# Review of "100 years of progress in tropical cyclone research" by Kerry Emanuel

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This review of tropical cyclone research is a massive undertaking (96 pages of references!) and the author has done a masterful job in putting it together. I have a number of comments, none major, plus a bunch of typo corrections, so I recommend publication subject to minor revision.

## Comments

1. Line 60: "warning" → "warnings".
2. Line 105: "has" → "had".
3. Line 142: "combing" → "combining".
4. Line 370: In the paragraph on UAVs, the role of the Global Hawk in NASA's HS3 project certainly deserves mention; even with the problems experienced with this platform, it was able to make extended observations from 55000-60000 ft of developing and mature TCs in all corners of the Atlantic basin from a launch point at Wallops Island.
5. Line 497: Does "latter" refer to Shutts or Kleinschmidt?
6. Lines 502-503: This sentence is confusing. Isn't $M$ just angular momentum?
7. Lines 512-518: I have always wondered about the D-A leg in the thermodynamic efficiency calculation for a TC. The work done in a thermodynamic cycle is

$$w = \oint T ds \tag{1}$$

   where $w$ is the work done per unit parcel mass, $T$ is the temperature, and $s$ is the specific entropy of the parcel, not the saturated entropy $s^*$. I therefore don't see why constant $s^*$ helps on leg D-A unless the environment is saturated, which seems unlikely. Some further explanation is needed here. (The assumption of moist-neutral stability in the environment should be stated explicitly, as is the inclusion of latent heating in $dq$.)
8. Equation (8): $\dot{Q}$ needs to be defined. (Heat input?)

9. Equation (10): This integrates just over the boundary layer. However, there is some latent heat input via diffusion of water vapor into it as it descends through the free troposphere. I suspect that you are ignoring that on the assumption that it is counter-balanced by radiative cooling as the parcel descends. This would be ok if the specific entropy (as opposed to the saturated specific entropy) of the parcel remained constant in the descent. If you are assuming this as an approximation, it needs to be mentioned and the reasoning needs to be made explicit.

10. Discussion after line 657: It seems to me that Ooyama (1982; J. Met. Soc. Japan, vol 60, p 369) is important and ought to be discussed somewhere, as it provides a clear explanation of the cooperative intensificatiom mechanism and debunks many interpretations of CISK.

11. Lines 666-670: Kilroy et al. (2017; QJ, vol 143, p 450) also suggest that the physics of intensification (i.e., Ooyama's cooperative intensification hypothesis) is the same throughout intensification.

12. Line 742, 754: Wouldn't "angular momentum" be better than "angular velocity"?

13. Line 817: "convective coupled" → "convectively coupled".

14. Line 880" "classes if spiral" → "classes of spiral".

15. Line 914: Do you mean "...also neglects the vertical flux..."?

16. Line 934: "whether" → "where"?

17. Lines 1103-1104: Awkward wording (too many "models"); suggest "...and even today, some research and operational forecasting efforts employ uncoupled models."

18. Line 1334: "date" → "dates".

19. Line 1348: "have" → "has".

20. Line 1368: NOAA carried out IFEX2010 (http://www.aoml.noaa.gov/hrd/HFP2010/IFEX.html) in cooperation with PREDICT and GRIP, so they probably should be mentioned as well. See https://doi.org/10.1175/BAMS-D-12-00089.1

21. Lines 1383-1384: I am not so sure that subsurface ocean conditions don't affect cyclogenesis. Ordinary convective systems in the West Pacific can quickly demolish a thin, sun-warmed layer at the ocean surface and the shallow thermocline in the East Pacific makes that region sensitive to mixing by garden variety convectively induced winds as well.

22. Lines 1413-1414: I think that this is unfair to Montgomery and colleagues. Even Montgomery doesn't say that surface fluxes aren't needed for cyclogenesis, in spite of his arguments that WISHE isn't important. If you look closely, what he is really saying is that putting a cap on the increase in fluxes with increasing winds reduces but doesn't stop intensification. This is actually easy to explain – as a system intensifies,

its gross moist stability decreases, making the surface fluxes more efficient generators of convection. (However, I do think that Mike overstates his case!)

23. Lines 1426-1427: "...but how that core gets established and what happens afterwards remain controversial." You might take a look at Raymond et al. (2014; Australian Meteorological and Oceanographic Journal, 64, 11-25.) In summary, every genesis case we have looked at exhibits the development of a mid-level vortex previous to genesis, which by PV inversion reduces the low to mid-tropospheric moist convective instability, thus producing bottom-heavy convective mass fluxes and strong low-level convergence. This promotes cyclogenesis, as occurs in your TEXMEX paper with Marja Bister. The key additional point is the existence of moisture quasi-equilibrium, in which the moist convective instability and the saturation fraction are inversely correlated. There are good cloud-physical reasons for this, but the consequence is that a stable environment is moist and vice versa, at least in strongly convective situations. We believe that in such situations the arrow of causality points from stability to moisture rather than vice versa. This is because the stability is a result of the PV distribution, which generally changes on a longer time scale than convection and environmental moisture. Thus, the problem of cyclogenesis reduces to understanding how the deep PV anomaly forms. Over warm SSTs, tropical convection generally has top-heavy mass flux profiles, which promotes the production of mid-level vorticity anomalies. So, if some mechanism produces a concentration of such convection, it can set the scene for cyclogenesis. In the real world, pre-existing disturbances do this. However, I would bet that the long simulations initialized with noise that result in eventual cyclogenesis also proceed by first producing a mid-level (or deep) vortex. Curiously, in the case of Typhoon Nuri (2008), the pre-existing wave had a shallow, surface vortex (typical of the west Pacific), which deepened before a low-level cyclone formed. Dave Nolan has seen this type of behavior in some of his simulations as well. The latest Kilroy et al. (2017) paper claims to see genesis in a model without a pre-existing mid-level cyclone, but they start from the moist, already-stabilized environment that developed after a large convective blowup in the development of Hurricane Karl (2010). Hope this helps.

24. Line 1441: A neutral environment in the tropics isn't going to stay that way for long unless it has the proper vorticity structure to support it, as described above.

25. Line 1451: Neutral GMS is not a given – it only occurs if you have either a very dry environment that makes the convective mass flux profiles bottom-heavy, or a moist environment stabilized by vorticity.

26. Line 1456: "...cyclonic vorticity must increase with altitude..." Yes!

27. Line 1464: "...cold-core cyclones..." Yet another way of stabilizing the environment.

28. Line 1532: "developed" → "develop".

29. Line 1594: "have" → "has"

30. Line 1634: Suggest "in tropical cyclone" → "on tropical cyclones".

31. Line 1836: “plays” $\rightarrow$ “play”.
32. Line 1873: “fluids” $\rightarrow$ “fluid”.
33. Line 1900: “cyclone” $\rightarrow$ “cyclones”.