


















Review of “100 Years of Progress in Tropical-Cyclone Research” by Kerry Emanuel

General Evaluation: The MS at hand presents a comprehensive summary of developing understanding of Tropical-Cyclone (TC) dynamics since the founding of the American Meteorological Society. It is an impressive story. Before the 20<sup>th</sup> Century practical knowledge was primarily the navigators’ “Law of Storms” based upon cyclonic flow around low pressure centers. This chapter describes how the modern synthesis of TC motion, structure, dynamics, intensity change, role in climate, etc. emerged during the 20<sup>th</sup> and early 21<sup>st</sup> Centuries—largely since 1940. For the most part it is comprehensive and adequately written. A particular strong point is the comprehensive reference list. I’m recommending PUBLICATION AFTER MINOR(ish) REVISIONS.

















My significant reservations are: **The tone of abstract and the first page or so of the body is too effusive.** While the story is impressive, **fewer superlatives would communicate it more effectively.** The first few pages could do a better job of setting the stage with **more discussion of 19<sup>th</sup> Century foundations.** The writing generally needs the efforts of a good copy editor. I couldn’t resist providing too many recommendations on the writing, but **professional help is what’s needed here.** An awkward point is that **the story is very Emanuel-centric.** Unquestionably the author’s contributions are stellar, but judiciously adjusting the emphasis would improve the narrative. This comment applies especially to one-sided discussions of a couple of controversial topics. A key part of the story should address ideas that didn’t work out. They are mentioned, but concise details of why they failed would add to the narrative. On the other hand the disparagement of CISK is perhaps excessive, even though it is a good cautionary tale of how what everybody thinks can become what nobody believes. **Finally, a paragraph or two about operational forecasting just before the summary would be a productive addition.**
















In the detailed comments that follow numbers in parentheses are MS line numbers:











1. (1) How about “A century ago meteorologists regarded tropical cyclones as shallow vortices, extending...”? Passive voice is not the way to keep readers attention. 
2. (4) How about “densely populated” instead of “highly populous”? 
3. (5-6) Revise to “...had blossomed into an endeavor that encompassed fields ranging...”. I by no means intend to impose a different writing style on the whole MS, but the abstract truly needs work. 
4. (16) Wasn’t Piddington’s book titled *The Sailor’s Hornbook*...? 
5. (12-25) Wouldn’t it be worthwhile to mention Fr. Viñes’ monograph, Isaac Cline’s (1926) *Tropical Cyclones*, and perhaps Gisela Kutzbach’s (1979) *The Thermal Theory of Cyclones* as a way of setting the stage and for the main story. 
6. (26-29) Again, fewer superlatives. 
7. (37-40) Revise to “...Duckworth, with navigator Lieutenant Ralph O’Hair, flew an AT-6 trainer into the ‘Surprise Hurricane’ as it made landfall on the Texas coast and became the first aviators to penetrate the eye and live. By the late 1940s airborne reconnaissance of hurricanes and typhoons had become routine.” 
8. (42-43) At this point the author should probably mention Haurwitz (1935) who deduced from the hydrostatic relation that tropical cyclones must extend through the depth of the troposphere. 
9. (49) I’d recommend “mariners” instead of “men”. It’s the non-sexist term that those who follow the sea use for themselves, even though the souls lost with *Warrington* were clearly all guys. 

10. (51) I'm reasonably certain that it was Naval Air Station (NAS) Lakehurst then. A "Naval Facility" used to mean something very different. 
11. (53-54) It's a misstatement that the typhoon inflicted more damage to the 3<sup>rd</sup> Fleet than the enemy had. The court of inquiry wrote that TF38 suffered more damage than it had received previously without inflicting comparable damage on the enemy. US losses in the (broadly defined) battle of Leyte Gulf (October 1944), for example, were much greater, but the battle essentially destroyed the offensive capability of the Imperial Japanese Navy. As an aside, most of the USN combatant ships had radar, but only one photograph comes down to us. 
12. (64) Recommend striking "streaming in". 
13. (72-73) As I read this list of names "pantheon" is the word that comes to mind, but how about changing "luminaries" to "pioneers", or some such. 
14. (74) How about starting a new sentence after "...weather"? Isn't it "...the Institute..."? 
15. (76) Does it make sense to change "notable scientists" to "colleagues"? 
16. (80) Recommend starting with "A seminal paper from that time was..." 
17. (81-82) Recommend revising to "...explicitly links heat transfer..." 
18. (103) Sorry, I can't resist. How about changing "...was a highly advanced view of..." to "...foreshadowed the modern understanding of..."? From here on the writing seems to flow a bit more smoothly. 
19. (152) Strike "to come". 
20. (154) Recommend revising to read "...would also significantly advance tropical...". 
21. (159) Recommend revising to "...work of which he was particularly proud." 
22. (202) recommend revising to "...the then lack of processor speed and memory capacity did not allow..." 
23. (217) Recommend revising to "...from the Air Force. NHRP conducted..." 
24. (225-231) The issues with Miller's calculation are a) That he didn't add the moist enthalpy increment derived from replacing losses to pressure work as the inflowing air expands and that the computed hydrostatic pressure should be in the eyewall not the center of the eye. Of course we now realize that the eyewall slopes outward, which would have complicated the hydrostatic equation. 
25. (243) Would it be more clear to write "...integrate to about 3 simulated hours..."? Also in line (249)? 
26. (272) Recommend revising to "...of a tropical cyclone rather than that of individual cumulus clouds." 
27. (291-297) Would it provide historical context to state that Wave CISK predicted most rapid growth on the smallest scale that could be represented by the computational grid? 
28. (307-315) In contemporary writing STORMFURY was all caps. It's important to recall that STORMFURY was mainstream science of its time. I recommend citing at least one contemporary paper, perhaps Simpson and Malkus (1964) or Gentry (1970). The STORMFURY strategy did not depend upon release of pre-existing conditional instability. The initial hypothesis was that the vortex was near inertial instability so that AgI seeding would release the instability and cause the eye to expand. When observations (or for that matter Riehl's "steady state"  $r^{-\frac{1}{2}}$  profile) showed inertial stability, the hypothesis was revised to construction of an outer eyewall by seeding. Regardless of the validity of CISK, we now know that eyewall replacements weaken hurricanes with  $V_{max} > 50 \text{ m s}^{-1}$ . If the investigators had been able to build outer eyewalls by 

seeding, STORMFURY would have worked. The reasons for STORMFURY's demise (Willoughby et al. 1985) were not enough supercooled water for AGI seeding to work and observed weakening through eyewall replacements in unmodified TCs. It's important to cite original sources and to get the science right, but it's also essential to write about these events in a way that respects Bob's and Joanne's memory and the debt that we all owe to them for their many contributions to the field.

29. (331-332) Recommend replacing "...foundational knowledge on..." with "...fundamental physical understanding upon..." 
30. (355) Recommend revising to "... used the older Omega system based upon ground-based radio transmitters..." 
31. (357-363) The author should discuss the original Dvorak pattern-recognition system based five scene types (Curved Band, Shear, Banding Eye, CDO, and Eye) in visual imagery. In this scheme intensity combines climatological intensification rates with measurements specific to each scene type to produce (sort of) objective intensity estimates. It is now augmented by "Digital" Dvorak that compares the warmest temperatures inside the eye with the coldest temperatures in the surrounding cirrus shield to estimate intensity. Advantages of the latter scheme are that it can be automated and that it works at night. Velden et al. (2006) is the authoritative reference here. 
32. (429) Strike "Jr" after "Fortner". (435) Replace "are associated with" with "cause". 
33. (436) Insert "based upon passive microwave imagery worldwide" after "eyewalls". 
34. (472-656) One questions whether this long, intricate and carefully parsed thermodynamic discussion is appropriate to a reasonably general review article. I found it informative from my own perspective and took the trouble to follow it in detail. Still, how many readers, even given that they are professional meteorologists, will be similarly motivated? This is a discussion that the author and the editors should have. It is vital to keep in mind that the Carnot heat engine formulation must be adequately described here because it is a powerful first-order theory that explains TC energetics well and provides gratifying quantitative agreement with observations. That said, it this the place for so much detail? 
35. (681-687) Perhaps this isn't the right place, but work by Williams et al. (2008) argues that deceleration of the inflow should produce shock-like features in the boundary layer that are governed by a form of Burger's equation. In these features diffusion across the shock also limits the collapse toward a discontinuity. 
36. (702) The Montgomery reference needs editing. 
37. (717) Recommend revising to "...slower intensification and weaker final intensity..." 
38. (723) Is it a reasonable hypothesis that RI happens when nothing inhibits it? 
39. (755-765) Jordan (1961) was the first to describe these transformations and to relate them to intensity change. 
40. (837) It might be clearer to explain the reversals of radial vorticity gradient in terms of a U-shaped wind profile inside the eye. 
41. (862) Insert "shortcoming" after "This". 
42. (880) Change "...if spiral..." to "...of spiral...". 
43. (899) Strike everything after "...research" 
44. (904-906) Recommend revising to read "...energy. In the mature state much of the secondary circulation, especially the eyewall, is driven by frictional inflow in the boundary layer. Boundary layer processes may cause secondary..." 

45. (911) Revise to "...momentum, neglecting any vertical..." 
46. (926) Change "has" to "have". 
47. (932) Doesn't the author mean "wind." or "wind vector.", since wind speed is a scalar? 
48. (962-965) Here might be a better place for the Williams et al. reference from (35). 
49. (1007-1018) This is clearly expressed, significant insight.
50. (1072) Recommend inserting "efforts" between "These" and "are". 
51. (1101) Recommend revising "...owing to the small intensity of..." to "...stemmed from the weakens of...". 
52. (1113) I would change "partial resonance" to "near resonance". 
53. (1139) Why is  $\Delta k_0$  defined here and then not used until much later? 
54. (1196-1211) A series of observational papers based upon Eastern Pacific Hurricane Norbert (Marks et al. 1992, Houze et al. 1992, Gamache et al. 1993) provide observational support or confirmation for these ideas, as does a later paper on Hurricanes Olivia and Jimena (Black et al. 2002). 
55. (1279) Recommend changing "...contributes to loss of predictability of tropical cyclones influenced by such interactions." to "...reduces predictability." 
56. (1294) Recommend revising to read "...parts of New York City, coastal Long Island, and New Jersey..." 
57. (1337) Aren't the gyres northeast and southwest of the center? 
58. (1346-1347) Aren't these authors Tian Youngshan and Luo Zhexian. Chinese family names are typically one syllable and in Asian usage come before the given names, which are typically two syllables. Thus, the citation should be "Tian, Y. and Z. Luo, 1994." Apparently the given and family names were transposed when the paper was first cited in the west. If my guess is correct, it would be a good idea to point out the error in a footnote so that readers can find the publication. 
59. (1356-1357) Upon careful reading, .tThese lines do make sense.
60. (1368-1370) This sentence has a certain Zen to it, sort of like Rumsfeld's "known unknowns".
61. (1375, or so) Does the author want to connect back to Palmén on p. 9 here? 
62. (1466-1468) The citations in these lines need editing. 
63. (Footnote at the bottom of p. 1567) The "oar-loving" Achaeans' ships were fragile by modern standards. While they were seafaring people and knew something of nasty weather, the eastern Med has all kinds of gales. They probably recognized things like the Bora, but it's hard to imagine that medicines would have been part of their seaman's lore. It's a bit much to advance this speculation. 
64. (1599-1631) Although this argument has appeared in a peer-reviewed journal, it is questionable. Heat moves in the solid Earth by molecular diffusion; heat moves in the sea by eddy diffusion. If one imagines that the oceanic mixed layer is 30 m deep and responds in a TC on a diurnal ( $10^5$  s) timescale, the scale thermal diffusivity is  $900/100,000$ , or  $9 \times 10^{-4}$ , a value consistent with published observations (e.g., Cronin et al. 2015) and  $10^3$  x the increased molecular diffusivity cited in Emanuel et al. (2008). The simulation they describe starts from an unrealistic initial condition in which elevated surface temperature extends to 2 m depth in the soil column and is maintained at 2 m depth as the domain boundary condition. In reality (consistent with Fig 18b) small diffusivity means daytime elevated surface temperatures extend to <10 cm depth. Below that the soil temperature approaches the diurnal surface temperature average and at somewhat

- greater depth the seasonal or annual mean. Thus, in soil excess heat moves a factor of 30 more slowly than it does in water and there is much less (3%, or so) of it. One is left with the disappointing conclusion that the initial condition of Emanuel et al. (op cit.) was tuned to get the desired result. A more consistent explanation of intensification over land is that, while the WISHE mechanism is dominant almost universally over the tropical seas, in places like the ITC of the Australian Wet where mean rising motion wipes out the mid-tropospheric  $\theta_e$  minimum or rare instances in the US Midwest where CAPE is much greater than it is in the Trades, lateral enthalpy convergence can sustain a tropical storm. This statement is by no means a refutation of WISHE. It's the exception that supports the general rule. On the other hand, a calculation engineered to confirm —rather than to test—the idea of intensification over a hot land surface detracts from the almost universally correct WISHE argument for intensification over water in the Trades. An informative reference on land-surface properties' role in TCs is Tuleya (1994). 
65. (1714-1717) The argument for sulfate aerosol forcing of suppressed late 20<sup>th</sup> Century hurricanes is interesting and should be considered. It has, however been refuted (e.g., Zhang et al. 2013) persuasively, though perhaps not definitively. Other arguments for dominance of ocean circulation in the 1970-1995 inactive period are the worldwide SST response in which the EOF corresponding to the AMO extends beyond the North Atlantic Basin and also appears in the 1904-1925 less active episode. It is also true that during much of the late 20<sup>th</sup> Century inactive period global air and sea temperatures were rising rapidly. Goldenberg et al. argue that it's increased shear that suppresses Atlantic TCs, not a weaker oceanic energy source. To my mind, the case against the oceanic AMO's role in reduced 1970-1995 activity is overstated here. 
66. (1734) Recommend changing “morphology” to “sedimentation”. 
67. (1725-1773) As a one-upon-a-time geologist, I'm enthusiastic about paleotempestology. Nonetheless, I wonder if the detailed treatment here is a good use of limited number of pages. Particularly so since this chapter devotes essentially no effort to describing advances in operational forecasting. I recommend reducing the paleo discussion to a single paragraph and replacing it with a summary of what's happened with forecasting. 
68. (1745) Recommend replacing “...associated with...” by “...caused by...”. It is a truism in paleontology that the fossil record is incomplete because most creatures don't become fossils. While paleotempestology is fascinating, it's by no means clear that it offers hope for reconstructing a reliable past climatology. Let's hope that it does. 
69. (1760) How about “dripstone” instead of “stalagmites”? 
70. (1765) How about “incompleteness” instead of “compromised quality”? 
71. (1781) Recommend inserting “quantity” between “This” and “is known...”. 
72. (1842) “...slower translation speeds...”
73. (1851) Recommend changing “such a” to “this”. 
74. (1852) Isn't substantial heat export accomplished by mixing warmer mixed-layer water into the colder water below the thermocline? 
75. (Summary) The summary needs work. It's not the place for cheerleading or unfocused generalities. To be sure, it should say that the science has advanced. Then it should list accomplishments and unsolved problems. Perhaps striking the whole first paragraph, except for the last sentence, which could find a place later on, would be a good start.

---Best of luck with the revised chapter. I look forward to seeing it in print  
Hugh Willoughby

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