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Comments on: Nonlinear response of a tropical cyclone vortex to prescribed eyewall heating with and without surface friction in TCM4: Implications for tropical cyclone intensification, by J. Heng and Y. Wang

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In their paper, Heng and Wang (2016) purport to clarify what they believe is a recent debate concerning " ... whether surface friction contributes positively or negatively to tropical cyclone (TC) intensification". The study is based on a thought experiment involving "two idealized numerical experiments, one without and the other with surface friction, using the fully-compressible, nonhydrostatic TC model TCM4, with prescribed eyewall heating." The "debate" appears to refer to the efficacy of the boundary layer spin-up mechanism articulated by Smith et al. (2009) and its role in exerting a control on the dynamics of vortex intensification and structure change as discussed by Kilroy et al. (2016).

A main conclusion is that " ... with surface friction included, the intensification rate of the TC vortex is largely reduced, indicating that surface friction contributes negatively to TC intensification" and that "although surface friction largely enhances the boundary layer inflow and the contraction of the radius of maximum wind (RMW), the positive tangential wind tendency resulting from the frictionally-induced inward absolute angular momentum (AAM) transport in the boundary layer is not large enough to offset the negative tendency due to the direct frictional loss of AAM to the surface."

To the casual reader, these results would appear to refute the validity of the boundary layer spin-up mechanism in tropical cyclones. We argue here that the thought experiment and supporting numerical simulations used to cast doubt on this mechanism are ill-conceived for a number of reasons, not the least of which is the fact that this spin-up mechanism *depends on the presence of friction*! The conclusion that " ... with surface friction included, the intensification rate of the TC vortex is largely reduced, indicating that surface friction contributes negatively to TC intensification" is akin to saying that it is harder to accelerate one's automobile with the brake on, a fact that is hardly controversial. However, we would suggest that Heng and Wang's simulation that excludes friction is irrelevant to the debate. In that case, there is no boundary layer for spin up to occur in. The issue is not whether the spin up is weaker in the presence of friction, it is whether the maximum tangential wind speed is largest in the boundary layer in the case with friction and why.

In a nutshell, the boundary layer spin up mechanism may be understood as follows. It is well known that air parcels converge comparatively rapidly in the boundary layer because, unlike the flow above the layer, which is in approximate gradient wind balance, the flow is subgradient, i.e. the sum of the centrifugal force and Coriolis force acting on an air parcel is less than the inward-directed pressure gradient (see e.g. Montgomery and Smith 2017 and references).

As the air parcels spiral cyclonically inwards in the boundary layer, they lose some of their absolute angular momentum, M to the surface on account of the opposing frictional torque. However, since the tangential wind component, v is related to M by the formula $v = M/r - \frac{1}{2}fr$, where r is the radius and f is the Coriolis parameter, v may increase significantly as r decreases. Indeed, the increase in v may be large enough for v to exceed its local (gradient) value (say v_g) at the top of the boundary layer, if the fractional rate of loss of M is less than the relative rate of decrease in r following an air parcel. Since the rate of loss of M decreases with the number of spirals the air parcel makes per unit radial displacement, the rate is a monotonically decreasing function of the inflow speed. However, the rate increases monotonically with the surface drag, and thus the frictional torque.

If v does exceed v_g at some radius, the agradient force (the sum of the centrifugal, Coriolis and pressure gradient forces) acting on an air parcel is positive and we say that the flow there is *supergradient*. If this happens, the agradient force combines with the radial frictional force to produce a rapid deceleration of inward-moving air parcels, whereupon the flow turns upwards. As air parcels are expelled vertically from the boundary layer, they carry their tangential momentum with them and the positive agradient force drives them outwards while approximately conserving their M. As a result, v decreases as the air parcels adjust towards a new state of gradient balance above the boundary layer.

Whether or not v does actually exceed v_g at some inner radii can be ascertained *only* by doing a nonlinear boundary layer calculation or a full vortex simulation, although the foregoing considerations show this to be a plausible possibility. Indeed, many tropical cyclone simulations show that the maximum tangential wind is located within the strong inflow layer (e.g. Zhang et al. 2001; Smith et al. 2009; Persing et al. 2013) as do many recent observational analyses of real storms (e.g. Kepert 2006a,b; Bell and Montgomery 2008; Zhang et al. 2011; Sanger et al. 2014; Montgomery et al. 2014). In particular, both simulations and observations of intensifying and mature tropical cyclones show regions of supergradient flow as the air decelerates radially in the boundary layer and turns upwards into the eyewall (Bao et al. 2012; Smith et al. 2009; Montgomery et al. 2014).

The most surprising aspect of Heng and Wang's paper is that after spending a whole introduction casting doubt on the efficacy and importance of the boundary layer spin up mechanism, they point out on page 1320 and show in Fig. 3d that the maximum tangential wind speed in their simulation with friction included "occurs in the interior region in the boundary layer"! If this is not simply the boundary layer spin up mechanism at work, we wonder how they explain this result? It is certainly not the classical (or conventional) balance spin up mechanism[1], which they argue on page 1316 to be the explanation for spin up! Indeed, the authors themselves appear to unconciously support the boundary layer spin up mechanism articulated above in their statement in the abstract: "Although surface friction shows an overall net negative effect on TC intensification, it plays a critical role in producing the realistic boundary layer structure with enhanced inflow, *a low-level jet in tangential wind with supergradient nature* (our emphasis), and a shallow outflow layer at the top of the inflow boundary layer". What other mechanism would produce the supergradient winds?

It would seem to us that one of Heng and Wang's concluding statements (on p1331) that "The negative contribution of surface friction to TC in-

[1]See Montgomery and Smith (2014) or Montgomery and Smith (2017) for an in-depth discussion of this and other mechanisms of spin up.

tensification found in this study contradicts the positive contribution hypothesis of Smith et al. (2009)" is misconstrued as the authors demonstrate nicely with their model simulation with friction included that "the hypothesis" is alive and well! Rather than "clarifying the debate" concerning the boundary layer spin up mechanism, Heng and Wang seem to have misunderstood and confused the issues involved.

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