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

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Appendices to Final Editor Comment

Appendix 1: Referee Call

Dear Colleague,

I would be very grateful, if you could act as a referee for the manuscript acp-2008-250 by Makarieva et al. submitted for publication in Atmospheric Chemistry and Physics (ACP).

The manuscript deals with fundamental aspects of the physics and description of Hurricanes and atmospheric circulation, and it is accessible through the online editorial system of ACP as detailed in the technical instructions appended below.

An earlier version of the manuscript has been published and publicly reviewed in the interactive

discussion forum of the journal, Atmospheric Chemistry and Physics Discussions (ACPD):

http://www.atmos-chem-phys-discuss.net/8/17423/2008/acpd-8-17423-2008-discussion.html

As explained in the final comments of the public discussion, the handling editor has decided no to accept the revised version for final publication in ACP, and the authors have filed an appeal against this decision:

http://www.atmos-chem-phys-discuss.net/8/S12168/2009/

http://www.atmos-chem-phys-discuss.net/8/S12153/2009/

Accordingly, the ACP executive committee will carefully re-assess the editorial decision, and I would very much appreciate your advice in this process.

In particular, I would be very grateful if you could address the following questions and explicitly comment on them in your referee report:

1) Are the traditional Hurricane models described in the papers of Emanuel et al. as cited by Makarieva et al. fully consistent with the laws of thermodynamics?

2) Are the arguments of Makarieva et al. correct when they show that the traditional Hurricane models of Emanuel et al. violate the laws of thermodynamics? If not, why?

3) Is the concept of describing a hurricane as a Carnot cycle (dissipative heat engine) equivalent to a perpetuum mobile (perpetual motion machine of the second kind)? If not, why?

4) Is the critique of Makarieva et al. relevant for the applicability of the Hurricane models of Emanuel et al.? If not, why?

5) Is it plausible that the driving forces and properties of Hurricanes and other forms of atmospheric circulation may better described by the concept of Makarieva et al. (evaporative-condensational pressure gradient force due to local pressure drop upon condensation of water vapour in the atmosphere) rather than by conventional meteorological formalisms? If not, why?

6) Would you recommend publication of the revised manuscript of Makarieva et al. under the original title or a title like: "Are Hurricanes better described by a Carnot cycle or by an evaporative-condensational pressure gradient force?" If not, what else would you recommend?

When considering points 5) and 6), please take into account not only the manuscript of

Makarieva et al. and its discussion in ACPD but also the discussion of related recent papers in the journal Hydrology and Earth System Sciences (HESS):

Meesters et al.: Comment on “Biotic pump . . . ” by Makarieva and Gorshkov

http://www.hydrol-earth-syst-sci-discuss.net/6/401/2009/hessd-6-401-2009-discussion.html

http://www.hydrol-earth-syst-sci-discuss.net/6/S280/2009/

http://www.hydrol-earth-syst-sci-discuss.net/6/S302/2009/

Please note that your referee report will not be automatically published in ACPD. Depending on your agreement and on the further evolution of the review process, however, it may be desirable and useful to make your comments publicly available – for the benefit of scientific discourse and progress (interactive open access journal concept of ACP).

Depending on your preferences and the further editorial handling and decisions, publication of your comments could proceed in different ways (to be flexibly arranged as we proceed):

(a) publication of you referee report as a formal Referee Comment in ACPD (anonymous or under your name, as you prefer);

(b) inclusion of (parts of) your referee report in a formal Editor Comment, which I intend to publish in ACPD upon completion of the manuscript review and re-assessment of the earlier editorial decision.

Please let me know if you have any questions or concerns with regard to this or any other issues, and inform me at your earliest convenience if you accept the invitation to act as a referee.

Looking into all of the above questions, manuscripts and discussions will probably require substantial efforts, but I hope that you will find the topic interesting and I can assure you that your contributions will be highly appreciated by all involved parties.

Many thanks and best regards,

Ulrich Pöschl

Appendix 2: Comment of Referee A1

This is rather a quick response to your review request, so please dismiss all my markings about. In my opinion, this manuscript should be returned to a beginning of a full cycle of ACPD review process.

The paper has been extensively revised during the first round of the editorial process, and it is best considered as a new submission with all the forthcoming reviews pulbished as a standard ACPD manuscript. It should be treated in this manner especially regarding the fact that the editor has decided to reject this paper with two negative reviews.

Currently I am on travel, and I cannot promise to provide you comments more in depth before the due date. I have carefully read the original manuscript, the editorial decision letter with final reviewers, and the authors’ response to the decision. I also went through all the exchanges at ACPD very rapidly.

After examining all the materials available in this manner, I conclude, my best recommendation is as given above without getting into any scientific issues at this very moment. Of course, if you decide to renew the editorial process of this article as a new ACPD submission, I am more than happy to serve as one of the reviewers.

Appendix 3: Comment of Referee A2

Makarieva et al. have significantly changed and extended their original ACPD manuscript, in part based on earlier referee comments. I would, however, not recommend the revised manuscript for publication in ACP. My specific concerns are:

Sect. 3.1:
Isn't the expression for A in Eq. 10 equal to the contribution from the a–c isotherm in Eqs. 7 and 8? I can see that the treatment of the o–o′ isotherm in Eq. 16 of Emanuel (1991) is very different from that in Eq. 8. of the revised manuscript. The contribution from the a–c isotherm, which is discussed in Sect. 3.1, however, seems identical. Therefore, I find the discussion in Sect. 3.1 very misleading.

Sect. 3.2:
In his discussion of the second law of thermodynamics Bazarov (1964) states that "heat cannot be converted into work completely without compensation". As far as I understand it, in the case of hurricanes this compensation occurs at least in part in the form of exchange with the environment. Emanuel explicitly and repeatedly states in his papers that the energy cycle in a hurricane is in fact open, since air flowing out near the top of the storm usually experiences strong exchanges with the environment. He also repeatedly states that considering hurricanes as a closed cycle (identical state in the beginning and the end of a full cycle) is an idealization. While I would certainly have welcomed a critical re-examination of the implications of this idealization, I do not agree with the criticism raised in Sect. 3.2. As far as I can see, the models by Emanuel et al. are consistent with the laws of thermodynamics. In my opinion, in discussing the "efficiency" of the "dissipative heat engine", Makarieva et al. did not sufficiently take into account that this "engine" does not perform work on the environment.

Sect. 3.3:
A large part of the discussion in this section (as well as the third paragraph of the introduction) describe what would follow if one required the areas where heating and cooling take place to be equal. To my knowledge, this has not been suggested in the relevant literature and I do not understand the motivation for this discussion. I assume the intention of this section has not been to criticize the assumption of local radiative convective equilibrium in some numerical models with periodic boundary conditions?

Sect. 4:
I think that Bernoulli's equation is not applicable to the partial pressure of water vapor. Furthermore, I don't think the mechanism proposed in this section can explain observations of almost undiluted ascent of air in moist convection all the way to the tropopause, while such ascent is readily explained by conventional theory. If a gas with a smaller density (water vapor) relative to some mixture (moist air) is removed from that mixture, the remainder becomes more dense, and it is not clear to me why this should result in ascent. In my opinion, one good method to assess the potential importance of the suggested mechanism would be a model sensitivity study in which a correct treatment of the proposed mechanism is implemented into a 3-D model which is capable of simulating hurricanes.

Reference: Bazarov, I. P., Thermodynamics, New York: The Macmillan Company.

Appendix 4: Comment of Referee A3, Dr. Hubert Savenije

The paper by Makarieva et al. on the physics of Hurricanes and atmospheric circulation has been heavily debated, and may be seen as one of the most interesting papers that have appeared in recent years. The closely related paper in HESS on the "biotic pump" (Makarieva and Gorshkov, Hydrol. Earth Syst. Sci., 11, 1013–1033, 2007), which first presented the condensation-evaporation driven atmospheric circulation, was equally heavily debated, and has rocked the hydrological "boat" until this moment in time (through a Comment and Reply exchange in HESS). In view of the present difficulty we are experiencing in accurate prediction of climate and weather through meteorological models, these papers are extremely important, if only for raising fundamental questions to which the hydrological and meteorological communities have not yet been able to provide adequate answers.

My arguments address three aspects: the substance of the paper, the process of discussion, and the ethics of publishing.

The Substance

From my perspective as a hydrologist, with merely a general background in physics, the theory is solid, difficult to refute, based on fundamental laws of nature, consistently using theoretical deductions without the introduction of empirical relationships, clearly presented without the use of unnecessary jargon, and using straightforward and even simple mathematics (particularly the explanations in ACPD, 8, S8904–S8915, 2008 are enlightening). The only thing on which one can differ in terms of opinion is the order of magnitude of the forces at play. One can not debate the "evaporative force", one can only debate its relative magnitude compared to others. Hence, for me, there is no doubt that the paper is sound. Moreover, they have demonstrated that the order of magnitudes of the vertical and horizontal wind velocities, obtained by purely physical reasoning, are correct.

Only the future can tell, by experimental data, if the relative magnitude of the evaporative force is relevant under all circumstances.

The Process

The exchange of arguments in the paper and in the contribution to the discussion is of highest quality. At all times, the argumentation is solid, based on theoretical arguments, well-thought

through, original patient and detailed, always going back to basic physical arguments in clear mathematical terms. Even if some of the opponents are sometimes defensive of their established theory (and sometimes even aggressive), the authors always reply in a composed and dignified way. The authors are without any doubt serious and high level scientists. This is, by the way, also proven by their heavy impact and groundbreaking theories on Metabolism in the Journal of Theoretical Biology and in the Proceedings of the Royal Society of London and Proceedings of the National Academy of Sciences (Makarieva et al, 2005a, 2005b, 2006, 2008). In all these cases they refute existing theories by fundamental scientific arguments, based on fundamental laws of physics. The result is a potential breakthrough for an entire discipline, which can result in unexpectedly important advances. A striking example of this can be found in the reconstruction of past climate analyzing a fossil of an ancient animal, published recently in Nature (Head et al, 2009), a reconstruction which was only possible using the corrected fundamental metabolic relations proposed in the above mentioned theoretical work by Makarieva et al. Authors who write such high level papers and who maintain their arguments in a high level open discussion deserve their work to be published, controversial or not.

Although not a specialist in meteorology, I see a pattern in the discussion carried out in ACPD (and in HESSD as well). Referees who argue against the paper, thinking within the existing paradigms and challenging the authors' new theory by referring to established theories with underlying assumptions that they take for granted; and the authors who clearly counter these assumptions by fundamental reasoning against which it is difficult to argue in fundamental terms. It is a problem of "schools". Established scientists both in Hydrology and in Meteorology think within the "schools" that have shaped their way of thinking. It is very hard to think outside these established theories and to accept new ideas that challenge some of the basic assumptions of one's trade. This brings me to the ethics of publishing.

Ethics of publication

There are in history many examples of groundbreaking theories that only became accepted many years after they were formulated. More often than not the scientist was silenced, disreputed, exiled or even tortured and killed. Of course there are also scientists whom everybody believed but who were later proven wrong. We can make two errors in science: publish a false theory, or reject a correct theory. The first error is often made, can sometimes be damaging to a journal, but there is the opportunity for the theory to be proven false. The second error is also made, is made more rarely, but is a more egregious error for the opportunity is denied for it to be eventually proved right: because the paper never comes to light and so nobody knows that a ‘serious’ mistake has been made until much later. In general scientists, as normal human beings, are defensive of their trade.

Innovations or groundbreaking theories are seldom welcomed at the time they are launched. They hurt the feelings of people who were trained within a certain paradigm and they threaten reputations, power and interests. A good journal should not be afraid of hosting such a debate, certainly not open access journals like ACP, where the entire community can see that the debate was openly and sincerely done. The reverse could be more damaging to ACP: a paper which has been debated openly, with such solid (and difficult to refute) argumentation by the authors, which is subsequently rejected on defensive and weak arguments. Science is not a democracy. It is not the majority that decides if a paper should be accepted or not. It is about the substance of the debate and not about whether people like it or not. If the majority should decide then no paper that is out of the ordinary would be accepted.

In short, I think the revised paper needs to be published, as far as I am concerned with the same title. The paper can, after publication in ACP, entertain “comments” on the published paper as any other journal would (we have a Comment and a Reply process ongoing in HESS related to the paper of the same authors at this moment). This is the way science should work. We do not do science a favour by stifling the debate and rejecting this very interesting paper.

References:

Makarieva A.M., Gorshkov V.G., Li B.-L. (2005) Energetics of the smallest: Do bacteria breathe at the same rate as whales? Proceedings of the Royal Society of London, Biological Series, 272, 2219–2224

Makarieva A.M., Gorshkov V.G., Li B.-L. (2005) Gigantism, temperature and metabolic rate in terrestrial poikilotherms. Proceedings of the Royal Society of London, Biological Series, 272, 2325–2328

Makarieva A.M., Gorshkov V.G., Li B.-L. (2006) Distributive network model of Banavar, Damuth, Maritan and Rinaldo (2002): Critique and perspective. Journal of Theoretical Biology, 239, 394–397

Makarieva AM, Gorshkov V.G., Li B.-L., Chown S.L., Reich P.B., and Gavrilov V.M., (2008), Mean mass-specific metabolic rates are strikingly similar across life’s major domains: Evidence for life’s metabolic optimum, PNAS, 105(44), p.16994–16999

Head, J.J., et al, (2009) Giant boid snake from the Palaeocene neotropics reveals hotter past equatorial temperatures, Nature, 457, p 715–718, doi:10.1038/nature07671

Appendix 5: Comment of Referee A4, Dr. Daniel Rosenfeld

The main issue at question here is whether the depressurization of the air upon condensational removal of vapor at constant volume can serve as an alternative explanation for the pressure drop that propels a tropical storm. I will address only this question here on fundamental considerations with the simplest possible formulation that should be very clear. Because the validity of this mechanism is the central question, the criticism of the Emanuel's thermodynamic model is of little relevance here.

The authors assert that condensational removal of water vapor that is held in an air parcel at a constant volume causes a decrease of its pressure when not considering the released latent heat that must occur with condensation. When the restriction of constant volume is removed, the decreased pressure causes air from the outside to move in, and so cause the convergence that can energize a storm.

For the sake of the argument, let's adopt this position of the authors, while considering along the same physical reasoning the consequence of the latent heat that must occur with the adiabatic condensational removal of the vapor.

An isobaric removal of water vapor from an air parcel should decrease its volume by the molar fraction of the vapor mixing ratio. If the volume is held constant the pressure should drops respectively. For example, lets take arbitrarily air at the cloud base in the hurricane can be at dew point of 25°C or T0=298.2 K, pressure of P0=950 hPa, and vapour mixing ratio of 20 g of vapor per kg of air. The specific values do no make a difference here with respect to making the point. The ratio of molecular weight of vapor with respect to dry air is given by ε=0.622. Therefore, the volumetric mixing ratio is (20/ε)/1000=0.032.

If all that water was somehow suddenly removed in a constant volume at 950 hPa without allowing additional air to move in and without release of latent heat of condensation, this would incur a corresponding pressure drop of 950$\times$0.032=30.4 hPa. The depressurization would induce an adiabatic cooling of about 3 K, which would increase the pressure drop to about 40 hPa. The air parcel temperature and pressure would be T1=295.2 K and P1=910 hPa, respectively.

However, the condensation does release latent heat in the physical world, and that process is adiabatic in the free atmosphere. Therefore, if the condensational removal of vapor occurs

adiabatically at a constant volume, there would be a respective latent heat release at a rate that would increase the air temperature by dT = w L /Cv where w = 0.020 kg of condensed water vapor, the latent heat of condensation of vapour into liquid water is L=2,501,000 J kg-1 , and the heat capacity of air at constant pressure is Cv=719 J kg K-1.

Using these values result in dT=3.5 K for each condensed gram of vapor per 1-kg of air. In our example, condensation of 20 g should increase the air temperature by 70 K. This should increase the pressure of the air by a factor of (298.2+36)/298.2=1.235, which amounts to a pressure rise of 223 hPa. The air parcel temperature and pressure would be T2=365.2 K and P2=1173 hPa, respectively.

The bottom line is that in processes that occur at a constant volume the pressure drop by adiabatic condensation is overcompensated by latent heat induced pressure rise of the air. The authors considered only the condensational decompression part while neglecting the inherently coupled latent heat-induced compression that is more than five times larger in magnitude and hence dominates the changes in air pressure.

The fact that this process occurs isobarically and gradually along the ascent of the air parcel does not change the relative magnitudes of the two terms and the eventual result at the top of the ascent of the air parcel where practically all vapor is condensed. To demonstrate it we can decompose this thermodynamic process into the constant volume process that was calculated above, followed by isobaric expansion to a reference pressure. This is best done by calculating the potential temperatures θ, using θ=T (1000/P)$^{0.287}$, where P is the air pressure in hPa. The results are:

The reference case: θ0=T0 (1000/P0)$^{0.287}$=298.2(1000/950)$^{0.287}$=313.9 K

After the removal of the vapor: θ1=T1 (1000/P1)$^{0.287}$=295.2(1000/910)$^{0.287}$=302.3 K

With the latent heating added: θ2=T2 (1000/P2)$^{0.287}$=365.2(1000/1173)$^{0.287}$=348.9 K

If all condensation would have occurred at a given constant level, or gradually with the ascent of the air, the eventual potential temperature at the top of the ascent, where all vapor have condensed, would be the same. This clearly demonstrates that condensation actually warms and hence expands the air more than the removal of the vapor cools and hence shrinks the air volume, as opposed to the claim of the authors.

(This could have been also calculated directly using the more traditional way of isobaric change of temperature and using Cp, the air heat capacity at a constant pressure. The step of constant volume calculation was used here to address the considerations of the two components of pressure change due to condensation and latent heating.)

In summary, taking into consideration for an adiabatic parcel *in a fixed volume* both decompression due to condensation and compression due to the inevitable latent heating that must occur with the condensation, demonstrates that the latent heat induced compression overwhelms the condensational decompression. Therefore, accepting the reasoning of the authors, who assert that the condensational decompression energizes hurricanes, leads to a contradiction with the calculations presented here. This contradiction renders the hypothesis presented by the authors invalid.

Appendix 6: Comment of Referee A5

Generally, I am open to novel ideas and approaches. Novel ideas, however, need to be on solid grounds and ought to demonstrate a perspective how this novel idea can be better than previous ones in order to be useful. In reading through the revised manuscript, I can only say that this manuscript is seriously flawed and does not meet the standards for publication.

In the first part, the authors use textbook classical thermodynamics in an attempt to show that Emanuel's hurricane model violates the second law of thermodynamics. Emanuel's model, however, is based on non-equilibrium thermodynamics, and not on classical equilibrium thermodynamics. By using classical equilibrium thermodynamics, thus not allowing for entropy exchanges to the surroundings, the authors choose an inappropriate approach. After all, a system in thermodynamic equilibrium cannot perform work and dissipate energy in steady state. For such non-equilibrium systems to be sustained (work performed = dissipation in steady state), a net entropy exchange to the surroundings needs to take place to export the entropy produced by dissipative processes within the system. In case of Earth system processes, the net entropy exchange ultimately takes place at the Earth-space boundary.

Specifically, the authors state in the manuscript (section 3.2) that "We thus have an engine that does not receive any net energy input from the environment, but recirculates dissipated heat to work and back within itself at a potentially infinite rate." This statement is incorrect, and therefore one of their major conclusions is flawed. It ignores the non-equilibrium thermodynamic nature of a hurricane. In steady state, heating equals cooling (which the authors interpret as not receiving any net energy input), but the addition and removal of heat takes place at very different temperatures. This results in no net heating (i.e. temperatures are in a steady state), but in a net entropy exchange, that is, heating and cooling take place at different temperatures. In steady state, the entropy produced by dissipative processes within the system equals the net entropy export. This basic theoretical background on how the second law applies to non-equilibrium, dissipative systems can be found in any introduction to non-equilibrium thermodynamics. Entropy is imported to the hurricane at a rate delta Qin/Tin (where delta Qin and Tin correspond to the authors' variables delta Qs, surface heating, and Ts, surface temperature), and exported at a rate of delta Qout/Tout (where delta Qout is as in the manuscript and Tout = T0). Hence, the net entropy export is delta Qout/Tout–delta Qin/Tin, which is the rate of entropy production sigma by irreversible processes within the system in steady state. Hence, we have sigma = delta Qout/Tout–delta Qin/Tin = delta Qin (Tin–Tout)/(Tout Tin) (assuming steady state: delta Qout = delta Qin) and further sigma = A/Tin (using A = (Tin–Tout)/Tout delta Qin as in the manuscript). In other words, in steady state we have work performed A = frictional dissipation D $\propto$ net entropy exchange to the surroundings. These equations directly follow from the second law of thermodynamics as extended to non-equilibrium thermodynamic systems. Reviewer 1 made important and valuable comments regarding this, but unfortunately the authors apparently did not consider these in their revision. Hence, the authors' conclusion as expressed in the abstract "... that the existing thermodynamic theory of hurricanes ... is not physically consistent, as it comes in conflict with the laws of thermodynamics." is fundamentally flawed.

The second part of the manuscript attempts to attribute the driving force of hurricanes to a novel "condensational" force. This proposed force is supposedly caused by gradients in water vapor pressure alone. Using Occam's razor, we should ask first if such a new force is necessary to properly explain larger-scale circulations such as those found in a hurricane. Given Emanuel's relatively simple theory of hurricanes works rather well, I do not see a justification for demanding a novel force. Such justification would only exist if the authors were able to show that their explanation is in better agreement with observations than Emanuel's model. The authors do not present any results that demonstrate such an improvement.

In more general terms, I also cannot see the need for this novel force using basic arguments of the driving forces that set the atmosphere into motion. The common assumption in atmospheric science is that the air at small scales is sufficiently mixed. The authors do not show any observations that would show that this common assumption in atmospheric science is flawed. Given this simple assumption of mixing, any gradient in vapor pressure e is compensated by a countergradient in dry air (p–e) if the total air pressure p is constant for simplicity, so that no net force solely due to gradients in water vapor can arise. The same argument would hold for pressure gradients, i.e. motion would result from the gradient in total pressure into which gradients of partial pressures are subsumed (including vapor pressure gradients that are compensated by gradients in dry air pressure).

In addition, Reviewer 4 makes a convincing back-of-the-envelope estimate that "the local drop of air pressure that arises during condensation and its disappearance from the gas phase" as expressed in the abstract (which seems to be different to the novel force that the authors

propose), is of little relevance since the latent heat release during condensation overpowers such an effect by an order of magnitude.

That this novel force cannot play a dominant role in driving atmospheric circulations is not surprising. After all, there are atmospheric circulations that do not involve the hydrologic cycle, e.g. the land-sea breeze circulation, or the atmospheric circulation on Venus. On the other hand, there cannot be a hydrologic cycle without atmospheric motion. Critical work on this aspect (including a thorough thermodynamic treatment that includes the non-equilibrium nature of the hydrologic cycle) is done by Pauluis and Held (2002), which the authors should pay much closer attention to before claiming the necessity of a novel force.

Most of this (and more) has been pointed out by the reviewers, but the authors, again, do not seem to pay much attention to these criticisms.

In conclusion, the authors' conclusions are based on fundamental flaws. I see no prospect that these flaws can be suitably addressed in a revision and therefore recommend rejection.

Appendix 7: Response of Authors, Dr. Anastassia Makarieva et al.

In our original manuscript submitted to ACPD 7 May 2008 we outlined the physical foundations of a new theory of atmospheric circulation. The theory provides a unified quantitative description of such atmospheric phenomena as (1) the large-scale (continental and oceanic) circulation, (2) hurricanes and (3) tornadoes. Noteworthy, the standing meteorological paradigm relies on the horizontal differential heating to explain the **moderate** winds of the large-scale circulation patterns; in a logical controversy, it then radically abandons the concept of differential heating to describe hurricanes (the **strong** winds) as originating due to heat extraction from the horizontally isothermal oceanic surface; finally, it does not possess any coherent physical theory for tornadoes (the ever **strongest** winds developing over land) (see more on comparison between the new theory and the meteorological paradigm and its problems in Makarieva and Gorshkov 2009a, Section 4). We have shown that all these phenomena arise due to water vapor condensation that occurs in the atmosphere on a variety of spatial scales. Since May 2008 the theory has become much more detailed (Makarieva and Gorshkov, 2009b,c). Sound numerical estimates of the wind wall and eye radius and wind velocity profiles (tangential, radial and vertical) for hurricanes and tornadoes are produced by the new theory using but a limited number of fundamental atmospheric parameters like the circulation radius and surface roughness (Makarieva and Gorshkov, 2009c). We invite the Editors to pay a special attention to the fact that the theory, after an account of the radial symmetry has been made, predicts a maximum pressure drop in hurricanes to equal $2.5\Delta p$, where Δp is the drop of air pressure due to water vapor condensation. This should eliminate a previous concern of Referee 1 (http://www.cosis.net/copernicus/EGU/acpd/8/S12168/acpd-8-S12168.pdf, p. S12174), who noted that while the maximum value is $\Delta p{=}40$ mb at 30°C, the observed pressure drop in hurricanes is larger and can reach 100 mb.

Here we present several considerations in response to the newly available comments of the referees. Most importantly, the conclusions of Referee A4 (supported by Referee A5) about the effects of condensation on air pressure, on which the ACP Executive Committee have largely based their final decision, are physically flawed. They are also in conflict with some other relevant considerations present in the mainstream meteorological literature.

Referee A4 states that condensation at **constant volume** leads to an increase of air temperature due to latent heat release and that the rise of air pressure associated with that tempera-

ture increase overwhelms the reduction of air pressure due to removal of water vapor molecules from the gas phase. First, such a process is physically impossible. Condensation in a saturated air parcel occurs only after the parcel's temperature **drops**, it **never** leads to a temperature rise, as prohibited by the laws of thermodynamics. The reader might wish to take a closed jar (constant volume) filled with saturated water vapor and make sure for him/herself that condensation will never spontaneously occur until the jar is **cooled**. This is what the fundamental Clausius-Clapeyron law is about – the saturated concentration of vapor is a function of temperature; it diminishes (i.e. condensation occurs) only after the temperature decreases.

The quantitative details of the physical error behind the conclusion of Referee A4 are as follows. For an adiabatic process the second law of thermodynamics reads as $dQ = 0 = C_V dT + pdV + Ld\gamma$, where $\gamma \equiv p_v/p$ is the relative partial pressure of water vapor, which changes during phase transitions only; L is the molar heat of vaporization; V, T and p are the molar volume, absolute temperature and pressure of air, respectively. Considering the process of adiabatic condensation at constant volume Referee A4 essentially puts $pdV = 0$ to obtain $-Ld\gamma = C_V dT$. Since during condensation $d\gamma < 0$ (the vapor content diminishes), it is concluded that condensation induces a temperature rise with $0 < dT = -Ld\gamma/C_V$. This conclusion **totally ignores the Clausius-Clapeyron law**, which relates the change in saturated water vapor partial pressure p_v to the change of temperature T as $dp_v/p_v = [L/(RT)]dT/T$, where R = 8.3 J/mol/K is the universal gas constant. Noting that $d\gamma = \gamma(dp_v/p_v - dp/p)$, using the Clausius-Clapeyron law and the ideal gas law, $pdV + Vdp = RdT$, and putting $dV = 0$, we have $d\gamma = \gamma[(L - RT)/RT]dT/T$. Putting now this expression for $d\gamma$ into the second law of thermodynamics equation for adiabatic condensation at constant volume, $-Ld\gamma = C_V dT$, we have $(-L[(L - RT)/RT] - C_V T)dT = 0$. Due to $C_V > R$, the multiplier $(-L[(L - RT)/RT] - C_V T)$ at dT is always less than zero, which means that the equation has a single solution, namely $dT = 0$. Recalling that $dQ = 0 = C_V dT + pdV + Ld\gamma$, at $dV = 0$ and $dT = 0$ we have $d\gamma = 0$, i.e. **no phase transitions may take place adiabatically at constant volume**. The calculations of Referee A4 were made for a thermodynamically prohibited process and are not physically sound. **In no case can condensation be associated with a rise of temperature of the saturated air parcel where it occurs.**

Latent heat release diminishes the temperature drop for any given amount of energy (heat or work) removed from the air parcel where condensation takes place. All calculations of the evaporative/condensational force effects were made for the observed mean temperature lapse rate of 6.5 K/km (making them for the moist adiabatic lapse rate of 4-5 K/km would not change the results in any significant way). Due to the latent heat release in the ascending air parcels the global mean lapse rate is smaller than it would be in a dry atmosphere (9.8 K/km). Thus, when, as done in our work, the effect of condensation on air pressure is considered **for a given vertical temperature profile**, such a consideration, in contrast to the statement of Referee A4, already takes into account the effect of latent heat release and does so in a physically consistent way.

Second, observing that the atmospheric column exists in the gravitational field, one recalls that there is an (approximate) equality between the air pressure and the weight of the air column above the considered point. Weight of the air column clearly does not depend on gas temperature, but is determined by the number of gas molecules in the column only. (For this reason, for example, the sea level pressure on Earth is nearly the same on the pole and on the equator, despite the substantial surface temperature differences.) Therefore, water vapor condensation within the atmospheric column almost instantaneously reduces (not increases!) local air pressure at the surface. One can refer to Makarieva and Gorshkov (2009a) for details, if necessary, but otherwise this obvious and fundamental physical fact appears to be readily appreciated by many meteorologists. For example, Dool and Saha (1993) (see their Eq. 6) indicate that the local temporal change dp/dt of surface air pressure p is the sum of the rate of air advection (i.e. the horizontal influx of air into the considered area) AND the rate of the local change of water vapor content in the atmospheric column, $E - P$. The latter is equal to the difference between evaporation E (which increases the local air pressure) and condensation (precipitation) P (which decreases the local air pressure). Trenberth (1991) is explicit on the same point (see his Eq. 5). The radically contrasting conclusion of Referee A4 and its support by Referee A5 and by the entire ACP Executive Committee do, in our opinion, warn one clearly about the possible absence of a universally appreciated physically coherent ground under the current understanding of the fundamental atmospheric processes, namely water vapor phase transitions, in meteorology.

We note in passing that, in our view, Dool and Saha (1993) showed a good physical intuition by noting that the phase transition of water vapor can serve as a physically distinct "water vapor forcing" for atmospheric motions. However, this idea has not been fruitfully developed by either these or other authors (e.g., Lackmann and Yablonsky, 2004) because the attempts to account for the effect were made in the form of a model mass balance parameterization,

which neglected the energy balance. In brief, no account was made for the dynamic forces that arise in the atmosphere due to water vapor condensation and make the air move imparting some "additional" kinetic energy to the air masses (otherwise this energy would have to be postulated to come from nothing). In reality, when the dynamics of water vapor condensation is quantified, it appears to be the major driver of the observed winds (Makarieva and Gorshkov 2007, 2009b,c).

Responding to Referee A2, in our work Bernoulli's equation is not applied to partial pressure of water vapor, but to the change of pressure of moist air as a whole. Responding to Referee A5, the circulation on Venus does involve phase transitions and related phenomena, because the atmosphere contains liquid clouds of sulfuric acid and because the main atmospheric component on Venus (carbon dioxide) is beyond the critical point of the gas-liquid diagram. The physical importance of phase transitions in the atmosphere of Venus is undoubtedly as underestimated as it is on Earth. Notably, Martian circulation is also based on gas-liquid phase transitions. The land-sea breeze circulation is the only circulation on Earth that does not involve water vapor condensation; remarkably, the global impact of this circulation is negligible. Breezes, these weak winds, demand a huge horizontal temperature gradient (in the order of several degrees Kelvin per kilometer) to arise. This gives an idea of the negligibly small intensity of global circulation that would persist on Earth governed by the characteristic global horizontal temperature gradients that are hundreds of times smaller, if one removed water vapor from the terrestrial atmosphere.

Regarding the heat engine model of hurricanes of K. Emanuel we emphatically maintain that the model is physically flawed. In no case can dissipation of heat within the engine (or anywhere else) increase the efficiency of the engine and the work produced by it (within itself or elsewhere). First, any physically realistic heat engine is constructed to receive a given amount of heat Q_s from the heater to perform some work A. Heat can be received only when the temperature of the engine is somewhat lower than that of the heater. When this work is dissipated within the engine, this leads to heating within the engine, which **lowers (!)** the amount of heat Q_s received from the heater by precisely an amount of the internally dissipated heat. That is, when work A is dissipated within the engine, the value of Q_s diminishes. This is what is missed in the formulae for entropy change listed by Referee 1 and Referee A5. In the result, even if one artificially manages to synchronize the times of dissipation of work A and the time taken by the warmer isotherm of the engine (these are independent physical processes), the maximum amount of work produced by the engine will still remain A, i.e. will be equal to the maximum work given by the Carnot formula. Second, any realistic (non-equilibrium) heat engine must have a dynamic mechanical driver that makes the engine move to compress and expand the gas (e.g., a compressing and decompressing metallic spring). It is not that the engine operates because it receives heat from the heater, but, conversely, it can only receive heat because some parts of the engine are moved by the driver. This driver must possess a certain store of potential energy to make the engine move. In order to depict hurricanes as heat engines, one must identify such a store of potential energy in the atmosphere. In other words, one should, for example, identify what makes air move along the sea surface for the Carnot isotherm to be possible. (The Carnot cycle is by definition an equilibrium cycle and cannot begin by itself. For the cycle to have a finite period, it must be externally driven.) But as soon one identifies the reason of why the air moves along the surface at a given velocity, one no longer needs any further explanations for the air movement. The Carnot heat engine model (whether dissipative or not) is thus meaningless. We learnt from the discussion that these considerations should be presented in a much more detailed form; the interested reader may refer to (Makarieva and Gorshkov, in preparation, http://arxiv.org/abs/0910.0543). It is shown that the dissipative heat engine concept first advanced by Rennó and Ingersoll (1996) and later employed to described the hurricane development is physically inconsistent.

Regarding the publication process as whole, we are grateful to all scientists who participated in the evaluation of our results and spent their time on following our arguments. We express our sincere appreciation to the ACP Editorial Board and Executive Committee for the firm adherence to the standards of openness and scientific freedom of speech and exchange of opinions. In our opinion, the discussion has clearly revealed a critical need for an urgent and deep physical re-validation of the current meteorological lines of thought (see also Makarieva and Gorshkov, 2009a). In our view, such a re-analysis could be most fruitfully performed by involving independent physicists not belonging to the “meteorological school” (using the wording of Referee A3), in constructive cooperation with meteorologists. We have unfortunately to agree (cf. the comments of Referee A3) that some emotional clashes have been unavoidable in the particular case of our work. We nevertheless wish to hope that the long path of our work in the ACPD, its extensive discussion and wide public exposure, will finally serve to attract (rather than discourage) the potentially interested and capable scientists, physicists and meteorologists as well, to participate in the exciting scientific endeavor of further developing the condensational

theory of atmospheric circulation on their own. We are firmly convinced that the presented approach will ultimately be appreciated as the physical basis of most of the atmospheric circulation theory excluding, perhaps, the breezes only.

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References

Dool H.M. van den, Saha S. (1993). Seasonal redistribution and conservation of atmospheric mass in a general circulation model. J. Climate, 6, 22-30.

Lackmann G.M., Yablonsky R.M. (2004) The importance of the precipitation mass sink in tropical cyclones and other heavily precipitating systems. J. Atm. Sci., 61, 1674-1692.

Makarieva A.M., Gorshkov V.G. (2007) Biotic pump of atmospheric moisture as driver of the hydrological cycle on land. Hydrology and Earth System Sciences, 11, 1013-1033.

Makarieva A.M., Gorshkov V.G. (2009a) Reply to A. G. C. A. Meesters et al.'s comment on "Biotic pump of atmospheric moisture as driver of the hydrological cycle on land". Hydrology and Earth System Sciences, 13, 1307-1311.

Makarieva A.M., Gorshkov V.G. (2009b) Condensation-induced dynamic gas fluxes in a mixture of condensable and non-condensable gases. Physics Letters A, 373, 2801-2804.

Makarieva A.M., Gorshkov V.G. (2009c) Condensation-induced kinematics and dynamics of cyclones, hurricanes and tornadoes. Physics Letters A, in press, doi:10.1016/j.physleta.2009.09.023

Rennó N. O., Ingersoll A.P. (1996) Natural convection as a heat engine: A theory for CAPE. J. Atmos. Sci., 53, 572-585.

Trenberth K.E. (1991) Climate diagnostics from global analyses: conservation of mass in ECMWF analyses. J. Climate, 4, 707-722.

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