due to regeneration of heat Q_A back to work" (S7949). This is a rather odd claim: this regeneration occurs through a combination of (reversible) adiabatic expansion and compression, in which case the entropy is constant. The implication that adiabatic processes could reduce the entropy of a system would be a violation of the second law. In fairness, I am not sure how much the authors want to stand by their discussion on page S7949, but it is far from convincing. In any case, most of the issues I mentioned above have been addressed in the literature, and the authors choose to simply ignore it.)

Perpetual motion machine of the second kind.

In their comments, the authors claim that "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". This is incorrect. A perpetual motion machine of the second kind is one that exchanges energy with only a single source/sink (i.e. at a single temperature). The dissipative heat engine discussed in Emanuel receives energy from a warm source, and loses it to a colder sink (i.e at a different

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temperature than the energy source). It is therefore not a perpetual motion machine of the second kind.

Cooling and section 3.1

In the original manuscript, the authors claim in section 3.1. that the Emanuel framework "comes in conflict with the first and second laws of thermodynamics" (p. 17429). This is however based on the assumption that there is no cooling ($\Delta Q_0 = 0$ on p. 17429). In their comments, the authors now accept the notion that there is cooling, albeit at a warmer temperature than stated by Emanuel. Once the authors have accepted it, the authors can only argue that the Emanuel framework overestimate the efficiency. This is fundamentally different claim from that made in section 3.1. that there is a "conflict with the first and second laws" which is based on the authors incorrect assertion that the Emanuel's framework implies an efficiency of 1.

Atmospheric cooling

In their reply, the authors argue that the average cooling temperature of the Earth should be on the order of $265K$ (actually, the amount of IR emitted by the Earth corresponds to a black body temperature of $255K$, but this is a minor detail.) They add that the "coldest atmospheric layers with $T \approx 200K$ makes a negligible contribution to the outgoing heat flux". They failed to note that there are large regions of the tropical atmosphere that emits IR at a temperature of $200K$. This is pretty obvious on any IR satellite pictures, and corresponds to high-level cirrus clouds at high level (12-16km) that emits radiation at temperature as low as $200K$. In particular, hurricanes are always associated with such high-level clouds and net cooling at very low temperature.

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

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