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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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Dear ACP Executive Committee:

We have been informed by the Handling Editor Dr. Peter Haynes (the Editor hereafter) that our revised manuscript "On the validity of representing hurricanes as Carnot heat engine" has not been accepted for publication in ACP based on three sets of the referees' comments and the Editor's evaluation. Here we would like to appeal to the ACP executive committee for revision of the editorial decision as described in Chapter 7, Option A of the ACP FAQ (http://www.atmospheric-chemistry-and-physics.net/general_information/faq.html#chapter7).

We would like to emphasize that we are grateful to the ACP(D) journal for considering our work and for allowing us to express our views on its pages. We have no doubts that all the involved parties, including the Editor and the referees, have acted to the best of

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their capabilities and in the interest of science. However, we are firmly convinced that the final decision to reject our paper has emerged as erroneous for a variety of reasons, and is in need of a careful re-examination in the view of the arguments below. We are thus using those legal opportunities that the ACP is kindly providing to approach the ACP executive committee with this appeal.

Our paper consists of two parts. The first one contains a critique of the hurricane model of Kerry Emanuel and the second part proposes a novel approach to hurricane's physics which takes into account the release of potential energy during condensation of water vapor. Anonymous Referees No. 1 and No. 3 recommended our paper to be rejected, Anonymous Referee No. 2 recommended to accept it. Below we are listing five major issues that we would kindly ask the ACP executive committee to consider.

1. We would like to kindly ask the ACP executive committee to re-consider the decision regarding our paper **strictly and accountably ignoring the position of Referee No. 3** concerning the second part of our paper (the new approach to hurricane physics). The Referee devoted only two phrases to this in his/her final comments, which were:

"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."

These statements do not contain any specific information about our results and can be applied to any proposition some person does not like and/or wants to humiliate. Such statements do not conform to the review quality standards adopted by the ACP and to the ACP General Obligations for Referees (http://www.atmospheric-chemistry-and-physics.net/review/obligations_for_referees.html) which say: "Referees should explain and support their judgments adequately so that editors and authors may understand the basis of their comments."

2. We would like to kindly ask the ACP executive committee to consider that during the Interactive Discussion Referee No. 1 **did not make any specific comment regarding**

the content of the second part of the paper (the novel approach). In his/her final comments, the Referee put forward three objections all of which equally pertain to the original ACPD paper as well as to the revised manuscript. Therefore, the authors were not given an opportunity to respond to the critique of the referee during the Interactive Discussion. By consequence, the Editor was not able to evaluate our response when making the decision.

In the view of item 1 above, the second part of our paper has not been discussed at all during the Interactive Discussion by the two referees who negatively evaluated our work.

3. We would like to kindly ask the ACP executive committee to consider that **the physical core** of the novel approach that we propose, namely the release of potential energy upon condensation of water vapor, **has not been critically considered or mentioned at all** in the comments of the two negative referees and the Editor. This is despite the fact that potential energy release during condensation was discussed in detail by Dr. A. Nefiodov and Dr. P. Nobre (discussion participants) and explicitly mentioned by Referee No. 2 in his/her final comments: *"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)."*

Referee No. 1 in his/her final comments and Referee No. 3 in his/her discussion comment made two statements showing that, instead, they continue to view the hurricanes as a **heat engine** with no attempt of analyzing or simply mentioning the novel approach based on potential energy release:

Referee No. 1: *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.*

Referee No. 3: *Finally, it is pretty hard to imagine what the hurricane is if it isn't a heat engine of some type (whether or not it holds perfectly to the Carnot model). What other plausible source of energy is there*

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Without attending to the physical core of the proposed approach it is not possible to form a scientifically substantiated judgment about it.

4. We would like to kindly ask the ACP executive committee to consider that the main shortcoming the Editor finds in our results is that *"no careful justification of this drop in pressure [associated with condensation] is given."* However, to be the reason for rejection, we would expect such a statement to be more specific, e.g. outlining what precisely in the presented justification was not careful. Otherwise it is not at all possible to attend to such a critique.

The Editor seems to suggest, if we got this issue right, that we should prove 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,"* as if the standard approaches were against it. The classical Clausius-Clapeyron law says that when temperature decreases (and this is what happens in the ascending parcel) partial pressure of saturated water vapor drops. As the total weight of the air column is the function of the total number of gas molecules in the column, this reduces the weight of air column and disturbs the hydrostatic equilibrium if it originally existed. These basic facts do not seem to need any specific justification as they are firmly established.

On the same note, we would like to kindly ask the ACP executive committee to consider that while Referees No. 1 and No. 3 argue that there are not enough "details" in our presentation of the novel concept to make "predictions", the Editor, on the contrary, advises to put "much more emphasis on careful (and very basic) physical discussion" and thought experiments, and in the meantime Referee No. 2 praises the paper's clarity as also do three of the other four discussion participants. All this seems to reveal more a matter of personal inclination, interest and time investment regarding a novel proposition rather than an objective reflection of the paper's clarity and transparency.

In other words, four physically educated scientists (Referee No. 2, Dr. P. Nobre, Dr. A.

Nefiodov, Dr. S. Sherman) appear to understand the new approach that we propose from remarkably diverse angles and backgrounds. In such a situation it does not appear justified to reject the paper because it is "unclear" – no paper, especially carrying conceptually novel results, can be made immediately clear to everyone. Such a paper is rejectable if it is proved wrong and/or demanding specific clarifications, not on the basis of a general recommendation to improve clarity.

5. Regarding the critical (first) part of our paper, we would like to kindly ask the ACP executive committee to consider that the main statement we put forward is that the influential hurricane model of Kerry Emanuel that involves the so-called "dissipative heat engine" violates the second law of thermodynamics.

Referee No. 1 says that *"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."

Referee No. 3 says that *"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."*

These comments neglect that in the dissipative heat engine one considers isothermal dissipation of mechanical work at the highest temperature T_s of the engine (see, e.g.,

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Eq. 3 on p. S7916 of the interactive comment of Referee No. 1). Isothermal dissipation implies that heat is removed from the system where the dissipation takes place. Heat transfer can only proceed from a hotter to a colder medium with $T_1 < T_s$. However, in the dissipative heat engine this heat is further returned back at $T = T_s$ (added to the cycle). This implies heat transfer from the colder to the hotter medium (from T_1 to T_s). The impossibility of this process, overlooked by Emanuel as well as by Pauluis and Held (2002) and others who considered the dissipative heat engine, represents the classical formulation of the second law of thermodynamics as put forward by Clausius.

That this process is combined in the dissipative heat engine with "conventional" processes that do not violate the laws of thermodynamics and involve different temperatures does not change the fact that the dissipative heat engine is equivalent to perpetual motion machine of the second kind. The mere presence of different temperatures in the engine does not ensure its conformity to the laws of nature. Perpetual motion machines are usually presented as very complex and unobvious constructions, which sometimes demand much concentration to pinpoint where exactly the violation of thermodynamics laws takes place.

None of the papers discussing the dissipative heat engine was published in ACP. By now issuing an explicit statement that our critique is wrong (the Editor agrees with the comments of Referees 1 and 3 on this subject without issuing a judgment of his own) **the ACP publicly approves a concept equivalent to the perpetual motion machine of the second kind already after it has been exposed.** This is much worse a scientific standing than that of the journals that published those works having simply overlooked the flaw. In the view of the seriousness of these claims, we kindly ask the ACP executive committee to pay utmost attention to the evaluation of this point of our critique.

To facilitate re-evaluation of our manuscript, below we provide responses to the specific points made by the Editor and the referees in their final comments.

Referee No. 1

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 pc in the hurricane center by presetting only four parameters" is a gross misrepresentation of the arguments of Emanuel (1991). Indeed, Emanuel (1991, p. 185) actually states: "Given r, p, qa, Ts, and To, a lower bound on Pc is obtained by using (8) [with (9)] in (16)". The keyword here is lower bound, for which the Emanuel (1991) derivation is indeed correct.

Carnot cycle features maximum possible efficiency and produces maximum work that is associated with minimum possible p_c. "Lower bound on P_c" in the text of Emanuel simply means that it is the value of p_c obtained from the consideration of Carnot cycle. Our words regarding the "only four parameters" pertain, obviously, to Carnot cycle as well and, hence, to the value of p_c derived from it, i.e. precisely to the "lower bound on P_c" of Emanuel. Since the Referee does not disclose in any way how the "keyword" changes our conclusions and why the derivation of Emanuel becomes correct, the statement about the "keyword" looks like an attempt to escape the criticism behind phrases not immediately understandable to anybody not deeply acquainted with the problem, see also item 1 above regarding the requirements to the Referees' comments.

Section 3.2 dissipative heat engine

The comment is cited in full under item 5 above, where we also respond. Here we add the following. The Referee says that Pauluis and Held (2002) analysis *"firmly establishes that the dissipative heat engine framework do conform to Clausius' formulation of the second law"* and that we never acknowledge that. This is because Pauluis and Held (2002) make the same error as Emanuel did, considering isothermal dissipation of mechanical work at the warmer isotherm of the Carnot cycle. We explain that in none of the papers dealing with the dissipative heat engine the Carnot cycle was considered. This prevented the relevant authors from observing the violation of the second law of thermodynamics as formulated by Kelvin – indeed, see the revised manuscript p. S11268-S11269, the work that dissipates to heat at the warmer isotherm is further regenerated in full at the same isotherm. We would like to explicitly mention that these arguments of ours have never been considered by the referee, despite being available as early

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as in our first comment made in the Interactive Discussion.

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.

Here the referee seems to step back from his/her own words (ACPD S7917) which were:

In section 3.4, the authors argue that the atmosphere cannot be cooled sufficiently fast to support a hurricane as described by the Emanuel framework. Their argument is based on the fact that the latent heat flux in the eyewall of a hurricanes is up to 20 times larger than the radiation emitted atmospheric temperature. This argument is based on the assumption that the area where heating and cooling occurs are the same. However, in the case of a hurricane, the regions of high surface energy flux is concentrated near the eyewall (20-50km), which is much smaller than the overall extent of the upper level circulation (500-1000 km). The surface energy flux can be fully compensated by radiative cooling as long as the outer radius of the storm is 5 times larger than the radius of the eyewall.

In the revised version we used the recently available data of Trenberth and Fasullo (2007) to specifically attend to the above comment of the referee. We considered precisely the region of about 1000 km, i.e. the entire area from which the Referee presumes the hurricane cools. Air cycles in the hurricane along the four legs over about 1000 km. In order for the Carnot cycle formalism to be legitimate, all heat must be released within this area during the cycle period t. If it is not, this is not a Carnot cycle and one cannot use the Carnot cycle formalism. We show that the rate of heat release over this entire area (and not in the region of maximum flux) is at least twenty times larger than the flux heat radiated to space. Obviously, for any period of time the same inequality persists for any integrated amount of released/radiated heat. Hence, the Carnot cycle formalism cannot be used for hurricane description.

Here it is also pertinent to quote Referee No. 2: *"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*

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authors of the modern hurricane models, as it provides a crucial test for the validity of the very physical idea (Carnot cycle)."

Referee No. 1 continues:

Section 4. Alternative mechanism.

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

We consider in our paper the release of potential energy associated with drop of moist air pressure that occurs due to condensation. Condensation does not affect other air components except for water vapor. Therefore, consideration of other air components is not necessary for the numerical estimate of the power of the evaporative force associated with condensation.

This point seems to be appreciated by the Editor, who wrote: *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 associated with condensation – without that seeming to require that the it happens while individual components of the atmosphere are maintained in aerostatic equilibrium (12).*

Referee No. 1 continues: *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.*

There is no physical law that would order the atmosphere to be in exact hydrostatic balance when one component (water vapor) is always out of balance. Hydrostatic equilibrium is a numerical approximation of pressure distribution in a dynamic atmosphere (e.g., the hurricanes). Hurricane is obviously not in hydrostatic equilibrium, otherwise air would not accelerate in the upward direction. In Appendix 2 we consider in detailed way the magnitude of deviation from the hydrostatic equilibrium produced by the evaporative force.

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.

This is the main misconception about our approach, see also item 3 above. In heat engines the mechanical work produced depends on heat release, so the processes of heat input and dis-

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posal are critical for the calculation of this work. In the novel approach we propose mechanical work appears not from heat, but directly from the release of potential energy during condensation, see Eq. (5) on p. S11278 of the revised manuscript. As is well-known, this process occurs with an efficiency equal to unity, like the potential energy of a ball on a hill is fully converted to its kinetic energy (or the potential energy of a compressed spring is converted to its kinetic energy). The magnitude of potential energy released as an air volume ascends in the atmosphere is practically independent of the value of latent heat, p. S11278. The scale of produced velocities u is given by water vapor partial pressure, $p_v \sim \rho u^2/2$. Therefore, in this conceptually different approach where and how latent heat released (and whether condensation is accompanied by heat release at all) is not important for the calculation of velocities. Hurricane is not a heat engine, it is a dynamic engine similar in its nature to explosion, avalanche, etc. i.e. phenomena working on the basis of the release of previously accumulated potential energy.

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.

As is well-known, air pressure in the hurricane monotonously diminishes from the outskirts towards the center. In the meantime, air velocity shows a distinctly different pattern: it grows towards the eyewall and then rapidly diminishes from the eyewall to the eye center. Pressure drops across these two parts of horizontal trajectory are approximately of the same magnitude, around 40 mb at maximum, see, e.g., Fig. 2 of Holland (1980, Mon Wea Rev 108: 1212).

Therefore, hurricane velocities formed via potential energy release develop over only approximately one half of total hurricane air pressure drop. This is accurately quantified by our approach. The physical nature of the additional drop of pressure in the eye (the one not related to hurricane-like velocities) is pretty clear – it is formed by the centrifugal forces that make the rotating air tend away from the center towards the eyewall. This process is counteracted by the pressure imposed by momentum of the air masses spiraling in towards the eyewall, which is of the order of ρu^2, i.e. of the order of p_v (partial pressure of water vapor). Therefore, the same pressure difference p_v (the osmotic force) that accelerates air to hurricane velocities also balances the centrifugal forces that are responsible for the additional pressure drop of approximately the same magnitude in the hurricane eye. It would be helpful to have received these

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basic comments and been able to respond to them during the Interactive Discussion.

Referee No. 2 provided a number of valuable observations which we do not list here in full (some are cited above) as they do not contain critical comments or suggestions, but the following one does:

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.

Today it is nearly one year since our paper was submitted to ACPD and nearly five months as the revised version has been considered for ACP. Since we have been working intensely on further developing the concept through all this time, we can now refer the interested reader to a more complete treatment of turbulent friction submitted to the Interactive Discussion on 22 March, 2009 (ACPD, p. S11826).

Referee No. 3

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 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).

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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.

See response to Referee No. 1 regarding Section 3.3

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.

See response under item 5 above.

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.

See item 1 above.

The comments of Referee No. 3 end with the following: *This paper should have been rejected the first time around. The fact that it wasn't seems to have resulted in some education of the authors by well-meaning and capable volunteers who have commented on the paper on the discussion site. This is all well and good, but is in my view not the way to run a peer-reviewed journal with high standards. If it becomes known that poor papers can survive for multiple rounds of reviews while the authors go endlessly back and forth getting others to do the work of improving their papers, ACP will become a magnet for papers of this kind and will acquire (and deserve) a poor reputation. Perhaps this is an isolated incident and I should not extrapolate like this; but as this is my first review for ACP and I have never had a similar experience with another journal, it does strike me as something the discussion process may inadvertently encourage and against which the editors may want to be on their guards.*

To this statement of The Anonymous Referee we can respond by quoting a phrase attributed to one of the Editors of a EGU journal: **"I doubt that anybody gets hurt just because a paper is accepted that in the end turns out to be wrong. The opposite is much worse - an idea is right after all but is rejected according to current thinking. That is bad for science."**

Specific Comments of the Editor

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 nu-

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merical 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.

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.

See item 5 above. We add that the critique of Emanuel's works by others does not cancel the plausibility and originality of our own critique, which deals with quite different issues than those mentioned by the Editor.

Neither of reviewers 1 or 3 finds the description in Section 4 satisfactory and, I have to say, neither do I.
See item 1 above regarding taking into account the position of Referee 3.

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 associated with condensation – without that seeming to require that the it happens while individual components of the atmosphere are maintained in aerostatic equilibrium (12).

See above our response to Referee No. 1 made under Section 4. We add that the evaporative force is related to condensation, which only concerns water vapor and not other air gases. Drop of air pressure in the gravitational field is apparently not related with the release of potential

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energy if the pressure distribution is static. Therefore, the amount of potential energy released during condensation is estimated not from the bulk pressure drop water vapor experiences as the air ascends, but from the difference between the temperature-driven and aerostatic water vapor partial pressure distributions. This is not a problem of presentation, but an essential calculation of the degree of non-equilibrium water vapor finds itself in.

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.

See item 4 above.

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-adibatic 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-adibatic 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.

We very much appreciate the Editor's attention to the fundamental notion of latent work, which is included into the revised manuscript. Indeed, we point out that vaporization involves two processes: the one is two overcome the intermolecular forces and "tear" the water molecule away from the liquid to gaseous phase (L_v) and the second one – to perform some work to "squeeze" the newly appearing gaseous molecules into the volume already occupied by air

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molecules ("latent work" $\sim RT$), $L = L_v + RT$. To our knowledge, this dichotomy has never been considered before. Namely this latent work forms the store of potential energy that is released during condensation in the open atmosphere.

The release of latent heat and latent work are characterized by different time scales that depend in different ways on the size of the volume where they are taking place. In small volumes the dynamic air motions generated by latent work release rapidly dissipate to heat that is added to latent heat. In the result, measurements of vaporization constants L made in small laboratory volumes should include the value of latent work, in accordance with our above statement cited by the Editor. This does not undermine the significance of latent work for atmospheric processes, where air motions take long time to dissipate. This poses the question on whether one should include latent work into vaporization constant when describing condensation in the atmosphere and how that would impact the moist adiabatic gradient. All these are exciting issues, and we are continuing to study them, but they are neither directly relevant nor necessary for the justification of the process of potential energy release during condensation. Therefore, the conventional approaches are indeed limited in that they have overlooked the latent work as the source of condensation-related dynamic energy, simply because they have never considered any work performed in relation to evaporation.

We would like to add that the Interactive Discussion has developed in an apparently dramatic manner with several opinions clashing. In such a situation, an attentive recommendation from the Editor upon the discussion closure would have been of great value to the authors as to which among the many discussed issues should have been developed or reflected in a more detailed way in the revised manuscript. In the absence of such a recommendation or feedback we included into the revised version only those issues that we considered absolutely indispensable for presenting our views. Consideration of the precise value of the moist adiabatic gradient was not judged by us to be one of them.

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

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