Reply

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Leary has argued that our analysis methods and our analyses biased the results which we presented in our original paper (Mogil and Holle, 1972). Leary, also, has noted several limitations that one should recognize before using standard synoptic data. While we disagree with Leary on most points concerning our analyses and analysis methods, we welcome the opportunity to discuss further several aspects not made clear in our original paper. We hope to reaffirm that our analyses are as unbiased as possible.

In Case 1, for example:

1.) We hydrostatically checked all radiosonde reports using significant and mandatory data points as reported in *Northern Hemisphere Data Tabulations*. We used the hypsometric equation, with virtual temperature correction, to recompute layer thicknesses; all computations were performed on a CDC 6400 computer. *While our analyses show reported heights, we used the recomputed values in preparing them.* As we mentioned in our paper some of the height errors were as much as 25 m.

2.) We prepared 4-dimensional analyses to ascertain temporal and spacial continuity. The north-south height gradient and the 200-mb winds both increased with time across western Texas. We found no evidence to support small-scale perturbations in either the height or wind fields over our area of interest; the features were large-scale and quasi-steady state for 24 hours after 0000 GMT on 5 December. Fujita (1955) defined mesoscale as the scale of "disturbances with linear dimensions of 10 to 100 miles." Since both observed and derived fields were larger than 100 mi across we are convinced that the features are representative of the larger scale.

3.) We derived all geostrophic fields from height analyses (spacing 10 m) and not from the isolines as shown in Fig. 4.
4.) We examined average winds for December 1963 at Midland to insure against a systematic bias towards high speeds.

The fact that Leary's analysis for Case 1 is different from ours is not surprising, since:

1.) Leary used a mesh length two and a half times ours.
2.) Vorticity computations are *very sensitive* to mesh length variations.
3.) She analyzed for u and v components directly while we analyzed for isogons and isotachs.

The first two reasons probably account for most of the differences. Assuming f to be nearly constant across western Texas then Leary's Fig. 1 (relative vorticity) can be compared with our Fig. 5B (absolute vorticity). The vorticity gradient along a line from just northwest of El Paso to Del Rio is two and a half times larger in our analysis than in Leary's. The National Meteorological Center currently uses a limited area fine mesh model (LFM) with a mesh length well below the average spacing of radiosondes. We feel that our detailed 4-dimensional analyses justified the use of a smaller mesh length.

For Case 2, Leary has presented an alternative analysis (Fig. 2) to our 200-mb height analysis (Fig. 7). The computational methods used for Case 1 are applicable for this case as well. Extra care was taken for Case 2 analyses owing to the very weak height gradients, which in some instances implied flow opposite to that which was observed.

There are several inconsistencies in Leary's analysis for this case. We plan to examine only two:

1.) If Leary's height analysis is correct then there exists a height gradient of only 7 m between Little Rock and Oklahoma City. This gradient supports a geostrophic wind of only a few meters per second. Since the average wind along this line exceeds 20 m sec^{-1}, the ratio V_{act}/V_g is well above the critical value for anomalous flow. Thus, Leary has introduced even more pronounced anomalous flow by analyzing actual heights than we did by using recomputed heights. In addition, Leary has forced the winds near Little Rock to flow perpendicular to the height contours. While we accept the fact that other reasonable analyses are possible, there is simply not enough height gradient to the east of the anticyclone to support the strong winds; other analyses would likely relocate the region of anomalous flow and not eliminate it.
2.) Leary notes that there are no observations within the 3.0 isoline in the ratio field (Fig. 8A). However, there are several stations within the 2.0 isoline and it is this value which is critical in locating anomalous flow. While there exists a basis for requiring a data point to lie at or near a singular point in *observed* fields, there does not exist such a basis for the analysis of *derived* fields.

In summary, we believe that our analyses, based upon recomputed data and continuity of features in time and space, offer a more realistic solution than Leary's analyses based upon raw data. While we agree that other reasonable analyses are possible, we remain firmly convinced that they, too, will show the existence of anomalous winds.

REFERENCE

Fujita, T., 1955: Results of detailed synoptic studies of squall lines. *Tellus*, **7**, 405–436.