Reply

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We are pleased that our paper (Reeder et al. 1991; hereafter RSL) has provoked debate on the issue of tropical cyclone asymmetries and the observational network required for their detection. Before replying to the critique of Holland et al. (1992; henceforth HLD), we would like to restate the claims of RSL.

The *principal* aim of our study was to estimate the minimum horizontal resolution required to detect the large-scale asymmetric gyres that, according to numerical model predictions, are responsible for tropical cyclone motion. To this end, we used a barotropic numerical model with fine (10-km grid) horizontal resolution to generate a vortex with an appropriate gyre structure. The scale of the gyres lay within the range of what might be expected to be broadly typical of a tropical cyclone (see below). Using this vortex solution, we sought to explore the minimum resolution that would be required to reconstruct the vortex asymmetries by performing objective analyses of regular subsets of the data. It was found that for the vortex size chosen, a regular grid on the order of 100 km was necessary. This resolution coincides approximately with the resolution sought by the Hurricane Research Division of NOAA (the U.S. National Oceanographic and Atmospheric Administration) in experiments to determine the environment of Atlantic hurricanes using airborne Omega dropwindsondes (Lord and Franklin 1987; Franklin 1990).

The research was stimulated by our participation in planning for the recent tropical cyclone motion experiment TCM-90 (Elsberry 1990). It seemed worthwhile to apply our methodology to the proposed data network for that experiment at an early stage of planning. Accordingly, we carried out an additional calculation for the *enhanced* radiosonde network shown, for example, in Fig. 4 of Elsberry (1990). We used *all* the stations within the largely oceanic domain that we took, not just a few as implied by HLD. Our results showed that when the vortex was located within this data network, the simulated data were totally inadequate to resolve the gyres realistically. We pointed out (p. 853), however, that typically, in an operational numerical weather prediction environment, an analysis is constructed from both observations and a background field, which is usually produced by an assimilating model. Therefore, in data-sparse regions, the analyses are as much (or more) a product of the assimilating model than they are of the observations. Our note was intended as a warning that *since the assimilating model may contain the gyres, their presence in the analysis does not necessarily imply that they exist in nature or that the model is replicating nature's gyres.*

Holland et al. (1992) carried out a calculation similar to ours using the actual TMC-90 network. They used a *considerably larger* simulated vortex than ours and they considered only the situation where the vortex was located close to the relatively dense land-based radiosonde network. They used also a high density of cloud-drift winds. They showed that the asymmetric gyres could be recovered from the analysis. When the vortex and gyre scales are compared with the data resolution, however, their results are essentially identical with ours. It is very important to recall that RSL suggested that a supplementary data source, such as aircraft dropwindsondes, would be necessary to resolve the gyres. Similarly, HLD introduced satellite-derived winds to their *low-level* analyses. Since satellite data are available only at upper and lower levels in the troposphere, it remains to be shown what contribution they make to the analyzed asymmetries at *other* levels. The relevant (definitive) calculation would be a com-

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parison of three-dimensional analysis with a simulated three-dimensional gyre structure.

We would now like to respond to the assertions of HLD point by point.

1) Vortex size. HLD assert that the vortex profile used is unrealistically small; we disagree. To support this contention we examined storm messages from the Joint Typhoon Warning Center in Guam for the second half of 1991 and carried out a statistical analysis of the "observed" radius of 15 m s^{-1} winds R_{15}. The results are summarized in Table 1. The dataset includes 116 tropical storm reports (maximum wind less than 17.5 m s^{-1}), 94 typhoon reports (maximum wind greater than 33 m s^{-1}), and a subset of the typhoon reports with maximum winds between 37.5 and 42.5 m s^{-1}. These classes are under column headings STM, TYP, 40 m s^{-1} in the table. The average $\bar{R}_{15}$ and standard deviation $\{R_{15}\}$ were calculated for these three classes, shown in the second and third rows of Table 1. We took 300 km for R_{15}; HDL took 400 km. Each value is well within the probable range (+1 or −1 standard deviation) for the typhoons and the 40 m s^{-1} storms. It is true that the reports, which are available four times daily, are not independent of each other, but they are probably the best data available and they counter HLD's assertion.

Holland et al. (1992) argue that the much larger vortex advocated by Evans et al. (1990) is much more realistic because the profile conforms more closely with the radial profiles of outer-region winds in average typhoon data from the western North Pacific (Frank 1977). The difficulty with this argument is that the "average" typhoon does not lie in an "environment" at rest. Accordingly, it is impossible to know which part of Frank's average profile is characterizing the typhoon and which part is characterizing the environment. Again, the partitioning problem is not unique. Of course, HLD may choose to *define* their vortex as including the far-field azimuthal average of the environment, but they are *not* entitled to claim that our own method is incorrect.

To illustrate this problem further, consider, for example, the case of a vortex embedded in a uniform zonal shear flow as studied by Ulrich and Smith (1991) and Smith (1991). Suppose the shear has a magnitude of −10 m s^{-1} (1000 km)$^{-1}$, and the vortex profile is that used by RSL. Then *the azimuthal average of the combined flow (vortex plus environment) differs markedly from that of the vortex alone in the outer region* (Fig. 1). It is to such a combined flow that Frank's average profile relates, not the vortex alone. We do not wish to imply that this magnitude of shear is necessarily typical of northwest Pacific typhoons. We use it simply to illustrate the point that the symmetric vortex profile that leads to particular asymmetries in a sheared environment is not simply the azimuthal averaged profile of the total flow about the vortex center. Holland et al. (1992) admit in their third footnote that the large cyclone structure in the northwest Pacific is a result of the monsoon shear zone in which they are embedded. They appear to have overlooked the partitioning problem. Note that vortex profiles similar to that of RSL have been used in the important studies of Chan and Williams (1987) and Fiorino and Elsberry (1989).

TABLE 1. Mean radius of gale-force winds $\bar{R}_{15}$ and standard deviation $\{R_{15}\}$ in tropical storms (STM) and typhoons (TYP) in the northwest Pacific during the latter half of 1991. Here 40 m s^{-1} refers to typhoons with reported wind speeds between 37.5 and 42.5 m s^{-1}; N denotes the number of reports in each class. Radii are in kilometers.

	STM	TYP	40 m s^{-1}
N	115	94	20
$\bar{R}_{15}$	187	380	324
$\{R_{15}\}$	101	164	164

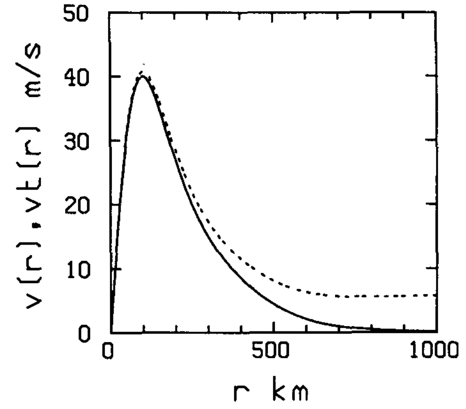

FIG. 1. Radial profile of vortex velocity (solid line) for the vortex used in the calculations by Ulrich and Smith (1991). Dashed line is the azimuthal average about the vortex center of the total flow (vortex and environment) when the vortex is embedded in a uniform shear flow with shear strength 10^{-5} s^{-1}.

2) Analysis method. No apologies are made for the simplicity of our methodology in the belief that a proper understanding of the problems at hand are more likely to emerge from such an approach. We are fully aware that current meteorological data-analysis systems do not make a static analysis and have stressed this point throughout RSL and in the foregoing discussion. We regret, however, the misleading use of the phrase "state of the art" to describe our analysis procedure. Holland et al. (1992) argue that ". . . the time dimension can be obtained from observations alone by the use of a previous analysis and does not necessarily require the use of a numerical model." Even so, the procedure for doing this necessarily involves modeling assumptions of some kind, and one is back to the uncertainty of these assumptions in data-sparse regions as emphasized above!

3) TCM-90 hypotheses. Holland et al. (1992) note that the basis for the TCM-90 experiment was to test hypotheses related to the effects on cyclone motion of the interaction between the cyclone and its environment. To the extent that these tests require detection of the vortex-induced asymmetric gyres there would appear to be an additional and potentially more serious problem than those addressed in RSL and HLD. The difficulty is exposed by model calculations involving vortex motion in a zonal shear flow (Ulrich and Smith 1991; see especially Fig. 7 and the accompanying discussion). In the presence of a shear flow, the vortex-induced asymmetries are apparent only when the vortex *and* the initially imposed shear flow are subtracted from the total flow. This involves a three-way partition of the flow between *vortex, vortex-induced asymmetries,* and the *vortex environment.* In the atmosphere, we do not have an "initially imposed" environment that is unchanging with time, and it is far from clear how a meaningful separation of the vortex-induced asymmetries can be made. Notwithstanding the apparently encouraging calculation of HLD, we believe that the asymmetric gyres may be exceedingly difficult to unambiguously detect in nature.

4) Vortex-center position. No bogus vortex was used in our analysis.

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