2.1 FOOTNOTES TO "CONCEPTUAL EVOLUTION"

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1. Introduction

A paper entitled "Conceptual evolution of the theory and modeling of the tropical cyclone" was published in 1982 in the Journal of the Meteorological Society of Japan. Although it has since been cited by many authors in AMS journals, it may be unfamiliar to most AMS members, so that Dr. Joanne Simpson (1996, personal communication) suggested to me that an updated version should be published in an AMS journal. While I have not yet committed myself to go that far, this session on *Historical Perspectives and Future Directions* gives me an opportunity to clarify some issues and comment on others. For those who are still unfamiliar with the 1982 paper, I take the liberty of reproducing its *abstract* (split into three paragraphs here for readability), to give an impression of what the paper is all about:

Dynamically, the tropical cyclone is a mesoscale power plant with a synoptic-scale supportive system. By the early 1960's, the general structure and energetics of the system and basic components of the supportive mechanism were fairly well documented by instrumented aircraft observation of hurricanes and through diagnostic interpretation of the data. The prognostic theory which would have unified these basic findings in a dynamically coherent framework had a more difficult time emerging. When a viable theory finally emerged, a change in the theoretical perception of the problem was necessary.

The parameterization of cumulus convection was an important technical factor in the reduction of a multiscale interaction problem to a mathematically tractable form. Nevertheless, it was the change in our perception of the basic problem and the re-arrangement of priorities that made the parameterization a tolerable substitute for real clouds. Even then, the validity and limitations of the new theory, known as CISK, were fully appreciated only through careful experiments with nonlinear numerical models. In the meantime, the mathematical simplicity of certain parameterization schemes enticed many to apply the scheme to other tropical disturbances, including the easterly wave, in the traditional idiom of linear stability analysis. More confusion than enlightenment often ensued as mathematics overran ill-defined physics.

With further advances in numerical modeling, the interest in tropical cyclone research shifted from conceptual understanding of an idealized system to quantitative simulation of the detail of real cyclones, and it became clear that the intuitive parameterization of whole clouds would have to be discarded. Now that some models have returned to explicit calculation of the cloud scale, one may wonder if all the exercises with parameterized convection were an unfortunate detour in the history of tropical cyclone modeling. The answer depends on one's philosophical view of "progress."

By the time Ooyama (1982; hereafter, O82) was written, the theoretical understanding of tropical cyclones through nonlinear modeling had advanced far beyond the model of Ooyama (1969; hereafter, O69). It was convincingly demonstrated by O69, and confirmed later by more sophisticated models, that the latent heat supply from the warm ocean to the boundary layer inflow was a crucial requirement for the intensification and maintenance phases of tropical cyclones. These models, including ones without convective parameterization, had also reached a consensus that the parameterization, if used, was a technical means of computationally economizing the models, and that the linear instability, commonly known as CISK (see next section), did little toward explaining observed behaviors of real tropical cyclones.

In spite of the fact that CISK, as linear instability, turned out to be nothing but a mathematical curiosity, the acronym had shaped intense debates in certain sectors of tropical meteorology. On one hand, the true achievements of nonlinear models, which were not simple enough to acquire a new acronym, were criticized by historical or imagined association with CISK. On the other hand, the mathematical simplicity of "CISK parameterization" gave birth to many opportunistic linear theories of tropical waves, including the questionable wave-CISK. By dissecting the circumstances in which the acronym had become a useless term in any sensible communication, I wrote O82 for the purpose of promoting reasoned dialogs on substantive science. Except for a warm initial reception in a small circle of readers, the paper soon disappeared from the scene, although CISK did not. Nevertheless, I drew a small consolation from the fact that a flow of CISK-type papers ceased to issue from at least one prominent author, although this could have been pure coincidence,

With the rise of a new advocacy, known as WISHE, the battle of acronyms has recently restarted. A fair review of the arguments from all sides may be found in Smith (1997). More heated exchanges are in *a comment* by Stevens, Randall, Lin, and Montgomery (1997) and *a reply* by Emanuel, Neelin and Bretherton (1997). Since O82 already paid my debt to the community, I would like to stay this time on the side lines. Regrettably, however, the WISHE camp has launched an egregious attack by Craig and Gray (1996), which, in my view, should not stand uncorrected. After some comments on history, I shall comment on WISHE and respond to those direct attacks on my contributions.

2. Tales of a beginning

In Charney and Eliassen (1964; hereafter, CE64), Charney acknowledged conversations with me that had led him to the discovery of *conditional instability of the second kind* (CISK). A lot more than that was actually involved. Through my note scribbled on a Christmas card to Ogura at MIT, Charney learned that something was cooking at NYU, and summoned me to Boston. On January 4, 1963, Charney, Eliassen, Ogura and I sat together for lunch at the

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3.2 *Cooperative intensification and maintenance*

A question has been raised about the meaning of *the cooperative intensification theory*, in the paragraph quoted above. I am not sure, whether it is linguistically a *theory* or not. I could have called it a *point of view*. On the other hand, it should be clearly understood, I thought, what the cooperating participants are. They are, in brief, the primary and secondary circulations, as are discussed in earlier sections of O82. Since this apparently has not been clear, I shall elaborate it below. The discussions were, and will be, limited to the assumed axisymmetry. I should have added *maintenance* to the phrase but omitted it in favor of brevity. However, the exclusion of *genesis* is significant. The root of the genesis question has been discussed separately in a section of O82, and it is hardly an axisymmetric problem.

The primary circulation is the swirling wind of the tropical cyclone, often approximated by a free-spinning steady-state vortex in gradient-wind balance with a low central pressure. Taken alone, the primary circulation is the hurricane-like vortex studied in fluid dynamics, but it does not explain the meteorological characteristics of tropical cyclones. In the cooperative point of view, the primary circulation is important in defining the structure of a rotationally stiffened local environment in which the secondary circulation takes place.

The secondary circulation in an axisymmetric vortex is all the motion that takes place in the meridional plane. In the tropical cyclone, energetically important physical processes are all carried in the secondary circulation, which comprises the following observationally recognized branches: the frictionally induced inflow in the boundary layer, the vertical motion in the inner core region, the outflow in the top layer, and the weak radial inflow in the deep middle layer. The first three branches were historically the subjects of intense investigations. The heat fluxes from the warm ocean to the boundary layer inflow, and the vertical transport of mass of enhanced entropy with concomitant release of latent heat, mainly in the eyewall clouds, are the physical processes of particular importance. The fourth branch in a deep middle layer is crucial for the inward concentration of absolute angular momentum and generation of the kinetic energy of the primary circulation.

We all, not just WISHE, owe our knowledge of tropical cyclones to observational analyses and interpretations of the secondary circulation by steady state theories. In general, however, the steady state theory takes the primary circulation as given and does not explain how it occurs or how its size and intensity are determined. It takes a time-dependent model to understand the cooperative interaction between the primary and secondary circulations, and to explain the mechanism of intensification, as well as the resulting structure, of a tropical cyclone. The model has to be nonlinear in order to represent all the interacting physical processes properly. Such a model also should, as O69 did for the first time, explain the maintenance of mature cyclones without the *a priori* assumption of a steady state.

In a steady-state theory, the vertical branch of rising moist air may be depicted by drawing appropriate streamlines, and may be assumed, as E86 emphatically advocates, to be *strictly* neutral. In a time-dependent dynamic model, there is no place for such a dogmatic assertion. The model has to anticipate, and technically cope with, moist instability in the cloud scales. In the early 1960s, explicit calculation of the cloud-scale convection was beyond our means, and cumulus parameterization was invented to cope with the problem. Charney may have thought the parameterization solved the entire tropical cyclone problem. For myself, it is only an expedient means of coping with one branch, in order to get at other branches of the secondary circulation. With advances in both computers and computational techniques, the need for of parameterization has practically vanished in axisymmetric models, although it may continue to be profitably utilized in computationally more demanding models, depending on the appropriateness of schemes for specific purposes. Therefore, as was fully explained in O82, the parameterization of convection is a technical problem of modeling and not at all an essential requirement for understanding tropical cyclones. It does become an issue, if any scheme is taken out of context and applied as a panacea, as many followers of Charney's CISK have done.

3.3 *Point of departure?*

As their point of departure from CISK, the WISHE papers repeatedly hint at the roles of convection, and boundary layer convergence that supports convection in CISK. I have no apology for Charney's CISK. However, since a dead horse is not much fun to beat, O69 and O82 are always dragged into their ceremonial beating of CISK. My comments below are concerned with those innuendos obviously directed to O69 or O82.

I admit that the initial value of η, or roughly the initial CAPE, that was used for starting all the experiments in O69, was unrealistically large; it was a carry-over from the failed model of O64. However, as soon as the new model ran successfully, it was found that the initial CAPE had short memory and affected very little the significant sections of the experimental runs. I had specifically demonstrated in O69 that a cyclone vortex could not develop by the initial CAPE alone, driving the last nail to the coffin of the dead linear theory. The reason for keeping the initial condition was a matter of computational economy; I had to start a disturbance somehow, although I could have used a finite strength vortex alone as did RE87 and E89 later. For the reasons that were elaborated later in O82, I also knew and, so stated in O69, that there was no physical significance in the incipient stage of the axisymmetric simulation, regardless of the initial conditions employed. Thus, in O82, the initial CAPE was not recognized at all as a factor in tropical cyclone development or genesis.

I have written in O82: *the moist updraft must be convectively unstable,* in the context that the energy necessary to drive the deep-layer inflow must be provided by excess energy released by the moist updraft. This is in the developing stage, and the experiments in O69 showed that the degree of instability was rather small, but even that weak instability had to be maintained, not by the initial CAPE, but by the infusion of latent heat into the boundary layer from the warm ocean. As the cyclone matures, a warm core develops aloft and the moist updraft becomes practically neutral. The model of RE87, which calculates convection explicitly on cloud scales, concludes that convection is *almost* neutral. Were moist ascents always strictly neutral, as asserted by E86, the model did not need to calculate clouds. Although RE87 is considered as a demonstration of WISHE, it is not a WISHE model, since it does not incorporate the assumptions advocated in E86, except for the zero-CAPE initial condition. If the 20–year difference in technology is taken into account, RE87 can be considered as a confirmation or, at the least, non-rejection of O69, in substantial aspects of tropical cyclone simulation.

The prognostic model of E89, in which moist convection is parameterized by the assumption of neutral ascent, is a genuine WISHE model. However, its title referring to cyclogenesis must be taken with a grain of salt, since an axisymmetric model is an obvious example of *playing a game with loaded dice* (see O82). Since later stages of the cyclone development tend to obscure differences in cloud parameterization, I may just say that the neutral ascent with radial diffusion in E89 and the entraining cloud mass flux in O69 are both acceptable means of expediency. In both models, boundary layer inflow converges in the inner core where the parameterized convection occurs (regardless of whether it is strictly or almost neutral), and the importance of the heat fluxes from the ocean are also recognized equally well.

It is, therefore, rather difficult to agree with the standard refrain in WISHE papers that the tropical cyclone in O69 is driven by convection controlled by frictional convergence, while the cyclone in RE87 or E89 is controlled by the surface heat fluxes enhanced by strong winds. At best, it is a chicken-and-egg proposition. If anyone reads O69 without prejudice, and also understands E86 and the more difficult E89, he or she may discover the less-advertised but critical reason for WISHE's success, that brings O69 and E89 much closer than they are advertised to be. I shall explain this below.

3.4 *Why is the air-sea interaction so crucial?*

Although I may not have uttered some magic words that WISHE people like to hear, I have repeatedly referred in O82 to the importance of the air-sea interaction in tropical cyclones. In fact, the majority of experiments in O69 were devoted to showing how crucial the heat fluxes from the warm ocean were to the intensification and maintenance of tropical cyclones. Why is such input from the warm ocean so crucial? Although the reason was clearly stated in O69, it was not elaborated in O82. I take this opportunity to explain the reason, which is, in a nutshell, the subsidence at the top of the boundary layer.

With minor changes of notation, I may copy from O69 the prognostic equation for θ_e of the boundary layer, and other necessary definitions:

$$\frac{\partial \theta_{e0}}{\partial t} + u_0 \frac{\partial \theta_{e0}}{\partial r} + \frac{w^-}{h_0}(\theta_{e0} - \theta_{e1}) = \frac{C_E|v_0|}{h_0}(\theta^*_{es} - \theta_{e0})$$

$$w^- = \frac{1}{2}(|w| - w), \quad w = \frac{\partial \psi_0}{r \partial r}, \quad \psi_0 = \frac{C_D |v_0| v_0}{f + \zeta_0}$$

where u, v, θ_e, ζ, and ψ, with subscript 0, are the customary variables in the boundary layer of a constant thickness h_0; θ_{e1} is θ_e in the layer above the boundary layer; θ^*_{es} is the saturated value at the temperature and pressure of the sea surface; C_D and C_E are the bulk exchange coefficients for momentum and heat energy, respectively, at the sea surface. While w is the vertical motion at the top of the boundary layer, only the subsidence w^- (i.e., it is zero if w is upward) enters the prognostic equation due to vertical discretization of the model.

Although we often say that the frictionally driven boundary layer inflow converges toward the center, it is important to note that it is generally *divergent* in the area outside the radius of maximum v_0, causing widespread subsidence in the outer area. This fact has nothing to do with the way of parameterizing the moist updraft in the inner core region where the inflow is convergent, and may be confirmed for any radial profile of v_0, as long as it resembles an active tropical cyclone. In the outer area, the air is relatively dry above the boundary layer, so that θ_{e1} is significantly less than θ_{e0}. Thus, the third term on the left-hand side of the θ_{e0} equation is a sink term. The magnitude of subsidence w^- is only a few cm/s, according to the result of a typical experiment discussed in O69. On the other hand, $C_E|v_0|$ in the source term on the right-hand side is also a few cm/s even for fairly strong winds. Therefore, in the outer area of a tropical cyclone, the source and sink terms in the θ_{e0} equation are competing at nearly equal rates, just to keep θ_{e0} of the increasing volume of the inflow air near the normal value of the tropical environment.

Malkus and Riehl (1960) showed that an intense tropical cyclone hydrostatically required much higher values of θ_{e0} than the normally available tropical value. It was then shown by calculating along assumed spiral trajectories that the required high values could be attained by a relatively small amount of evaporation from the ocean in the central area of lower surface pressure before the air rises in the eyewall. The calculation, however, started at radius 90 km with the normal θ_{e0}. We know now that a huge amount of evaporation was needed to bring the inflow air of normal θ_{e0} to the point where their calculation began. To clarify this question about attainability of high θ_{e0} further, O69 conducted several experiments in which the size of the warm sea surface was varied.

In the steady state theory of E86, the radial variation of θ_e (= θ_{e0}, above) was derived first by the trajectory method that typically disregarded the sink term. The author recognized that the formula, his (34), led to absurd results, and then proposed "vigorous turbulent exchange of θ_e through the top of the boundary layer" as a remedy. This has conceptually the same effect as the subsidence term in my Eulerian formulation of the boundary layer, but the idea was not followed through. Instead, the prognostic WISHE model, E89, adopted the exact copy of the O69 equation, except for notational changes. I am gratified by his adoption of the formula, but I also know how it works in the model. The legend to his Fig. 13, for example, is essentially an echo of what I found with the same equation in my model, except for artistic embellishments. Yet, E89 insists on such flimsy excuses as quoted below:

> *In effect, the similarity of Ooyama's results and those presented here is due to spatial-coincidence of moisture convergence and HPE convection, This coincidence is at least partially accidental, however. [the rest not copied]*

Whether by accident or not, the subsidence in the outer area controls the radial distribution of θ_e, in such a way that high values are attained only in the last segment of the inflow leg, where the inflow is truly convergent and turns into the updraft. The strength and radial distribution of subsidence are determined not by the air-sea interaction but by the primary circulation. Thus, we see here again, the cooperation of the primary and secondary circulations.

3.5 Coup de grâce?

A valiant but questionable attempt has been published by CG96 to deliver a death blow to CISK. Their strategy is to design a critical experiment which compares the dependence on C_D and C_E of the growth rates of vortex spin-up by either CISK (including O69) or WISHE. (Their C_T is immaterial and omitted here.) They set up a thought experiment, and predict that the vortex growth will be faster in WISHE, but unaffected in CISK, if C_E is increased, and conversely that the growth will be faster in CISK, but unaffected in WISHE, if C_D is increased. Then, actual tests are run with a version of RE87, the results are checked against the predictions, and they proclaim that WISHE beats CISK.

This is certainly a clever design, because both cannot win the contest. Unfortunately, their thought experiment is flawed. As usual, CE64, O64 and O69 are lumped together as CISK, and the prediction for CISK (including O69) is based on common prejudice about CISK. Likewise, the prediction for WISHE is derived largely from their standard cliché. Since C_E is practically infinite in CE64 and O64, these are not testable; neither is E86 which is a steady state theory. Realistically, O69 and E89 are the only contestants, but both use the same boundary-layer equation in which C_D and C_E appear. How is it possible to arrive at diametrically opposite predictions, unless the thought experiment is rigged for desired results?

In the case of O69, there is no need for thought experiments: section 14 of the paper is marked *Experiments with C_D and C_E*, and presents the results of actual experiments, which clearly put O69 in the winner's column.

4. Conclusions

Mozart composed a little minuet (K.1) in 1761 at the age of 5, and the great C major symphony (K.551) in 1788. We may appreciate the musical growth of the genius by referring to Mozart (1761, 1788), but it would be sheer nonsense if the intent were to criticize the symphony by citing immaturity of the minuet. I am not a genius like Mozart, but I also learn and grow. I should not be puzzled by the mentality of many authors who write CISK (CE64, O64 and O69) and then indulge in innuendos on O69 by picking the faults of the first two. Only a few have axes to grind, and the rest follow the fashionable trend created by the few vocal leaders, without taking the time to check the truth themselves.

I philosophically accept that WISHE will be the trend for a while, until another catchy acronym comes along. My current work is toward technological evolution, and I am a little amazed by the fact that my old work is still something to knock around in the conceptual circle. Let me confide my last wish: "Don't bury me in the grave of CISK with Charney." I am quite certain that this wish will be honored, by throwing me down into an unmarked pauper's grave, just as they did to Mozart.

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