CORRESPONDENCE

Reply to "Comments on 'An Evaluation of Hurricane Superintensity in Axisymmetric Numerical Models'"

KERRY EMANUEL[a] AND RAPHAËL ROUSSEAU-RIZZI[a]

[a] *Lorenz Center, Department of Earth, Atmospheric and Planetary Sciences, Massachusetts Institute of Technology, Cambridge, Massachusetts*

(Manuscript received 30 June 2020, in final form 14 September 2020)

ABSTRACT: We concur with Makarieva et al. that in our earlier work on the hurricane differential Carnot cycle, we neglected the work done in lifting water and the dissipation of kinetic energy in the outflow (we explicitly acknowledged neglecting these terms). Here, we relax those assumptions, affirm the conclusion of Makarieva et al. that the water lifting term is small, and show that the effect of outflow dissipation is negligible. We remind readers that the differential Carnot theory is not a closed theory for potential intensity as it does not specify the outflow temperature or the boundary layer moist enthalpy at the radius of maximum winds. The addition of enthalpy to the inflow can raise the boundary layer enthalpy, reducing subsequent surface fluxes, regardless of whether that addition comes from surface fluxes themselves or from dissipative heating. We show that while this may indeed reduce the effect of dissipative heating, it does not eliminate it. We disagree with Makarieva et al.'s assertions that dissipative heating does not increase potential intensity and that only latent heat fluxes can drive tropical cyclones when dissipative heating is included.

KEYWORDS: Tropical cyclones; Thermodynamics

1. Introduction

In our paper on tropical cyclone superintensity (Rousseau-Rizzi and Emanuel 2019; hereafter RE), we developed an identity from differentiating Carnot-type integrals around two adjacent circuits:

$$\oint_{\text{inner}} T\frac{ds}{dt} = -\oint_{\text{inner}} \mathbf{V}\cdot\mathbf{F} - \oint_{\text{inner}} \frac{dq_t}{dt}\left(\frac{1}{2}|\mathbf{V}|^2 + gz\right), \qquad (1)$$

where T is temperature, s is the specific moist entropy, $\mathbf{V}$ is the three-dimensional velocity, $\mathbf{F}$ is the frictional source of momentum, q_t is the total mass concentration of water, g is the acceleration of gravity, and z is altitude. The "inner" notation designates an integral along an infinitesimal radial segment at the radius of maximum winds, a path up a surface of constant angular momentum and moist entropy, an infinitesimal downward branch at large radius, and a path downward along an adjacent surface of constant angular momentum and entropy (see Fig. 1 of the original paper).

The left side of (1) represents the net heating around the circuit, which represents the maximum amount of work available from the cycle. The first term on the right side of (1) represents frictional dissipation of kinetic energy while the second term is the work done in lifting water substance and in accelerating it to the local wind speed.

In addition to assuming that the only entropy sources and sinks and frictional dissipation are in the infinitesimal segments at the top and bottom of the inner cycle, RE also explicitly neglected frictional dissipation in the top segment and all of the work done to lift and accelerate water. We used aerodynamic formulas for the surface momentum and enthalpy fluxes to evaluate (1) as

$$\frac{T_s - T_{\text{out}}}{T_s}[C_{k10}|\mathbf{V}_{10}|(k_0^* - k_{10}) + C_{D10}|\mathbf{V}_{10}|^3] = C_{D10}|\mathbf{V}_{10}|^3, \qquad (2)$$

where $|\mathbf{V}_{10}|$ is the magnitude of the 10-m wind speed, C_{k10} and C_{D10} are the surface exchange coefficients for enthalpy and momentum, T_s and T_{out} are the absolute temperature at the surface and in the top segment, and $k_0^* - k_{10}$ is the difference between the saturation moist enthalpy of the sea surface and the most enthalpy of the boundary layer at 10 m. [One could substitute any reasonable reference height of use in the bulk aerodynamic formulas in (2).]

The term in brackets on the left side of (2) represent the net enthalpy source in the boundary layer from surface fluxes and from dissipative heating (e.g., see Batchelor 1967). The term on the right represents boundary layer dissipation of kinetic energy.

Solving (2) for $|\mathbf{V}_{10}|$ yields an expression for the potential intensity:

$$|\mathbf{V}_{10}|^2 = \frac{C_{k10}}{C_{D10}}\frac{T_s - T_{\text{out}}}{T_{\text{out}}}(k_0^* - k_{10}). \qquad (3)$$

We note here, as we did in RE, that (2) is not a closed expression for potential intensity because we have not specified either the outflow temperature T_{out} or the boundary

Denotes content that is immediately available upon publication as open access.

Corresponding author: Raphaël Rousseau-Rizzi, rrizzi@mit.edu

DOI: 10.1175/JAS-D-20-0199.1

layer enthalpy k_{10}. This will be important in some of what follows.

We next join Makarieva et al. (2020; hereafter ME) in examining some of the approximations alluded to above.

2. Cartoon of the differential Carnot cycle

We agree with ME that the integrals constituting the sides of our cycle must be along streamlines. As we stated in our response (Rousseau-Rizzi and Emanuel 2020) to Montgomery and Smith (2020), our cartoon of the cycle was motivated by the assumption that horizontal gradients in the distant environment of tropical cyclones are typically very weak, so that there is no thermodynamic or kinematic difference between points D′ and D and between A′ and A, referring to ME's Fig. 1b. Thus, RE drew them as the same points in their Fig. 1. The reader is welcome to conceptualize the argument using ME's Fig. 1b, as long as it is recognized that the properties of D′ and D, and of A′ and A are identical.

3. Work used in lifting and accelerating water

The last term in (1) represents the work done in lifting and accelerating water. Water is added in the bottom infinitesimal segment by evaporation from the ocean, and we note that even though we can define $z = 0$ there, some work must be expended to accelerate the water vapor to the free stream velocity. But this is a very small term in our atmosphere, since changes in kinetic energy are very small compared to changes in potential energy (even in a hurricane), and we neglect it here. The rest of the term arises from lifting of water substance, and an upper limit on its magnitude can be found by assuming that all of the gain in water from evaporation at the sea surface is lost (to close the cycle) in the top infinitesimal segment of the circuit. In that extreme case, and using the bulk aerodynamic formulas again, evaluating (1) yields an extra term on the right side of (2):

$$\frac{T_s - T_{\text{out}}}{T_s}[C_{k10}|\mathbf{V}_{10}|(k_0^* - k_{10}) + C_{D10}|\mathbf{V}_{10}|^3] = C_{D10}|\mathbf{V}_{10}|^3 + C_{E10}|\mathbf{V}_{10}|(q_0^* - q_{10})gH, \qquad (4)$$

where C_{E10} is the coefficient of surface evaporation, q_0^* is the saturation specific humidity of the sea surface, q_{10} is the specific humidity at 10 m, and H is the altitude of the top segment. If, for the sake of scaling, we assume that $C_E \sim C_k$ and that $k_0^* - k_{10} \sim L_v(q_0^* - q_{10})$, where L_v is the latent heat of vaporization, then the ratio of the new term [last term in (4)] to the first term in (4) is

$$\frac{gH}{L_v}\frac{T_s}{T_s - T_{\text{out}}}.$$

This is essentially the result derived by Makarieva et al. (2018). Taking $H = 10$ km, $g = 10\,\text{m}\,\text{s}^{-2}$, $L_v = 2.5 \times 10^6\,\text{m}^2\,\text{s}^{-2}$, $T_s = 300$ K, and $T_{\text{out}} = 200$ K yields a value of this ratio of 0.12.

Thus, even with the extreme assumption of reversible water loading, this water lifting term is only ~10% of the surface enthalpy flux term. But, as pointed out by Makarieva et al. (2018), most water falls out well below the top of the cycle and so the effective value of H in (4) is somewhat smaller than the depth of the troposphere, thus, the real effect is somewhat smaller. We concur with ME and with our previous assertion in RE that this term is small.

4. Outflow dissipation

ME also point to the possible importance of dissipation in the outflow, which RE neglected. ME's Eq. (10) accounts for the outflow dissipation and expresses the effect as the change in kinetic energy along the top segment of the circuit. Here we attempt to take their derivation one step further by including this contribution to dissipative heating and by relating the change in kinetic energy to the change to surface dissipation of angular momentum. We prefer here to work with rates of change of entropy and energy rather than changes of those quantities along streamlines; that is, we wish to estimate the contribution of outflow dissipation to the left side of (1).

We first note that for the circuit to be closed in angular momentum, the change of angular momentum along the top segment must be equal and opposite to that along the bottom segment. We make several assumptions about the outflow dissipation: First, we assume that the top segment of the circuit is oriented vertically, as in the diagrams of ME and RE. Second, we assume that the total kinetic energy dissipation is dominated by that of the azimuthal flow and neglect direct dissipation of the kinetic energy of the radial and vertical wind components.

Then in that top segment,

$$(\mathbf{F}\cdot\mathbf{V})_{\text{top}} \cong V\frac{dV}{dt} = \frac{V_t}{r_t}\left(\frac{dM}{dt}\right)_{\text{top}}, \qquad (5)$$

where M is the absolute angular momentum per unit mass, V_t is the azimuthal velocity at the top segment, and r_t is the radius of the top segment of the circuit. We next notice that time rate of change of angular momentum along this top segment must be equal and opposite to the change along the bottom segment, which we get from the drag formula:

$$\left(\frac{dM}{dt}\right)_{\text{top}} = -\left(\frac{dM}{dt}\right)_{\text{bottom}} = C_{D10}r_m V_{10}|\mathbf{V}_{10}|, \qquad (6)$$

where r_m is the radius of maximum winds and V_{10} is the azimuthal wind speed at the reference height.

Moreover, conservation of angular momentum along the streamline emanating from the radius of maximum winds and going to the top segment gives that

$$V_t = \frac{M}{r_t} - \frac{1}{2}fr_t = \frac{r_m}{r_t}\left(V_{10} + \frac{1}{2}fr_m\right) - \frac{1}{2}fr_t, \qquad (7)$$

where f is the Coriolis parameter. Here we have expressed M as the angular momentum at the radius of maximum winds.

Substituting (6) and (7) into (5) gives

$$-(\mathbf{F}\cdot\mathbf{V})_{\text{top}} \cong C_{D10}V_{10}|\mathbf{V}_{10}|\left\{\frac{1}{2}fr_m\left[1-\left(\frac{r_m}{r_t}\right)^2\right] - V_{10}\left(\frac{r_m}{r_t}\right)^2\right\}. \qquad (8)$$

Finally, we assume that $r_t \gg r_m$ in (8) and substitute the result into (1) along with the traditional boundary layer dissipation and the other terms, being careful also to include this contribution to the dissipative heating. Thus, (4) becomes

$$\frac{T_s - T_{\text{out}}}{T_s}[C_{k10}|\mathbf{V}_{10}|(k_0^* - k_{10}) + C_{D10}|\mathbf{V}_{10}|^3] + \frac{1}{2}\frac{T_{\text{out}} - T_s}{T_{\text{out}}}C_{D10}|\mathbf{V}_{10}|fr_m V_{10} = C_{D10}|\mathbf{V}_{10}|\left(|\mathbf{V}_{10}|^2 + \frac{1}{2}fr_m V_{10}\right) + C_{E10}|\mathbf{V}_{10}|(q_0^* - q_{10})gH. \qquad (9)$$

Rearranging the terms in (9) gives

$$C_{D10}\left[|\mathbf{V}_{10}|^2 + \frac{1}{2}fr_m V_{10}\left(\frac{T_s}{T_{\text{out}}}\right)^2\right] = \frac{T_s - T_{\text{out}}}{T_{\text{out}}}C_{k10}(k_0^* - k_{10}) - \left(\frac{T_s}{T_{\text{out}}}\right)C_{E10}gH(q_0^* - q_{10}). \qquad (10)$$

Note that both the water lifting and outflow dissipation terms in (10) reduce the potential intensity. The outflow dissipation is here represented by the $(1/2)fr_m V_{10}$ term in (10). We can neglect this if

$$\frac{1}{2}fr_m\left(\frac{T_s}{T_{\text{out}}}\right)^2 \ll V_{10}, \qquad (11)$$

ignoring the difference between V_{10} and $|\mathbf{V}_{10}|$ for the purposes of this comparison. If $f \simeq 5 \times 10^{-5}\,\text{s}^{-1}$ and even if $r_m = 100$ km, the left side of (11) is only about $5\,\text{m s}^{-1}$, so the inequality (11) is well satisfied for most mature storms. We conclude that outflow dissipation has a small effect on potential intensity.

5. Dissipative heating and surface heat flux

Dissipative heating makes an important contribution to the thermodynamic cycle of tropical cyclones, as first shown by Bister and Emanuel (1998). ME's assertion that since work against friction is already present in (1) it does not contribute to entropy gain is false, as demonstrated by Bister et al. (2011). A simple thought experiment suffices to show this: if friction with the sea surface is replaced by wind turbines that extract the same quantity of kinetic energy from the flow, the first term on the right side of (1) remains the same but the energy is exported from the system rather than internally dissipated, so the entropy source in the inflowing air is reduced. The thermodynamic cycle is different.

Moreover, when the thermodynamic and momentum equations are integrated in full-physics numerical models, heating by internal dissipation of kinetic energy is observed to make a large difference in the intensity of the simulated cyclones, as first demonstrated by Bister and Emanuel (1998) and in several other models (e.g., Bryan 2012). The energetics of these models are fully internally consistent.

As a consequence of their omission of dissipative heating in the entropy gain term in (1), ME arrive at an internal contradiction, namely, that (neglecting the water lifting and outflow dissipation terms) all of the heating in the inflow must be due to dissipation [the $\mathbf{F} \cdot \mathbf{V}$ term on the right side of (1)]; that is, the entropy gain given by the left side of (1) must equal the dissipative heating implied by the first term on the right side of (1). There can be no room for addition of entropy by surface fluxes. (This would be true regardless of the apportionment of the surface enthalpy flux between sensible and latent heat fluxes.) In essence, they have disproved their own assertion that dissipative heating should not be included in the entropy gain term. This leads them to the incorrect conclusion that the sensible component of surface enthalpy flux can play no role. Their Eq. (16) is wrong and inconsistent with the numerical results of RE and radically inconsistent with the successful simulations of completely dry tropical cyclones by Mrowiec et al. (2011) and by Cronin and Chavas (2019).

To illustrate the importance of dissipative heating even in the absence of latent heat fluxes, we ran dry simulations using the same model [Cloud Model 1 (CM1); Bryan and Fritsch 2002] in a similar configuration as in RE but initializing with a dry adiabatic environmental temperature profile in the troposphere, and an air–ground temperature difference of 10 K. The results, displayed in Fig. 1, show that the intensity is about 30% higher when dissipative heating is included. Yet there is a negative feedback in the system that could diminish (but not eliminate) the effect of dissipative heating. Recall that (1) is not a closed solution for the potential intensity, mostly because the boundary layer enthalpy at the radius of maximum winds, k_{10}, is left unspecified. Insofar as dissipative heating elevates k_{10}, it diminishes the potential intensity, though recall that (3) is already substantially larger than the result without dissipative heating, because its denominator is T_{out} rather than T_s. Also note that by making the storm more intense, dissipative heating would increase the radial inflow in the boundary layer, which would tend to reduce k_{10}. Numerical solutions, such as those cited above and illustrated in Fig. 1, contain all of the important boundary layer physics that determine k_{10} and leave no doubt that while its elevation by dissipative heating can diminish the additional intensity owing to dissipative heating, that addition is still quite positive.

Finally, we mention that not all of the dissipation induced by interaction between the airflow and the surface ends up heating the boundary layer. Some turbulence is dissipated within the ocean, and up to 10%–30% of the wind energy is used to generate ocean surface waves, which carry the energy away from the storm and dissipate it remotely (Kieu 2015). Thus, the actual dissipative heating of the boundary layer is likely less than what has been assumed here.

6. Summary

We have shown here that the contributions to tropical cyclone potential intensity by lifting of water and by kinetic

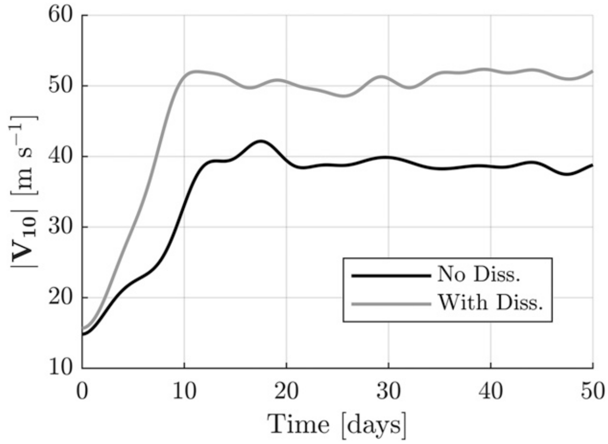

FIG. 1. The maximum wind speed at the lowest model level in two simulations using the axisymmetric CM1 as in RE, but using a dry adiabatic initial and outer boundary temperature profile, turning off latent heating, and imposing a 10-K initial temperature difference between the ground and the air. The black curve is for a control simulation and the gray curve is for an identical simulation but with dissipative heating included.

energy dissipation in the outflow are small, justifying their explicitly acknowledged neglect by RE. We reaffirm the importance of including dissipative heating in tropical cyclone thermodynamics and reject the assertion by ME that only surface latent heat fluxes can power tropical cyclones.

REFERENCES

Batchelor, G. K., 1967: *An Introduction to Fluid Dynamics*. Cambridge University Press, 615 pp.

Bister, M., and K. A. Emanuel, 1998: Dissipative heating and hurricane intensity. *Meteor. Atmos. Phys.*, **65**, 233–240, https://doi.org/10.1007/BF01030791.

——, N. Renno, O. Pauluis, and K. Emanuel, 2011: Comment on Makarieva et al. ‘A critique of some modern applications of the Carnot heat engine concept: The dissipative heat engine cannot exist.’ *Proc. Roy. Soc. London*, **467A**, 1–6, https://doi.org/10.1098/RSPA.2010.0087.

Bryan, G. H., 2012: Effects of surface exchange coefficients and turbulence length scales on the intensity and structure of numerically simulated hurricanes. *Mon. Wea. Rev.*, **140**, 1125–1143, https://doi.org/10.1175/MWR-D-11-00231.1.

——, and J. M. Fritsch, 2002: A benchmark simulation for moist nonhydrostatic numerical models. *Mon. Wea. Rev.*, **130**, 2917–2928, https://doi.org/10.1175/1520-0493(2002)130<2917:ABSFMN>2.0.CO;2.

Cronin, T. W., and D. R. Chavas, 2019: Dry and semidry tropical cyclones. *J. Atmos. Sci.*, **76**, 2193–2212, https://doi.org/10.1175/JAS-D-18-0357.1.

Kieu, C., 2015: Revisiting dissipative heating in tropical cyclone maximum potential intensity. *Quart. J. Roy. Meteor. Soc.*, **141**, 2497–2504, https://doi.org/10.1002/QJ.2534.

Makarieva, A. M., V. G. Gorshkov, A. V. Nefiodov, A. V. Chikunov, D. Sheil, A. D. Nobre, and B.-L. Li, 2018: Hurricane’s maximum potential intensity and the gravitational power of precipitation. arXiv, https://arxiv.org/abs/1801.06833v1.

——, and Coauthors, 2020: Comments on “An evaluation of hurricane superintensity in axisymmetric numerical models.” *J. Atmos. Sci.*, **77**, 3971–3975, https://doi.org/10.1175/JAS-D-20-0156.1.

Montgomery, M. T., and R. K. Smith, 2020: Comments on “An evaluation of hurricane superintensity in axisymmetric numerical models.” *J. Atmos. Sci.*, **77**, 1887–1892, https://doi.org/10.1175/JAS-D-19-0175.1.

Mrowiec, A. A., S. T. Garner, and O. M. Pauluis, 2011: Axisymmetric hurricane in a dry atmosphere: Theoretical framework and numerical experiments. *J. Atmos. Sci.*, **68**, 1607–1619, https://doi.org/10.1175/2011JAS3639.1.

Rousseau-Rizzi, R., and K. Emanuel, 2019: An evaluation of hurricane superintensity in axisymmetric numerical models. *J. Atmos. Sci.*, **76**, 1697–1708, https://doi.org/10.1175/JAS-D-18-0238.1.

——, and ——, 2020: Reply to “Comments on ‘An evaluation of hurricane superintensity in axisymmetric numerical models.’” *J. Atmos. Sci.*, **77**, 1893–1896, https://doi.org/10.1175/JAS-D-19-0248.1.