Reply

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25 July 1990 and 16 November 1990

1. Response

I welcome this opportunity to reply to Gray's (1991) comments on "Gradient Balance in Tropical Cyclones" (Willoughby 1990). He expresses reservations about four aspects of the analysis: averaging at fixed radii rather than at points positioned relative to the radius of maximum wind, the dynamic role of imbalances, thermal-wind balance as opposed to gradient balance, and representativeness of the cases presented.

a. Averaging strategy

I originally addressed the question of balance to see if the well-developed theory of secondary circulations in balanced vortices (Eliassen 1951; Willoughby 1988) was applicable to tropical cyclones. Gradient (and hydrostatic) balance at fixed radii is a *sufficient* condition for use of this theory.

Gray argues that averaging at points positioned with respect to the Radius of Maximum Wind (RMW) is more meaningful because averaging at fixed radii fails to resolve the structure of the wind maximum. As illustrated in Figs. 1 and 2 of Gray (1991), Gray and his coauthors combined several hurricanes with much different RMWs to form a composite analysis. By contrast, Willoughby (1990) treated each hurricane individually using a time-dependent scheme that analyzed the axisymmetric time-mean structure and the temporal change over a few hours. The cases (Arthur, Elena on 29 August 1985, and Gloria on 26 September 1985) with flat mean profiles that resembled the mean profile in Gray's Fig. 2 derived from averaging over flat individual profiles. When the individual profiles were sharply peaked, the analysis preserved the peaked structure and tracked the changes of maximum wind (V_{max}) and RMW. It is crucial, however, to distinguish between the slowly evolving maximum of the axisymmetric mean wind and the transient, local, often very sharp maxima in the individual profiles that make up the average.

The distinction is important because these features lie near the axisymmetric RMW, but do not necessarily coincide with it. Gray's analysis locks onto the transient maxima rather than the axisymmetric RMW. In a superposition of an underlying balanced vortex and nonbalanced convective or wave motions, Gray's procedure always adds the nonbalanced asymmetric maxima to V_{max} to yield supergradient wind at the RMW. On the other hand, averaging at fixed radii combines each asymmetric maximum with a random assortment of asymmetries along other azimuths to recover the axisymmetric vortex upon which the asymmetric motions appear as eddies. Thus, Gray's procedure always makes the wind at the RMW appear supergradient, leading to the (in my view, spurious) idea that supergradient winds "centrifuge" air out of the eye. By contrast, analysis at fixed radii is simpler, and it accounts for convective or wave eddies properly without invoking an artificially supergradient mean flow.

b. Role of imbalances

Unbalanced flows are not observed in hurricanes precisely because, as Gray's calculations show, small imbalances cause large radial flows. Moreover, even a small radial flow, if it continues in the absence of a counteracting torque, can cause a large change in the primary circulation. For example, a 5 m s^{-1} radial wind blowing outward at the RMW of a cyclone with V_{max} = 50 m s^{-1} and RMW = 20 km, would spin V_{max} down to 5 m s^{-1} in 1 h. When an imposed torque acts to upset balance, the primary circulation does become a bit agradient, but the departure from balance is no larger than the uncertainty in the wind measurements. The imbalance, small though it is, causes a secondary flow that advects nearly enough angular momentum to balance the imposed torque. This dynamic adjustment is what one means by inertial stability.

Because inertial stability works with a vengeance in tropical cyclones, it is possible to calculate both the secondary flow and the evolution of the primary flow directly from the momentum fluxes or heating without knowing the agradient wind. For example, the balanced theory accommodates cumulus friction, about which

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Gray makes so much, as an eddy momentum flux convergence. Similarly, the most plausible explanation for the low-level outflow from the eye is that it stems from the horizontal difference in latent heating between the eye and eyewall; although it is possible to contrive momentum transports to produce the same effect. Real, though small, departures from balance do indeed cause the secondary flows in both cases; but as a matter of practical computation, it is easiest to solve Eliassen's (1951) equation for the secondary flow and substitute into the thermodynamic and momentum equations to evaluate the time changes of the primary circulation and even the departures from balance.

c. *Thermal-wind imbalances*

The reason why Gray looked at thermal-wind balance rather than gradient-wind balance is historical. It lies in the Doppler navigation instrument used formerly. This device determined the flight-level wind as the difference between the aircraft motion over the sea—measured by the Doppler shift of a radar beam reflected from the surface—and the aircraft motion through the air. This estimate of the wind contained an unknown bias caused by the downwind streaming of the sea surface. Elimination of the bias was the motive for subtraction of simultaneous measurements at different altitudes to obtain the thermal wind. With modern inertia navigation equipment, the bias is not a problem; subtraction of winds at two levels increases the error by a factor of $2^{1/2}$, but accomplishes little else. If the pressure difference between levels is hydrostatic, how can the difference between nearly gradient winds at different levels be far from thermal-wind balance?

Accuracy is a real problem in Gray's calculations. The supposed imbalance in Jorgensen's (1984) Allen observations represents a difference in measured wind of <1.4 m s^{-1}, about the same as the rms difference between the gradient and swirling wind in Willoughby (1990). The imbalance is thus within the error of measurement if one considers the increase in error due to the combination of two measurements plus the error in measured virtual temperature due to sensor wetting by rain. Indeed, Jorgensen interpreted these observations as a confirmation of thermal-wind balance in Hurricane Allen. Given the uncertainties of measurement, I doubt that the small differences between the imperfectly known winds at different levels are more meaningful than the winds individually.

d. *Representativeness of cases*

The cases presented in Figs. 1–5 of Willoughby (1990) are admittedly a subset of the data. I chose them for uniform sampling of the vortex and to represent respectively tropical storms, developing hurricanes, intense hurricanes, hurricanes with concentric eyewalls, and hurricanes that had passed maximum intensity. Apart from somewhat better data coverage, they are consistent with the rest of the sample.

Figure 1 of this reply shows some additional cases that have come to hand since Willoughby (1990) was written: Gilbert at maximum intensity on 13 September 1988, Gilbert as it crossed the Bay of Campeche on 15–16 September after weakening over the Yucatan Peninsula, and Hugo as it strengthened in the Caribbean on 17–18 September 1989. These cases, chosen for notoriety more than anything else, are near gradient balance. The individual rms differences between the actual and gradient wind are 4.0, 1.5, and 1.9 m s^{-1}; the errors between the tendencies are 13.6, 0.7, and 3.3 m s^{-1} per 6 h. The weighted average of the wind errors is 2.0 m s^{-1} and of the wind tendency errors is 3.4 m s^{-1} per 6 h, slightly larger than the averages reported in Willoughby (1990). The sizes of the errors increase as the number of radial passes in the analysis decreases.

The large errors in Gilbert on 13 September clearly stem from an analysis based on only six radial passes with uneven coverage on the east side of the vortex. Although Gilbert on 13 September and Hugo on 17 September were both intensifying at the time of observation, Gilbert's winds had a 1.4 m s^{-1} subgradient bias while Hugo's had a 0.8 m s^{-1} supergradient bias. The 4 m s^{-1} supergradient wind at Hugo's eyewall is much like the theoretical supergradient swirling expected to result from decelerating inflow at the RMW [as illustrated in Fig. 6 of Willoughby (1990)], but the observed inflow deceleration is enough to explain only 0.1 m s^{-1} of supergradient swirling. Hugo as shown in Fig. 1c and, to a lesser extent, Anita on 2 September 1977 (Willoughby 1979) are the only instances among those examined with supergradient wind at the RMW; the others are near balance or a little subgradient. The two cases of eyewall supergradient wind and the larger rms error in the new data notwithstanding, the results of Willoughby (1990) are clearly representative and reproducible.

Gray's descriptions of Hurricane Hugo and Typhoon Flo fail to support his argument. In Hugo on 15 September 1989, the low-level aircraft entered the eye at 500 m altitude, encountering peak wind of 89 m s^{-1} (sustained wind, 80–85 m s^{-1}) 9 km from the vortex center (Marks and Black 1990). It circled in the eye for >1 h, climbing gradually on three engines, before exiting at 2 km altitude. Because the eye was clearly not so small at 2 km, the damaged aircraft was able to reduce its angle of bank and maintain altitude while orbiting in the eye. This aircraft passed through the RMW only once at 500 m. The other aircraft traversed the RMW four times at 5 km altitude and measured an average V_{max} of 70 m s^{-1} at an average RMW of 12 km. The observed rise in temperature from the eyewall to the eye, 9°C over 6 km, is consistent with the observed outward slope of the eyewall and reduction of

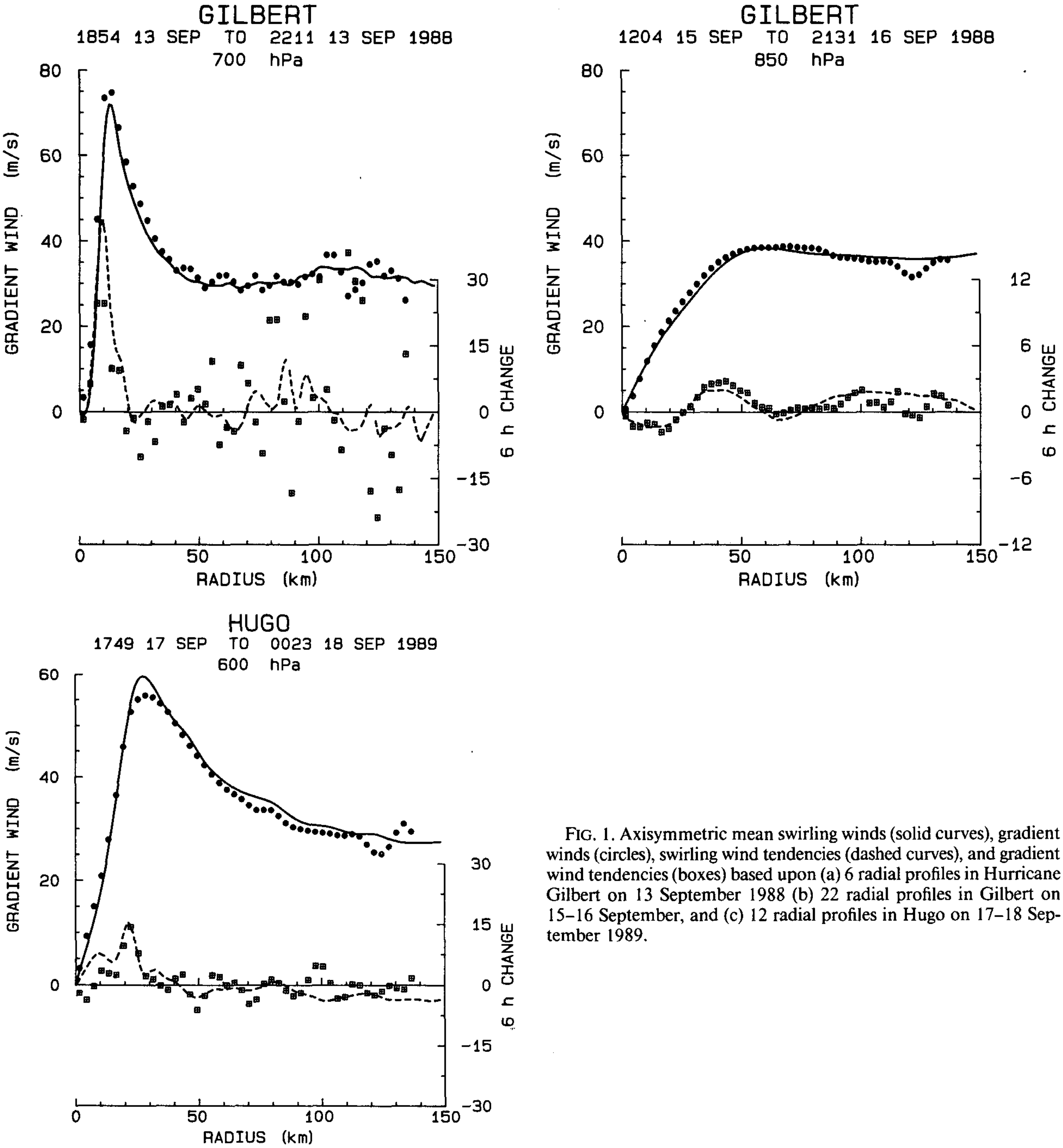

FIG. 1. Axisymmetric mean swirling winds (solid curves), gradient winds (circles), swirling wind tendencies (dashed curves), and gradient wind tendencies (boxes) based upon (a) 6 radial profiles in Hurricane Gilbert on 13 September 1988 (b) 22 radial profiles in Gilbert on 15–16 September, and (c) 12 radial profiles in Hugo on 17–18 September 1989.

V_{max}; although thermal-wind calculations based upon these data are questionable because there are too few observations to form reliable azimuthal averages.

It is difficult to see the relevance of the flight into Typhoon Flo, since Gray reports observations of neither temperature gradient nor low-level wind. Gray's comparison between flight-level wind and satellite estimates of surface wind is a notable example of the innocence about numerical accuracy that pervades his thought on this subject.

2. Conclusion

This reply shows that: (i.) averaging with respect to radius is the preferred way to assess balance of the primary circulation in hurricanes, (ii.) the great inertial stability of the lower-tropospheric primary circulation makes it easier to calculate the secondary circulation directly from the mechanical or thermal forcing than from imbalances of the primary flow, (iii.) the thermal wind is no more reliable as an indicator of balance than the gradient wind itself, and (iv.) the results of Willoughby (1990) both represent conditions in hurricanes and are reproducible. I stand by the conclusion that the axisymmetric, lower-tropospheric primary circulation in hurricanes is balanced to within 1–2 m s^{-1}. The same caveats expressed in Willoughby (1990) apply: first, the radial accelerations must not be large (as may happen in the boundary layer beneath the eyewall); and second, balance prevails only in the axisymmetric mean.

Acknowledgments: I am grateful to Bob Burpee and Lloyd Shapiro for their comments on an earlier draft.

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