

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

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We appreciate the statement of Referee 3 that our claim about the violation of the laws of thermodynamics demands a serious justification. We learnt already from the preliminary comments of Referee 1 that our critique, as it appears in the discussion paper (DP), cannot be easily understood by at least two highly qualified representatives of the meteorological community, as the ACP referees undoubtedly are. We then undertook a detailed expansion of our arguments (AC1), which we posted as early as on the next day after our paper was published September, 19th. Any review process normally includes revisions from the authors' side, especially those aimed at better presentation clarity. The ACPD platform makes this process easily accountable. We therefore suggest that the referee might be interested in reading the previous comments in this discussion, as they carry answers to all concerns expressed in the present review.

In particular, the referee suggests that "What is really needed is to step through Emanuel's arguments and show specifically where the errors (supposedly) are". Precisely this is done on p. 1417427-17428 in DP, where it is stated that the integration of Bernoulli's equation by Emanuel (1991), which is essential for the derivation of the main formulae, is incorrect. Further on, in (AC1) a detailed consideration of Carnot cycle is given (which is missing in the works of Emanuel criticized in the DP); the correct formula for work A in the cycle is derived; it is shown that the corresponding formula of Emanuel (1991) (the central one in the framework) is incorrect; finally, it is explained that this central formula is incorrect because it was obtained by incorrect integration of Bernoulli's equation, see pp. S7326-S7329 in AC1. Further on, turning to the dissipative heat engine concept that was introduced into the criticized framework by Bister and Emanuel (1998) and reviewed by Emanuel (2003), we specify in DP that the process of "heat recirculation" conflicts with the laws of thermodynamics. In particular, we showed that if one demands the constancy of entropy, this engine violates the energy conservation law (pp. 17427, 17431 in DP). Further on, in AC1 and further authors' comments responding to Referee 1 we showed that if, on the other hand, one demands the constancy of energy content for this engine, then the second law of thermodynamics is violated (see pp. S7948-7950 in AC2, pp. S8194-S8197 in AC3). We specifically pointed out which process (namely dissipation of work at surface temperature T_s and the resurrection of this work at the same temperature) is responsible for that violation (see p. S7331-S7332 in AC1, p. 8197 in AC3).

The statements about efficiency equal to unity, its relevance to heat loss to space, redundancy of this argument in the revised version of the paper (where we plan to show that the central formula of the criticized framework is incorrect) were discussed in our responses to Referee 1. Here we would like to add one more point. Referee 3 states that "What section 3.4 does argue is pure fallacy, namely that Emanuel's theory somehow implies that a significant fraction of the latent heat flux should be turned into mechanical energy, dissipated, and locally radiated to space. I see no basis for that, and none is given." Indeed, the criticized framework does not imply that, because it

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is based, as we argue, on an incorrect consideration of Carnot cycle and incorrect formula for the cycle's work. In the meantime, as we show on p. 7334 in AC1, see Eq. (16), the outgoing flux of heat into space should be, were the hurricanes a Carnot cycle, largely composed of latent heat.

We appreciate that Referee 3 emphasizes our statement that "there need to be "independent physical determinants of oceanic heat input". It is important and relates to the general logical summary of the criticized framework. As we have shown in AC1, in Carnot cycle heat input Q_s and pressure difference on the warmer isotherm are two interrelated variables. If one knows one, one knows the other (provided there is Carnot cycle). The main target of hurricane theory is to know pressure difference between the hurricane center and its surroundings. For this reason one must know heat input from independent considerations. An equation additional to those of Carnot cycle is needed. For this purpose, in the work of Emanuel (1991) Bernoulli's equation is involved. But this resulted in a physical error, because the equation was integrated incorrectly. Moreover, it was a useless measure anyway, because the equation contained an additional unknown variable, and hence, could not in principle contribute to the sought-for solution. In later works, this approach was abandoned without explicit reasons, but presumably because of the fact that the error was identified. However, the problem of finding one more equation remained. When, in the dissipative heat engine, dissipated work A was added to heat input Q_s , the needed equation was created (the one that violated the laws of thermodynamics). After that, the stationary hurricane velocity was found from the condition that the turbulence term, assumed to be equal to dissipative heating, grows with velocity in a different manner than does heat input: "Comparing Equation 4 with Equation 5 shows that the dissipation increases as the cube of the wind speed, whereas the heat transfer increases only as the first power of the wind speed, so that eventually the dissipation matches energy production and the storm achieves a quasi-steady state." (quote from Emanuel 2003, p. 85). In reality, we emphasize, if freed from the above inconsistencies, a correct consideration of Carnot cycle does not allow one to know both heat input and pressure difference. Heat input

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must be found from independent physical considerations that do not exist.

Referee 3 remarks that he/she can see no way for hurricanes other than to be heat engines and that he/she cannot see any similarity between hurricanes and tornadoes. The referee asks "what other plausible source of energy is there." We have made two specific comments clarifying that this energy is potential energy equal to partial pressure of water vapor. Hurricanes and tornadoes are all accompanied by intense phase transitions (water vapor condensation) during which this energy is released. The amount of this energy, as we quantify, is precisely such as to explain the observed high velocities. Its magnitude is not related to latent heat.

Finally, we would like to note that, in our view, the open discussion platform of the EGU journals sets up a new and high ethical and cultural standard of the peer review process. In this context, statements like "this paper is not worthy of publication in any respectable journals" should be perhaps viewed as atavisms of the background private communication between the editor and the referee during the conventional close review process. When such statements are made in open public discussion potentially read by hundreds of people, especially in the view that the referee cannot follow "much of the argument here", they can be classified as a public assault to both the authors as well as to all those discussion participants who sign their names under very different opinions as well as to the ACPD journal itself (who did publish the paper).

Moreover, in our view, the above statement of Referee 3 goes against the journal's interests not only in its form, but also in its essence. We believe that the main target of this discussion is to reveal the scientific truth. The discussion paper is citable, covered in Scopus and available for analysis. Indeed, we come up with a rare claim that a framework published in high-profile journals is based on the concept of a perpetual motion machine and is fundamentally incorrect. Our arguments are all here. In our view, if our paper were published in ACP, then the responsibility to respond to our critique would go to the author of the criticized framework, as the normal practice in scientific literature goes. If, on the other hand, the ACP declined our paper for publication in the

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second stage, as recommended by Referee 3, future readers of this discussion would ultimately decide whether or not the journal actually signed its official name (while Referee 3 remaining anonymous) among the defendants of perpetuum mobile and against a new approach to hurricane physics. In any case, however, we believe that this discussion has a very substantial value. We are very grateful to the journal for letting us express our views on its pages.

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

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