

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

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The following editor report has been completed and sent to the authors on 27 April 2009. After obtaining the referees consent for including their comments from the non-public part of peer review completion it is now also published online:

Thank you for submitting a revised review of this paper, which has been considered carefully by all three anonymous reviewers. I have appended copies of their comments below.

Two reviewers (1 and 3) are strongly critical of the revised paper and recommend that it be rejected for publication in ACP. The third reviewer (2) is very positive. In order to come to a decision I have considered the paper carefully myself. In doing this I kept

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Discussion Paper

Interactive
Comment

in mind that your ideas potentially have large implications for our understanding of the atmospheric circulation, including important aspects of future climate change. I also realise that your ideas are unconventional. falling outside standard current thinking on dynamical meteorology. As Editor I would not want to reject a paper simply because it was unconventional and controversial, particularly if it was also potentially important.

However, I am afraid that after due consideration I have decided to reject this paper for publication. This is because I do not believe it reaches the standards required for publication in its present form and I do not see a straightforward route to changing it to make it publishable. If you wish to pursue publication of the ideas in this paper then my suggestion is that you might want to consider presenting them in a different way, perhaps as I suggest in my more detailed comments below.

Your paper (both in its original and revised form) falls into two parts. One is a detailed critique of the Emanuel hurricane model (or models). On the basis of this critique you conclude that the physics considered by Emanuel is not adequate to explain hurricanes. Given this conclusion the second part argues that a new different physical ingredient is needed, provided by the 'evaporative force' and you present an explanation of this force and estimate the magnitude of its effects.

Emanuel's model is intended, and has been interpreted, as a simplified model which provides some basic quantitative predictions about the behaviour of the real atmosphere and also of more or less realistic numerical models of the atmosphere. As noted by Reviewer 3, whilst Emanuel's model has been influential, there is still a vigorous debate going on about certain aspects of it. For example, the tacit assumption by Emanuel of gradient wind balance in the planetary boundary layer has recently been questioned by Smith et al (2008, Quart. J. Royal Met. Soc., 134:551-561). Other work by Persing and Montgomery (2003) has highlighted the role of transfer of heat to the eyewall, neglected in the Emanuel model, in leading to hurricanes that are significantly more intense than predicted by that model.

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Notwithstanding the fact that there is ongoing debate in the meteorological community about the limitations of Emanuel's model, both Reviewers 1 and 3 find your critique of this model neither substantial nor significant enough to be suitable for publication and I have not found any reason to disagree with them.

This implies that your critique cannot yet be regarded as compelling evidence that a new theory (for hurricanes or for other atmospheric phenomena) is required. Nonetheless I have tried to consider the second part of your paper – sections 4 and 5 – independently as a description of a new physical effect – the evaporative force – that might be important in the atmosphere or alternatively as a novel conceptual model which summarises well-known physics in a clever way and leads to greater insight than was available before.

Neither of reviewers 1 or 3 finds the description in Section 4 satisfactory and, I have to say, neither do I.

The first aspect I find unsatisfactory is the emphasis on aerostatic equilibrium for individual species – your equations (12)-(15). Equation (12) describes the balance that would be achieved in an atmosphere that was at rest with molecular diffusion being the only transport mechanism. At one time it was regarded as a puzzle that different scale heights for different species were not observed in the lower part of the atmosphere – the accepted explanation for this is that bulk mixing dominates over molecular diffusive effects. This is not the same thing as saying that molecular diffusion is not essential in the bulk mixing process – but the bulk fluid motion has a large effect and what arises as a result is very different from your (12). Your (12)-(15) and accompanying text might be part of the description of the evolution of a column of moist air evolving under diffusion alone (and remaining in hydrostatic balance), but it does not seem very relevant to the lower part (i.e. troposphere, stratosphere, mesosphere) of the real atmosphere.

This emphasis on aerostatic equilibrium could be a problem of presentation rather than substance – you refer more generally (e.g. in section 5) to the drop in pressure asso-

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ciated with condensation – without that seeming to require that the it happens while individual components of the atmosphere are maintained in aerostatic equilibrium (12). But then a second shortcoming is that no careful justification of this drop in pressure is given. The implications of moisture and change of phase on thermodynamics have been considered carefully by physicists (including meteorologists) for two centuries or more and you would need to show either that the standard approaches (e.g. set out in textbooks such as 'Thermodynamics of Atmospheres and Oceans' by Curry and Webster) imply a drop in pressure associated with condensation, or else where those standard approaches – including perhaps approximations that are usually made – are wrong.

In some of your own online comments on the paper, posted during the online discussion, you pay some attention to thermodynamic details. Indeed in your comment made on 29 September 2008 (S7609-S7613) you give thermodynamic arguments leading to an expression for the moist-adiabatic lapse rate that is different from that given in standard meteorological textbooks. This was potentially interesting and might have given clues to how an evaporative force might have appeared from a careful (and non-standard) treatment of thermodynamics. However in a later comment on 18 October 2008 you make a correction and say that the formula you have derived for moist-adiabatic lapse rate is identical to that given in meteorological textbooks. You go on to say that this indicates that latent work – 'latent work' being part of your new formulation – 'has been implicitly included into the empirically determined value of L , so that the magnitude of latent work was not explicitly estimated and its physical meaning is not discussed'. This sounds as if you are saying something like 'conventional approaches have got this correct by accident', but from my point of view the onus is on you to show that conventionally approaches are limited in some very concrete way by the fact that they have not considered 'latent work' – otherwise there would be no good reason for the meteorological community to take notice of your new approach.

My own view is that if you wish to publish a paper on your ideas in a journal such

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as Atmospheric Chemistry and Physics then you need to put much more emphasis on careful (and very basic) physical discussion – perhaps presenting one or two clear, simple (and physically relevant) thought experiments in which the novelty and correctness of your approach is clear. Some of the material you have include in online discussion comments might be relevant. (The fact that this material exists in online comments does not directly help the cause of the paper I am rejecting – it would have to be put together in a proper paper and itself be subject to the scrutiny of reviewers.) This, in my view, would be much more effective than the indirect approach you have taken, of claiming to find serious flaws in a theory of hurricanes and then appealing to a 'need' for a new physical formulation.

I hope the above comments are helpful if you decide to continue this line of work.

Referee 1 Comments

The revised version of the manuscript shows some improvements, mostly due to the removal of several of the incorrect statements presented in the initial submission. Nevertheless, the discussion is still fairly poor. The criticisms of the heat engine theory for hurricane fail on multiple levels, and the authors do not discuss their alternative view in sufficient detail to make it convincing. This paper should not be published.

1. Criticism of the MPI theory for hurricanes:

The authors have raised several issues regarding the MPI theory for hurricanes. These issues range from marginal side issue to incorrect statements by the authors.

Section 3.1. Bernouilli's equation

This is a fairly minor issue. The authors analysis is technically correct, however their assessment that "contrary to the main result of Emanuel (1991), it is impossible to calculate pressure p_c in the hurricane center by presetting only four parameters" is a gross misrepresentation of the arguments of Emanuel (1991). Indeed, Emanuel (1991,

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p. 185) actually states: "Given r , p , q_a , T_s , and T_o , a lower bound on P_c is obtained by using (8) [with (9)] in (16)". The keyword here is lower bound, for which the Emanuel (1991) derivation is indeed correct.

Section 3.2 dissipative heat engine

The authors claim that dissipative heat engines violate the second law of thermodynamics. However, the modern version of the second law of thermodynamics - namely Clausius formulation - determines whether a process is physically possible depending on whether it is associated with a positive 'irreversible' entropy production. Pauluis and Held (2002) explicitly analyze the entropy production in the atmosphere described as a dissipative heat engine. This analysis firmly establishes that the dissipative heat engine framework do conform to Clausius' formulation of the second law. Despite their mentioning the work of Pauluis and Held, the authors never acknowledge this.

The authors never address Clausius' formulation of the second law in their paper. Rather, their criticism is rather loosely based on the claim that "the dissipative heat engine is equivalent to a perpetual motion machine of the second kind". This is simply incorrect, as such a perpetual motion machine only interacts with a single heat source/sink, while the dissipative heat engine explicitly requires an energy source at a warmer temperature than the energy sink.

Section 3.3 heat loss to space

The authors repeat their incorrect argument that the 'heat sink' in a hurricane would have to be warmer than the heat source. This point has already been challenged in the online discussion: only the integrated cooling along the trajectory matters for the Carnot cycle, not the cooling rate. It is rather disappointing to find that the authors simply keep repeating the same poor argument.

Section 4. Alternative mechanism.

The 'alternative' proposed here falls short from being a consistent theory. Among its

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many failings, I would mention three of them. First, the authors focus on the partial pressure of water vapor alone. However, none of the atmospheric constituent (oxygen, nitrogen, etc) is in aerostatic equilibrium. Furthermore, for an atmosphere in hydrostatic balance, the upward force associate with the vertical variation of the partial pressure of water vapor would have to be exactly balanced by the partial pressure of the other atmospheric gases. Second, at the fundamental level, the 'mechanism' proposed by the authors is still a heat engine that transport latent heat from the surface to the regions were condensation takes place. All their criticisms of the heat engine theory in section 3 apply directly to their own approach. Finally, on a quantitative level, the strength of the 'osmotic' force is at most given by the partial pressure of water vapor. At 30C, this is about 40mb. However, central pressures of less than 900mb have been observed in intense hurricanes. This means that the 'osmotic' theory as proposed by the authors underestimate the intensity of hurricanes by a factor 2.5.

Referee 2 Report

This paper is a ground-breaking contribution to atmospheric science. In its original form, submitted to ACPD, it contained a strong critique to established models of hurricanes, but now, in its revised version it goes beyond and describes a novel driver of atmospheric circulation, the one based on phase transitions of water vapor. I have already dwelt on the novelty of the proposed physics in my two referee comments submitted to the ACPD public discussion. Here I additionally highlight a few important issues as reflected in the revised manuscript.

1. The authors identify partial pressure of water vapor as a source of potential energy for convection. Traditionally, convective available potential energy has been calculated on the physical basis of buoyancy changes caused by temperature differences. Change of air buoyancy due to temperature effects and phase transitions of atmospheric moisture are two absolutely different physical processes. While the former has

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received much attention in meteorology, the latter (from the viewpoint offered by the present authors) remained practically ignored. The store of potential energy associated with atmospheric water vapor, a few thousand Joules per cubic meter, is right of the magnitude announced to be necessary for the maintenance of stationary atmospheric circulation and beyond, see, e.g., Rennó and Ingersoll (1996, p. 573).

2. Obviously in the long term average condensation is compensated by evaporation. Pinpointing intense condensation of water vapor as the physical cause of hurricanes and tornadoes the authors shed light on the problem of how to determine the frequency of occurrence of these phenomena. Condensation within the hurricane is hundreds of times more intense than the long-term precipitation average. So the frequency of hurricanes in a given area can be calculated from the condition that the time period between two consecutive hurricane events must not be less than the time when the amount of atmospheric moisture condensed within the hurricane is restored by evaporation.

3. Of paramount importance is the question that was gathered by the authors from one of the Short Comments (Nobre, 2008) of why there are regions (like forested areas) where hurricanes do not occur. In the revised manuscript the authors provide an answer (Section 4.3). They point out that hurricane wind speeds will not develop if the power available from condensation is spent on turbulent surface friction. This implies formation of small turbulent eddies rather than acceleration of air masses in a given direction. Surface friction proportional to the weight of atmospheric column is introduced (Eq. 18, first term) along with the conventional term proportional to squared velocity. This friction depends on a linear scale zT that characterizes surface roughness. Physically, this term has the meaning of friction of rest - this what the authors are explicit about in their latest contribution; I believe this should be mentioned in the revised manuscript as well. It is remarkable that this theoretical derivation coincides in form with the well-known empirical Charnock's relation.

The main idea is that work of surface friction force in a given circulation event grows proportionally to circulation length labeled LE (Eq. 19), while work of condensational

force is not dependent on LE (Eq. 16). So if there is a sufficiently large region (acceptor region, in the authors' terminology), where condensation is significantly different from that in the neighboring regions, then a large-scale stationary circulation forms largely guaranteed against weather extremes. Power of condensation will be spent on counteracting surface friction with no potential for hurricane wind speeds to develop. These ideas and the quantitative theoretical framework they are presented in have in my view an enormous potential for clarifying the consequences of landscape fragmentation for the regional atmospheric circulation. It follows that large scale forest fragmentation in Amazonia, for example, is burdened with the danger of breaking the circulation stability.

4. A single physical cause (intense condensation of water vapor) has been identified for hurricanes and tornadoes – something one in meteorology could hardly dream of before. Reverse explosions in slow motion - original, picturesque and thought-provoking metaphor.

5. All these blatantly new physical ideas come wrapped into an extensive and detailed critique of the existing hurricane theory. Let the authors forgive me if I am wrong, but from the reactions of other referees and community members I have a strong feeling that this part of the paper came as a forced political choice. That is, I have difficulties in imagining that without such a blow to the standing paradigm the authors could have hoped to draw at least a little bit of attention to their new ideas. Had they just submitted the new, radically different theory on fundamental physics of hurricanes (and atmospheric circulation), most probably they would have been referred to the well-known and widely cited works that "explain everything" in hurricanes.

Focusing on the critical points that were retained in the revised manuscript, these are (1) heat release to space as shown incompatible with the hurricane as Carnot cycle model (Section 3.3); (2) math (incorrect integration of Bernoulli's equation and incorrect formula for Carnot cycle) (Section 3.1); (3) the authors' critique of the concept of the dissipative heat engine (Section 3.2). Personally I am convinced by the authors' arguments that in all three points they present much food for the relevant part of the

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scientific community to analyze, digest and re-think. However, even a single of these points would already justify an editorial position very much caring about publication of this study.

I would also like to mention that personally I was very surprised not having found anywhere in the works of Emanuel and colleagues any attempts to estimate radiative heat flux to space from the hurricane area and to compare it with the presumed Carnot cycle fluxes, as the authors do in Section 3.3. I fully agree with the authors who wrote in the discussion in response to Referee 1 that this problem is not to be hand waved. I would have expected to find a very detailed quantitative treatment of it by the authors of the modern hurricane models, as it provides a crucial test for the validity of the very physical idea (Carnot cycle).

6. In my view, the revised manuscript is overall much more user-friendly compared to the original version. That is, preserving all the essential scientific content, the authors did their best to answer the concerns of the meteorological community available to them via discussion comments.

Renno N.O., and A.P. Ingersoll, 1996: Natural Convection as a Heat Engine: A Theory for CAPE. *J. Atmos. Sci.*, 53, 572-585.

Nobre P., 2008, Interactive comment on "On the validity of representing hurricanes as Carnot heat engine" by A. M. Makarieva et al. *Atmos. Chem. Phys. Discuss.*, 8, S8669-S8670, www.atmos-chem-phys-discuss.net/8/S8669/2008

Referee 3 Comments

The revisions have not changed my view that this paper should be rejected.

The critiques of the Emanuel theory are now spelled out more clearly but most of them fall into two categories.

Some are true but are already well known (and were, I believe, to Emanuel at the time he did the work). These represent simplifying assumptions made for the sake of

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Interactive Discussion

Discussion Paper



analytical progress. For these criticisms to have any import one would need to show not just that the assumptions do not hold precisely in the atmosphere, but that assuming them leads to substantially incorrect results; one way of doing this is by comparison with a more complete model in which the assumptions are not made (e.g., see the much more substantive and significant recent critique of Emanuel's theory by Roger Smith et al. in QJRMS). The radiative cooling being spread out over a large radius compared to the latent heating and the mixing of water vapor into the descending air fall in this category.

The other category of criticisms are those which are just wrong. In particular, the accusation that the dissipative heat engine is more efficient than a Carnot cycle is incorrect. The "efficiency" in question is not a thermodynamic efficiency, since no work is done on the environment. In the original (pre-dissipative) Emanuel theory, work was done on the ocean, but that is not true in the dissipative case.

I do not think much of the authors' own "theory" now presented in section 4. It is just not spelled out specifically enough or in enough detail to result in testable predictions. It would not be publishable in its own right and is not made so by being married to a poor critique of Emanuel's work.

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

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