CORRESPONDENCE

Reply to “Comments on ‘An Evaluation of Hurricane Superintensity in Axisymmetric Numerical Models’”

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1. Introduction

We thank Michael Montgomery and Roger Smith for their comment on our paper “An evaluation of hurricane superintensity in axisymmetric numerical models” (Rousseau-Rizzi and Emanuel 2019, hereafter RE19). We are concerned that most of this comment on RE19 pertains not to the scientific contribution of this paper, but to the assumptions made in previous papers by Emanuel (1986, hereafter E86) and Bryan and Rotunno (2009, hereafter BR09). The validity of the assumptions of E86 and BR09 has already largely been discussed in the papers themselves and in the literature since then. It was not the intent of RE19 to revisit the assumptions of E86, and especially of BR09, the latter of which neither of us authored. However, for clarity, we here address some of the concerns raised in the comment regarding these papers.

The comment seems to suggest that the derivation introduced in RE19 violates moist reversible thermodynamics assumptions (section 4 of the comment), basic calculus and the conservation of angular momentum (section 5), and mass continuity (section 6). We think that the overarching claim that any study violates such a number of the most basic principles of science should be made with more care than is apparent in this comment, and also be more substantiated.

2. Gradient wind PI

The main derivation and assumptions of the gradient wind PI theory are by no means new (E86) and all the

assumptions are detailed in the paper that introduced it. We are merely providing an overview.

a. Response to comment on the assumptions underlying the Maxwells’ relations

The Maxwells’ relations used in this derivation do not require the assumption of reversible thermodynamics, and they have been derived for both reversible and pseudoadiabatic thermodynamics (e.g., Emanuel 1988; BR09, appendix A).

b. Response to comment on the radius of maximum winds

While we agree that it is important, our paper did not address the determination of the radius of maximum winds.

c. Response to comment on the radial momentum equation

The interior, balanced flow only requires as a boundary condition the gradient of moist entropy (s) with respect to angular momentum, and this is just the ratio of the source of entropy to the sink of angular momentum owing to surface fluxes. The formulation in angular momentum (M) space does not require knowledge of the radial velocity. No assumptions are made about the radial velocity or the radial momentum equation. The efficacy of the boundary condition on ds/dM was verified by BR09.

d. Response to comment on balance in the boundary layer

We agree that assumptions relating the gradient wind to the surface winds are needed to simplify this formulation of PI. However, we do not agree that relating the surface winds to the gradient wind implies anything about balance in between the surface and the top of the

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boundary layer. In this framework, supergradient winds are still possible in the boundary layer, and we bound the gradient winds.

The fact that there may be supergradient winds in between 10 m and the top of the PBL does not invalidate our assumptions about the relationship between surface winds and gradient winds. It is a curious result of our differential Carnot cycle approach that the magnitude of the 10-m wind obeys the same relationship as the gradient wind PI, but we still must assume how that 10-m wind is partitioned into radial and azimuthal components.

e. Response to footnote 1 on balance in the boundary layer

We assume that s is well mixed along M surfaces in the boundary layer. We make no assumption about radial wind being well mixed.

3. Azimuthal wind PI

a. Response to comment on difference between boundary layer top and height of maximum winds

Equation (8) in RE19 is stated to pertain to the level of maximum wind, not the top of the boundary layer and we make no assumption about the relationship between the two altitudes. In addition, similar simulations in Rotunno and Bryan (2012, Figs. 3b,f) show that vertical diffusion terms in the radial and tangential momentum equations are small at and above the height of maximum winds.

b. Response to comment on radius of maximum winds

Once again, we are not trying to predict the radius of maximum winds and invite the authors of the comment to develop such a formulation. Note that the gradient wind potential intensity formulation of Emanuel and Rotunno (2011, hereafter RE11) does predict the relationship between the radius of maximum winds and the outer radius though. Once again, there is no evidence that the diffusive terms are very important near the altitude of maximum winds (e.g., Rotunno and Bryan 2012), nor do we think that the good agreement between the theory and observed peak wind speeds is an accident.

c. Response to comment on a priori potential intensity

Nowhere did we describe our analysis as an a priori theory for the potential intensity but rather as an analysis of the validity of potential intensity diagnostics at maximum intensity. An important next step would be to close the theory using, for example, the upper boundary condition developed by RE11.

d. Response to comment on the neglect of the radial momentum equation

Once again, we have no need of the radial momentum equation in our formulation of PI_g and PI_s , but it was used in the original derivation of the supergradient term in PI_a . We agree that the formulations we present here are not closed, absent either an assumption about the radial distribution of boundary layer entropy outside the radius of maximum winds or the imposition of an upper boundary condition as in RE11.

e. Response to comment on Maxwells' relation

As stated in our reply (section 2a) above, we need not assume reversible thermodynamics in deriving the Maxwell relations.

As for the surface PI, it is clearly stated in the text that the assumption is that of reversible thermodynamics. "Reversible moist entropy" is defined so that the entropy is conserved under reversible moist transformations. It does not mean that there cannot be a source. An "irreversible source of entropy" refers here to the fact that, under reversible moist processes, the total water content of a parcel is not supposed to change, and that such a change would entail a breach of the assumption.

4. Surface PI

a. Response to comment on differential cycle integral

The derivative of an integral is the integrand. There is no mathematical inconsistency in our formulation nor is there any need for a Dirac delta. The terms Tds/dt and $\mathbf{V} \cdot \mathbf{F}$ are finite and integrated over a cycle that is infinitesimally narrow. However, that integral is performed on both sides of an equality. We are stating that if

$$\int_x^{x+\varepsilon} A \, dx = \int_x^{x+\varepsilon} B \, dx,$$

then in the limit that $\varepsilon \rightarrow 0$, we have $A = B$.

b. Response to comment on angular momentum conservation

Conservation of angular momentum is respected here, just as it is when inflowing air loses angular momentum to the surface through the entire depth of the boundary layer. Mixing with high angular momentum environmental air allows the air in the outflow of the hurricane to regain angular momentum while the environment loses some. In other words, we are calculating changes along time-mean trajectories, not following every turbulent eddy.

The stresses taken into account in computing the angular momentum diffusive tendencies are indeed computed by taking the derivative of angular velocity, which is small until the anticyclonic outflow nears the quiescent environmental air mass, near the subsidence radius. This does not change the fact that eddies can move angular momentum radially since, as the authors of the comment point out, angular momentum is conserved (not angular velocity). Clearly, the angular velocity at a given radius cannot change without also changing angular momentum.

c. Response to comment on energetics

Heat energy does not offset the loss of angular momentum, but rather the dissipation of kinetic energy. This is why a gain of angular momentum in the outflow does not affect the energy budget in the same way as a loss of momentum near the radius of maximum winds (RMW). If angular momentum increases by mixing at low tangential velocity, the kinetic energy gain is small. By contrast, angular momentum lost near the RMW is lost at very high velocity, and is thus associated with high kinetic energy dissipation. We mention, “There must also be frictional dissipation in the leg C–C’ associated with a gain of angular momentum, but this will be small if the radius at which this occurs is not too large,” which is a clear explanation for this assumption. We would also like to point out that the energy loss at large radii in the outflow was accounted for in [E86](#).

d. Response to comment on outflow temperature variations

As clearly stated in the paper, one of the major advantages of the differential Carnot cycle theory is that it does not require the whole hurricane secondary circulation to be an actual Carnot cycle. It just requires the Carnot assumptions to be valid near the eyewall. The concerns raised in the comment, with respect to the full Carnot cycle, echo the very reason why this differential cycle theory was developed. The assumption of constant temperature might be poor for leg C–D, but we are not concerned with leg C–D here. We are concerned with the infinitesimally short outflow leg C–C’, for which their concern is immaterial. For the entropy mixing question, see the first paragraph of the point on angular momentum conservation.

e. Response to comment on ice phase

The simulations used here also neglect the impact of the ice phase and so the mathematical derivation is consistent with the simulations. So long as moist slantwise neutrality is assumed, the choice of neglecting the ice phase does not have physical implications for this

model beyond modifying the formulation of the surface thermodynamic disequilibrium. The authors of the comment are welcome to extend our formulation to include the ice phase.

f. Response to comment on Carnot efficiency factor

The entropy gained in leg B’–B needs to equal that lost in leg C–C’, which is lost at temperature T_{out} . The same must be true of the integral of the entropy tendency terms along these legs. Since temperature is known and assumed to be constant along each of these legs, T can be taken out of the integral and one only needs to know the entropy tendency near the sea surface to compute the entropy cycle integral. The entropy tendency near the sea surface is given by Eq. (4) in [RE19](#). Entropy is exchanged at T_s and T_{out} at the surface and the outflow, respectively; hence, the Carnot efficiency factor appears.

g. Response to a revised cartoon of the cycle

The concern expressed here by the authors of the comment, including their proposition of a revised cartoon, is caused by the fact that Fig. 1 seems to imply a merging of streamlines, which is precluded by mass continuity. However, in the description of Fig. 1, we say, “We can take points A and D to be the same in each cycle because they are in regions of very weak spatial gradients of entropy and angular momentum.” We believe this sentence clearly states that we approximate points A and D as common to both cycles in the phase space of the thermodynamic cycle, while at the same time acknowledging implicitly yet clearly that they are not common in physical space. In physical space the two different cycles are actually much more distant in the subsidence region than in the outflow region, and the fact that they are represented as common in physical space is to give the reader a sense of how the physical space relates to the phase space. The air does not need to go physically from B’ to B. It just needs to go from the thermodynamic properties of B’ to those of B, which is fully consistent with the steps of the derivation. Since we believe that this is clear enough from the paper, and that our Fig. 1 cartoon communicates the information relevant to this derivation, we see no reason to replace the cartoon.

For a response to the comment on angular momentum mixing, see [section 4b](#).

5. Nonclosure of surface PI

[E86](#) is closed by an assumption about the variation in relative humidity in the boundary layer. We could have made the same assumption here and closed the theory,

but refrained from doing so. The only truly closed theory that does not require an assumption about the variation of entropy in the boundary layer outside the radius of maximum winds is that of RE11. We could have married the work of RE11 to our new work to close the theory but elected not to do so in our paper.

6. Concluding remarks

The last sentence of our paper is “It would also be interesting to include a simple boundary layer model in a dynamical PI a priori method to estimate the unbalance in the boundary layer, and to estimate the maximum azimuthal wind for a given environment.”

While we agree that a more complete boundary layer formulation would allow one to estimate the maximum winds in the interior of the boundary layer away from the surface, this would in no way affect the conclusions of our paper.

Finally, this otherwise very detailed comment omits the good correspondence between intensity and the differential Carnot PI presented in Fig. 5 of the paper, which we do not believe is an accident.

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