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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.

A. M. Makarieva et al.

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Upon completion of the discussion here we describe changes made in the revised manuscript that was submitted to ACP. We thank the discussion participants and the anonymous referees for their valuable comments and critique. We would also like to note that an integral overview of the proposed physical approach to hurricane description is now available as a Short Comment in a related HESS Discussion (Makarieva A.M., Gorshkov V.G. 2009 HESSD 6: S59-S68) at http://www.cosis.net/copernicus/EGU/hessd/6/S59/hessd-6-S59.pdf.

06 December 2008

Dear Editor,

We have carefully studied all comments on our discussion paper "On the validity of representing hurricanes as Carnot heat engine". On the basis of these comments we revised our manuscript for ACP submission. Below all the revision points are specifically addressed.

We thank the Editorial Board for the constructive attention to our paper. We hope that in the revised form the paper will be suitable for publication in ACP. We are looking forward to hearing from you in due course.

Yours very sincerely,

Anastassia Makarieva

(on behalf of Victor Gorshkov and Bai-Lian Li)

List of revisions

I. Carnot cycle

The original paper's structure

(a) general theoretical critique of the consideration of Carnot cycle in the framework of Emanuel (1991, 2003, and others)

(b) specific criticisms on the basis of the general critique

(c) introduction of the condensation-based physical approach to hurricane description

was preserved.

We were advised by Anonymous Referee No. 2 (RC S9081) to include a detailed consideration of Carnot cycle (AC S7325) as an Appendix. However, in our opinion, the discussion showed that the explicit consideration of Carnot cycle, not present either

ACPD

8, S11254–S11259, 2009

Interactive Comment

in the work of K. Emanuel or in the now cited works on the dissipative heat engine, should be more properly placed within the maintext. As Referee 2 also advised to shorten the critique in the maintext, we placed the equations of Carnot cycle into Section 2 as the basis of general critique instead of the general consideration of the laws of thermodynamics originally given in that section. Formal consideration of the laws of thermodynamics was shortened; they were discussed piecewise in subsections of Section 3.

II. Specific criticisms (Section 3)

This section originally contained four subsections devoted to (1) integration of Bernoulli's equation and efficiency estimates in the work of Emanuel (1991); (2) discussion of the vertical enthalpy fluxes and dissipative heat engine formalism in the work of Emanuel (2003); (3) Estimates of dissipative heating; (4) Estimates of heat release to space.

Neither Anonymous Referee No. 1 (S7915, S8170) nor Anonymous Referee No. 3 (S8627) were satisfied with the discussion of efficiency in Emanuel framework made on pp. S17428-S17429 in the discussion paper. The objection was based on the fact that the reason for the derivation of efficiency $\varepsilon = 1$ for Carnot cycle, the result that we criticized, was given in Section 3.4, namely that heat released within the cycle cannot be radiated to space, i.e. $\Delta Q_0 \ll \Delta Q_s$, so $\varepsilon \to 1$. The referees did not admit the existence of such a problem in the framework. We explained in our Final Response (AC S9182), using the novel data of Trenberth and Fasullo (2007), that the problem does exist, see also below. However, we excluded the consideration of efficiency from Section 3.1 (also in line with the recommendation of Referee 2 to shorten the critique) and concentrated solely on the incorrect integration of Bernoulli's equation by Emanuel (1991) and the consequences it had for the framework. We note that Dr. Meesters (S9060), who originally (S8916) did not appreciate our critique on that point, admitted that he was mistaken and that Bernoulli's equation was indeed integrated incorrectly.

ACPD

8, S11254–S11259, 2009

Regarding Section 3.2, we excluded the discussion of vertical flux of enthalpy. This issue was not mentioned in the discussion and is not indispensable for our major conclusions. We concentrated on the concept of the dissipative heat engine. Using the equations of Carnot cycle introduced in Section 2, we aimed to explain the conflict of this concept with the second law of thermodynamics. We avoided using the notion of entropy or whatever equations on entropy stationarity. As the discussion showed (see comments of Referee 1 (S7915, S8170) and Dr. Meesters (S9060), our explanation of this conflict on the basis of such equations (AC S7325) was not perceived as sufficiently clear by the discussion participants. Thus, in the revised paper we derived our conclusion (that the dissipative heat engine is equivalent to perpetual motion machine of the second kind) in the simplest possible form, i.e. solely based on the equation for the warmer isotherm of the Carnot cycle and the formulation of the second law of thermodynamics as the impossibility of heat transfer from the cold to the warm object, the arguments against which no objections were put forward in the discussion.

Responding to the concern of Referee 3 (S8627) who found our statement there need to be "independent physical determinants of oceanic heat input" unclear, in Sections 3.1 and 3.2 we explained in great detail how the Carnot cycle formalism is organized, how many independent variables are involved and that it is not possible to determine the pressure drop within the hurricane without setting the value of heat input independently. We showed that the inconsistencies that we criticize precisely arose as attempts to overcome this inherent limitation of the Carnot cycle approach.

Responding to the concern of Referee 1 (S8170) that we might have ignored the existing literature on the topic of the dissipative heat engine, in the revised text we cited several key papers on the topic, including those listed by the referee. We noted that in none of these papers the processes and equations of Carnot cycle were explicitly considered and that this could be the reason of why the problem with the concept has not been earlier identified.

We dropped former Section 3.3 on dissipative heating from the revised paper. We gave

ACPD

8, S11254–S11259, 2009

Interactive Comment

a detailed response (AC S9342) regarding this issue to Dr. Meesters (S8916, S9060). We feel that the discussion of dissipative heating and of the existing approaches to its estimate can become a topic of a separate investigation. In line with the recommendation of Referee 2 to shorten the critique, we decided to exclude this issue from the revised paper.

Regarding Section 3.3 (former Section 3.4) – in his/her two sets of comments, Referee 1 objected to our statement that the heat released within the hurricane cannot be radiated to space. In the revised version of the paper we used the latest data on hurricane water budget (Trenberth and Fasullo 2007) to estimate flux of latent heat release within the entire hurricane area (radius 400 km). We showed that this flux is at least 19 times greater than the flux of thermal radiation emitted to space from the entire hurricane area. In agreement with the qualitative statement of Trenberth and Fasullo (2007) that hurricane is not a closed system, we quantified that hurricane is open both in terms of exported and imported latent heat.

III. Introducing the new approach

The need to represent the new approach in greater detail was emphasized by Referee 1 in his preliminary comments, who noted that, in his opinion, we did not make an attempt to build a quantitative theory that could be tested. Referee 2 (S9081) explicitly recommended to extend the paper at the expense of including a more detailed account of our approach. Referee 3 (S8627) also wrote that "if the authors have a unified theory for hurricanes and tornadoes they should present that". Following these recommendations and responding to concerns, we have significantly extended section 4 with a detailed quantitative description of the physics of condensation in relation to generation of dynamic air motions. In Section 4 of the paper, while considering hurricanes versus large-scale stationary circulation, we also aim to respond to the question of Dr. Nobre (S8669) on why there are no hurricanes in the Amazon river basin. The extension of Section 4 is made on the basis of the Authors' Comment (S8904) made in response to Referee 2. We also note that much of this material was presented in

ACPD

8, S11254–S11259, 2009

Interactive Comment

Preprint 2763 of Petersburg Nuclear Physics Institute, which was cited in the discussion paper (available in the Internet).

We have found the discussion extremely informative and helpful and we did out best to make full use of all comments that were made available to us. We thank the discussion participants and the anonymous referees for their valuable comments and critique.

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

ACPD

8, S11254–S11259, 2009

Interactive Comment