Atmos. Chem. Phys. Discuss., 8, S8931–S8934, 2008
www.atmos-chem-phys-discuss.net/8/S8931/2008/

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.

Received and published: 10 November 2008

Here we respond to comments of Dr. Meesters (hereafter SCM) on Section 2, Section 3.1 and Section 3.3 in the discussion paper. We note that Dr. Meesters has put forward an arguably irrelevant claim that the authors have been remarkably careless in criticizing other people's work. We believe that a scientific comment of relevance is the one on whether the critique is correct or not, rather than the one on possible behavioral reasons of why it could be so. We hope to refute this undeserved reproach in our response and would appreciate equal responsibility from Dr. Meesters in his further comments, if any.

Section 2: We appreciate that Dr. Meesters has refrained from dicussing the dissipative heat engine by noting that this is a subtle issue. However, this issue can be characterized as subtle in political terms only, because its positive discussion would

demand an open defense of the perpetual motion machine concept. However, it is not correct that "definite flaws in any part of the conceptual model are not pointed out". We repeat it for the fourth time in this discussion that the dissipative heat engine involves dissipation and regeneration of mechanical work A at one and the same surface temperature T_s. To substantiate this claim we offered a detailed consideration of Carnot cycle (Makarieva et al. (2008) ACPD 8: S7325). So far none of the discussion participants who took an explicitly critical attitude towards the discussion paper (Anonymous Referee 1, Anonymous Referee 3 and now Dr. Meesters) has ever mentioned this issue. (In our view, if one cannot make any specific comments on such a fundamental issue like the violation of the laws of thermodynamics, the most responsible route would be not to mention the issue at all.) We have already responded to the claim of Referee 1 regarding heat dissipation to space and the quantitative problems that this issue involves (Makarieva et al. (2008) ACPD 8: S8193). In (SCM) no new statements are made on the issue.

Section 3.1: Regarding integration of Bernoulli's equation from point a to point c it is noted in (SCM) that the neglect of the wind speed term $v_c^2 - v_a^2$ is acceptable, since it is two orders of magnitude smaller (10^2 m^2 s^{-2}) than the retained term $\alpha dp \sim 10^4$ m^2 s^{-2}. It is not specified in (SCM) where these estimates come from, but the first one is incorrect. We quote Emanuel (1991), legend to Fig. 1: "The hurricane Carnot cycle. Air begins spiraling in toward the storm center at point a..." and then second line from top on p. 185: "...assuming that v is zero at the beginning of the cycle". Hence, we have $v_a = 0$, which means that $v_c^2 - v_a^2 = v_c^2$. We now note that Bernoulli's equation is valid ***along the streamline***. As is well-known, hurricane wind speed reaches its maximum near the hurricane wall, where air starts to spiral in the upward direction. Therefore, the ultimate point ***along the horizontal streamline*** where Bernoulli's equation is still valid, is the point of maximum wind speeds. Given these of the order of 60 m s^{-1} for comparable $\Delta p = 50$ mbar (see, e.g., (Holland (1980) Mon. Wea. Rev. 108: 1212, Fig. 5b)) we have $v_c^2 - v_a^2$ = 3600 m^2 s^{-2} and $\alpha \Delta p = \Delta p/\rho$ = 4000 m^2 s^{-2}. Notably, the coincidence in the order of magnitude of the two figures is in full quantitative agreement

ACPD

8, S8931–S8934, 2008

with the proposed approach, see p. 17434 in the discussion paper.

Possibly the error in the estimate provided by Dr. Meesters (10^2 m^2 s^{-2} for $v_c^2 - v_a^2$) arose from the fact that Bernoulli's equation was integrated until the very center of the eye, where the hurricane-force wind speeds "fall again" to nearly zero, neglecting that the horizontal streamline along which the integration can only be performed ends near the hurricane wall ***without piercing through it***. In summary, neglecting the wind speed term by Emanuel (1991) is unjustified, as this term makes a major contribution to the considered integral, contrary to what is stated in (SCM). We also note that Dr. Meesters makes no mention of the fact that the central formula for work in Emanuel (1991) contradicts the one directly obtained from consideration of Carnot cycle (Makarieva et al. (2008) ACPD 8: S7325). Neither Referee 1 nor Referee 3 mentioned this fact either.

Section 3.3: It is stated in (SCM) that the calculation of dissipative heating rates by Bister and Emanuel (1998) is correct because of the fact that turbulent kinetic energy has no other route rather than to be converted into heat and that, since "production equals dissipation", it is enough to know the rate of turbulent kinetic energy production to know the (equal) rate of dissipative heating. However, this logic misses the critical point that turbulent kinetic energy can be transported, in the form of small eddies, far away from the hurricane area and take a much longer time to dissipate into heat than the time of hurricane existence. Hurricane is a spatially and temporally localized event, preceded and followed by, as well as surrounded by, prolonged periods and large areas of relative calmness, when the small eddies carrying turbulent kinetic energy can take their full time to dissipate. There are no physical grounds to assume that turbulent kinetic energy converts to heat exactly at the same instantaneous rate as it is produced within the hurricane, as done by Bister and Emanuel (1998). There is no "production equals to dissipation" rule for the hurricane. Note also that for hurricanes to develop, even turbulent dissipation power should be small compared to the power of the pressure gradient force, as showed in the authors' comment published prior to the comment of Dr. Meesters (Makarieva et al. (2008) ACPD 8: S8904; note that Eq. (18)

ACPD

8, S8931–S8934, 2008

Interactive Comment

should contain multiplier (1/2) at u^2).

In summary, we emphasize that we would cordially invite all discussion participants before attempting to defend the criticized hurricane framework based on Carnot cycle, to pay attention to the consideration of Carnot cycle that is given in the authors' first comment (Makarieva et al. (2008) ACPD 8: S7325). This consideration is missing from the works of Emanuel (1991, 2003) and others and cannot be found there. In the meantime, it shows that the central formula of the approach is incorrect. This fact and the dissipative heat engine equivalent to the perpetual motion machine are two major points of our critique, not minor or subtle issues, but two major ones. It is, in our view, highly illogical to try to defend the framework explicitly avoiding discussion of these issues.

Finally, we agree with Dr. Meesters that it is absolutely irrelevant whether one has a background of a theoretical physicist, meteorologist or any other, provided one has a good understanding of physics and a keen interest in science. We believe that using this discussion as a small arena for another battle between different fields of science (see, e.g., Lahsen (2008) Global Environmental Change 18: 204) would be very unproductive.

Comment modified by Copernicus Publications on 28 November 2008.

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