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Comments on: "An evaluation of hurricane superintensity in axisymmetric numerical models" by Raphal Rousseau-Rizzi and Kerry Emanuel

M. T. Montgomery[a1] and Roger K. Smith[b]
[a] *Dept. of Meteorology, Naval Postgraduate School, Monterey, CA, USA*
[b] *Meteorological Institute, Ludwig-Maximilians University of Munich, Munich, Germany*

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1 Introduction

In a recent paper, Rousseau-Rizzi and Emanuel (2019), henceforth RE19, develop a new theory for the potential intensity (PI) of an axisymmetric tropical cyclone, which predicts the maximum tangential wind component at the surface (nominally 10 m ASL). The new formulation, which is referred to as PI_s, appears to be a descendant of the pioneering formulation of Emanuel (1986), henceforth E86. The theory is constructed using reversible thermodynamics and is contrasted with a corresponding reversible formulation for the potential intensity of the gradient wind (PI_g). These two formulations are presented alongside a summary of the pseudo-adiabatic formulation of the potential intensity of the (total) azimuthal wind as developed by Bryan and Rotunno (2009), henceforth BR09.

RE19 provide an assessment of the three formulations on the basis of moderately high resolution (2 km radial grid spacing), axisymmetric, numerical simulations using two different numerical models. While the agreement between the new theory (PI_s) and the numerical simulations looks persuasive in a first pass, after some reflection there are elements of the derivations that we find puzzling. Because of the possible importance of this research, the purpose of this comment is to articulate these puzzling elements to the broader community in the hope of obtaining a better understanding of the basis for the new PI formulations.

2 Gradient wind PI

In the new derivation of the maximum gradient wind (PI_g), their Eq. (7), RE19 make a few assumptions, both explict and tacit, that we are particularly puzzled with:

- Eq. (1) is the result of combining axisymmetric thermal wind balance in pressure coordinates with a thermodynamic Maxwell relation that requires the assumption of reversible thermodynamics. Why do RE19 not mention the needed reversible assumption here?
- The formulation of the friction layer ignores the nonlinear acceleration in the radial direction, which is needed to accurately determine where inflowing moist air parcels rapidly decelerate, turn upwards and ascend out of the boundary layer. As a result, the gradient wind PI formulation presented herein does not determine the radius of maximum tangential wind, an important quantity to know for a tropical cyclone forecaster. The omission of the full radial momentum equation and the silence about the radii of maximum wind and updraft are limitations of the gradient wind formulation that concern us.
- The friction layer formulation appears to tacitly assume that the boundary layer flow is well mixed in both absolute angular momentum (hence tangential velocity) and moist entropy s_b at leading order since V_{10} is set equal to $V_{g,b}$, and $|\mathbf{V_{10}}|^2$ is set equal to ${V_{g,b}}^2$, where s_b denotes the boundary layer moist entropy per unit mass, $V_{g,b}$ denotes the gradient wind at the top of the boundary layer and $|\mathbf{V_{10}}|$ denotes the surface momentum per unit mass (at 10 m height)[1].

[1]Correspondence to: Prof. M. T. Montgomery, Department of Meteorology, Naval Postgraduate School, Monterey, CA USA. E-mail: mtmontgo@nps.edu

[1]If the flow within the boundary layer differs from $V_{g,b}$, wouldn't one then need a separate equation for this velocity? Our interpretation is consistent with that of Bryan and Rotunno (2009), who, in their derivation of gradient wind PI on p3045 (and its nonlinear generalization discussed in the forthcoming Section 3), explicitly assume that the boundary layer is well mixed: "The derivation considers the variables s, M, and radial velocity (u) to be well mixed (i.e., constant) in the PBL." Here, Bryan and Rotunno (2009) are following E86, p593.

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Since, according to boundary layer theory in the limit of large Reynolds number, the radial pressure gradient is approximately uniform in the boundary layer, the implication is that the boundary layer is tacitly in gradient wind balance also. Since the boundary layer flow owes its existence to the imbalance in the sum of centrifugal, Coriolis and pressure gradient forces per unit mass, isn't the near, gradient-wind, balance property of this formulation a worry?

3 Azimuthal wind PI

In their summary of Bryan and Rotunno's (2009) analytical model for the azimuthal wind (PI_a) given by Eq. (8), RE19 describe the model as a formulation "that accounts for the supergradient contribution" and say also that this formulation represents "a bound for the maximum azimuthal wind". We have a few questions about the accuracy of these characterizations of the BR09 theory:

- The boundary layer height is defined to be the height of the maximum tangential wind (BR09, p3045). However, high-resolution dropwindsonde observations of intense hurricanes indicate that the maximum tangential wind for an intense storm is generally located at heights around $500 - 700$ m, and still well within the frictional boundary layer that is at least 1 km deep or more (e.g., Kepert 2006, Fig. 6; **?**, Fig. 4; Sanger et al. 2014, Fig.10; Montgomery et al. 2014, Figs. 8-10; Zhang et al. 2011). In view of these observations, is the height of maximum tangential wind a defensible definition for the boundary layer height in the model? What would happen if a more realistic boundary layer height were incorporated into the theory?
- Strictly speaking, the extra nonlinear term on the right hand side of Eq. (8), involving in part the azimuthal vorticity and vertical velocity at the radius of maximum tangential wind, arises from the inclusion of the nonlinear momentum advection terms *above the boundary layer* where frictional and diffusive effects are everywhere neglected (see BR09, p3058). Apparently, the putative supergradient contribution does not include the agradient effects in the frictional boundary layer, which are responsible for the boundary layer inflow in the first place. Since the Bernoulli-like function used to integrate along streamlines in and near the eyewall of the vortex does not include friction, the azimuthal vorticity and the vertical velocity that are required to close the theory at the radius of maximum tangential wind must be determined separately as part of a full boundary layer calculation. In fact, the numerical models in this study are used to supply these values at the radius of maximum tangential wind, close to where the air decelerates rapidly and turns upward (Montgomery and Smith 2017, p567)[2]. In addition, like the gradient wind PI, the radius of maximum tangential wind is not predicted by the theory.
- Further highlighting the foregoing concerns, the second term on the right hand side of Eq. (8) cannot be evaluated from environmental soundings, but rather must be computed from the hurricane solution, itself, which is unknown *a priori*. RE19 acknowledge this feature, but given the inability of the formulation to predict the radius of maximum tangential wind, the radius of maximum updraft, the maximum updraft velocity, or the wind structure outside of the eyewall, is it really proper to refer to this formulation as accounting for the supergradient contribution if an important part of this contribution requires a determination of the boundary layer flow and the radius of maximum tangential velocity?
- Whereas the BR09 formulation for PI_a (Eq. (8)) has certainly proven to be a significant improvement over PI_g (Eq. (7)) for axisymmetric hurricane simulations in the limit of small horizontal mixing length (e.g., BR09, Schecter 2013), the foregoing considerations would suggest that the reader would benefit by knowing that the BR09 formulation is only a provisional upper bound, and more accurate upper bounds might be possible incorporating the fully nonlinear boundary layer effects noted above. Indeed, technically speaking, one should not speak of a bound unless a theory is complete. At the very least, we think there should be justification for the neglect of the full radial momentum equation used to obtain Eq. (8) (cf. Smith et al. 2008 and Smith and Montgomery 2008)[3].
- Finally, the BR09 formulation that yields Eq. (8) assumes strictly pseudo-adiabatic thermodynamics, where all condensed water immediately precipitates to the surface, and where the effects of water loading on rising moist parcels and evaporative cooling in precipitating regions are neglected. It would seem inconsistent to compare formulations based on reversible thermodynamics against a pseudo-adiabatic formulation.

4 Surface PI

In section 2 of their paper, RE19 derive a "new form of potential intensity bounding the maximum magnitude of the surface winds" using the idea of a "differential Carnot

[2]In Eq. (8), the direct contribution from the boundary layer enters through the left-hand side term and first right-hand side term via implementation of the E86 boundary layer closure (Eq. (3)) (see also BR09, p3054), together with the assumption that the boundary layer flow is well mixed in absolute angular momentum (hence tangential velocity) and moist entropy s_b at leading order.

[3]Smith and Montgomery (2008) show, in fact, that this approximation cannot be justified on the basis of a simple scale analysis of the vortex boundary layer equations.

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cycle". They state (p1707) that the differential Carnot cycle formulation has the advantage of "only requiring the Carnot cycles assumptions to be valid for the part of the secondary circulation located in the eyewall of the TCs (tropical cyclones, our addition), which is easier to satisfy." As noted by RE19, Hakim (2011)[4] showed that the "secondary circulation of a simulated axisymmetric TC corresponds approximately to a Carnot cycle in the inflow and in the eyewall, but not in the outflow and subsidence regions." The purported advantage of the differential Carnot formulation is that it is a way to avoid defending the explicit assumptions required to close the cycle in the outer part of the vortex. If correct, it would seem to suggest a novel way to analyze thermodynamics cycles and related heat engines in some applications without having to explicitly formulate half of the cycle! However, we have questions about the integral calculus used in this new methodology.

In their derivation of the expression for the maximum surface tangential wind (PI_s, their Eq. (16)), RE19 make several physical and mathematical steps that are puzzling also:

- The differential Carnot cycle model is based on the supposition that upon integrating a combination of Bernoulli and heating (entropy) rate functions around two very similar closed paths in the meridional plane (A-B-C-D-A and A-B'-C'-D-A, see their Figure 1), the difference of these integrals will, in the limit as these two loops approach one another[5], yield a finite result in the form of an expression for the maximum *surface* tangential velocity. At first blush, this would seem impossible, since, in the limit, the loop integrals must coincide. That is, the difference between these two closed line integrals in the meridional plane must approach zero. The only possible way out of this conundrum is if the integrands possess a singularity (an infinity, or equivalently, a Dirac delta function) somewhere along the residual loop (B'-B-C-C'-B') before the limit is taken. However, all physical variables employed in the formulation are finite and possess no stated singular structure. How, then, is it possible to obtain a finite result from this construction?
- In the description of the Carnot cycle, RE19 state that the absolute angular momentum M that is lost to surface friction in the inflowing leg is regained in the segments C-D and C'-D through "irreversible mixing" (p1700, rc). But how does irreversible mixing restore the angular momentum lost? Mixing *per se* does not increase M, it merely smears it out spatially in an irreversible manner, like the stirring of milk when added to coffee.
- In the leg C-D and C'-D, the air is assumed to descend isothermally. However, we were under the impression that the isothermal outflow assumption was "poor" (Emanuel 2012, p989[6]). This formulation seems to be a return to the original Carnot formulation of E86 that the second author of RE19 has recently spent effort to improve upon. Similarly, in the legs D-A and D'-A, the air parcels are assumed to regain their moist entropy to equal the starting value at A by "irreversible mixing" (p1700, rc). The same question raised about angular momentum can be asked again here for the moist entropy.
- As a prelude to the derivation on p1701, RE19 note that they neglect the ice phase, for the sole reason that by " ... including it would add terms related to thermodynamically irreversible ice-phase effects such as supercooling". That would not seem a sufficient reason from a physical point of view and the reader is left wondering how this neglect can be justified, given that for much of an air parcel's ascent along the path B-C (or B'-C'), the parcel temperature would be below freezing.
- On the mathematical side, the derivations on p1701 would have been easier to follow had the authors written down the variable of integration. In particular, it is hard to see how Eq. (15) is obtained from Eq. (14), since it would appear that the integrals on each side of the equation have been effectively canceled and replaced by a point evaluation of the integrands.

5 Non-closure of Surface PI

In their conclusions, RE19 state that "While PI_a applies to an actual wind speed and is very useful in assessing the contribution of supergradient flow to azimuthal winds, its computation relies on dynamical diagnostics. PI_s on the other hand, is a straightforward thermodynamic bound on surface winds, a quantity that is more relevant to hurricane risk assessment." We are puzzled by this remark since the formula for PI_s (Eq. (16)) depends on a knowledge of k_s^*, k_{10} and T_{out}, none of which are known *a priori*, but must be determined by running a numerical model (see their section 3b). Indeed, as can be seen in Fig. 5, PI_s depends rather strongly on the assumed horizontal mixing length for heat and momentum, which are presumably taken to be the same in the numerical model used. The same remarks would seem to apply to PI_g. If this is the case, in what sense is the bound for PI_s straightforward that makes it more useful that PI_a, for example? In other words, it would seem that, unlike the PI formulation of E86 and Emanuel (1995), none of the PI's discussed in this paper represent closed theories.

[4]Noted also by J. Persing, 2002, personal communication

[5]"... taking the circuit B'-B-C-C'-B' to be of infinitesimal width...", p1701.

[6]"Emanuel and Rotunno (2011) demonstrated that in numerically simulated tropical cyclones, the assumption of constant outflow temperature is poor and that, in the simulations, the outflow temperature increases rapidly with angular momentum."

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6 Concluding remarks

Here we have sought to articulate questions and concerns that arose while studying Rousseau-Rizzi and Emanuel's presentation of their new reversible, axisymmetric, PI theories, as well as a prior pseudo-adiabatic, axisymmetric, formulation, which retains full nonlinearity above the boundary layer. Notwithstanding their efforts to verify these theories using two axisymmetric numerical models, without convincing answers to these questions, we remain skeptical about the integrity of the new PI theories, especially that for the maximum tangential wind near the surface. In particular, we see such theories to be of limited utility if one has to run a numerical model in order to calculate them.

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