## Reply

J. C. KAIMAL
*Air Force Cambridge Research Laboratories, Bedford, Mass.*
AND J. A. BUSINGER
*Dept. of Atmospheric Sciences, University of Washington, Seattle*
22 December 1970

Lilly's comments on our paper provide a welcome opportunity to clarify some aspects of the convective plume and to further sharpen the requirements to be satisfied by a theoretical model. The two-dimensional model presented in our paper attempts simply to define a plume structure consistent with observational evidence. We should perhaps have pointed out that the main features of this model are found in most plumes regardless of cross section. The large sample of random cross sections available from the Kansas records provides no indication of a well-defined three-dimensional structure in the type of plume discussed in our paper.

Lilly's adaptation of Scorer's bent-over plume model, while attractive at first sight, becomes less so on closer examination. The distribution of wind velocities and temperature predicted by this model departs in many significant details from observational data. Following are some important points of disagreement:

1) In the proposed model the highest temperatures occur at the center of the two helixes. Plume observations, on the other hand, show temperature maxima at the upwind side of the plume, very close to the temperature discontinuity.

2) In Scorer's model $w$ decreases with height ($w \propto z^{-\frac{1}{3}}$). Natural convective plumes observed in the surface layer show a definite increase in $w$ with height. This increase with height, we believe, is critical to the maintenance of the plume's tilt.

3) The bent-over plume model as formulated by Scorer does not provide for downward motion immediately upwind of the temperature discontinuity. Yet this downward motion is a feature common to all natural plumes. Instead, the proposed model predicts negative $w$ along the two sides of the plume.

4) The model suggests that a central traverse through the plume will show a positive correlation between $w$ and $T$, which is indeed observed. But it also suggests that a traverse through either side will show a negative correlation between the two variables. Negative $w$, $T$ correlations are seldom, if ever, observed in association with plumes. Occasionally, a small negative excursion in $w'T'$ may appear near the temperature discontinuity; otherwise, the heat flux is always positive with much of it concentrated near the upwind side.

5) Helical motion, if present in a plume, can be easily recognized from the $v$ component as it is in the dust devil. The data show no evidence of this. A small amount of shear in $v$ is often observed right at the temperature discontinuity, but in only one case did the shear appear large enough to be of any significance.

The above discrepancies between the model and observations are not entirely surprising. The bent-over plume with its dual helical structure is a phenomenon observed in hot chimney plumes rising in a smooth wind (Scorer, 1968). It is easily disturbed by eddies in the surrounding air. Natural plumes, however, tend to maintain their physical integrity even in the presence of moderately high turbulence. In a hot chimney plume (in a neutral or unstable atmosphere) the process is essentially one of dispersion into the environment. In contrast, the natural plume is drawing together the scattered buoyant elements from near the surface into an organized vertical circulation. The physics controlling the dynamics of the two plumes is likely to be different, at least in the lower boundary layer, and this could conceivably result in structures that are dissimilar.

As for Lilly's suggestion regarding the choice of a proper $\bar{u}$ for the plume, we should point out that there is little room for adjustment in the observed value. The positive $u'w'$ near the upwind side of the plume is clearly the consequence of a local increase in $u$ in a region where $w$ is distinctly positive. The 2-min period we used is just long enough to provide a $\bar{u}$ very close to the observed 15-min average and also to include most of the contributions to the $u$, $w$ co-spectrum. Approaching the data with no preconceptions about the plume structure, one finds the 2-min average long enough to include regions of significant rising and sinking motions associated with the plume. There is no evidence to indicate that the sinking motions are confined to the lateral edges as suggested by Lilly. On the basis of available data we have to conclude that the problem of momentum transport in natural convective plumes is a challenge theoreticians must eventually accept.

## REFERENCE

Scorer, R. S., 1968: *Air Pollution*. New York, Pergamon Press, 148 pp.