Reply

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We are delighted that Drs. Smith, Mansbridge and Leslie have carefully read our paper (Eskridge and Das, 1976) and through their comments have given us this opportunity to clarify several points therein. We are especially pleased to find that they accept the view that "If the vorticity source is concentrated at low levels and that thermal forcing takes place from below . . .", tornadoes should form *before* the cumulus clouds with whlch they are associated. This is precisely the interpretation we want our statement quoted by Smith *et al.* (1977) to have. We have not overlooked the possibility of "intense convection aloft in the presence of a sufficiently high level of ambient vorticity" driving a vortex from above. Indeed, we have clearly recognized (see p. 71) a dominant role of the ascending motion of buoyant air in the formation and maintenance of a tornado; what we have difficulty in accepting is that the intense convection required is provided by thermals freely rising from the surface. Such convection needs to be forced and one possible mechanism for such forcing is provided by a precipitation-driven downdraft in stable air. Being interested in quantitative processes we prefer not to speculate before we run models, but just to give a hint of what is in the back of our mind, the pumping process that our trigger mechanism will initiate will end up drawing the moisture from the boundary layer, which will then supply the energy for driving a narrow shaft of intense convection. However, we shall hasten to caution that the picture, though attractive, is overly simplified. Extensive numerical modeling will be required before we can firmly state that such a mechanism does operate.

In the background provided above we can offer a

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critique of the experimental, theoretical and numerical work represented in Turner and Lilly (1963), Turner (1966) and Leslie (1971). It may be noticed that all these studies presuppose convection already exists and is sustained at a high intensity. The convection is prescribed as a sustained body force—a situation not easily visualized in the case of atmospheric vortices which are of a transient nature. A "submechanism" must be found for the development of the sort of intense convection required by these laboratory and numerical models.

The physical background provided above will also take care of the contention of Smith *et al.* that the phasing out of the density anomaly "will cause upper divergence . . . and lead toward the reestablishment of the initial state of solid-body rotation." Their conclusion would be perfectly legitimate if within our limited framework we were proposing a complete model of the concentration of vorticity in a tornadic vortex. However, this is not so. Our assumptions prevent the release of energy in the updraft by condensation. Availability of this energy is likely to produce an end product very different from that predicted by Smith *et al.*

A rather serious point raised by Smith *et al.* concerns the condition $w=0$ at $z=z^*$, the top boundary. This condition attempts to simulate the top of the downdraft in a cumulus cloud as pointed out on p. 73 of the text. Clearly if this level in a cloud is at a fairly constant height it is a reasonable condition. Wisner *et al.* (1972) show by use of a cloud model that after 60 min the height of the top of the downdraft remains reasonably constant (Wisner *et al.*, 1972, Fig. 4b). Even if the top of the downdraft moves, the physics still requires that the angular momentum be advected toward the axis below the top of the downdraft, and while the details of the tangential velocity distribution will be different, the process will still take place.

The suggestion that $\partial w/\partial z=0$ at $z=z^*$ is a better boundary condition is questionable since it will imply that we have either a maximum or a minimum of w at the top. When w is a maximum, w in the downdraft will decrease downward and for reasonable magnitudes of w at $z=z^*$ we can put $w=0$ without serious detriment to the physics of the problem. If w is a minimum, i.e., $|w|$ is a maximum at $z=z^*$, the downdraft decelerates downward and the model fails to simulate that part of the downdraft which we are interested in. As is easily seen the tenet of our paper is a downward *accelerating* downdraft and not a decelerating one.

We must admit that setting $w=0$ is not completely satisfactory, and that we are examining the possibility of using higher order boundary conditions and their implications. The most satisfactory solution to the problem is to construct an axisymmetric model of a rotating cloud in which the top of the downdraft is free to move. Short of doing this any boundary condition at the top of a limited-extent model will remain at least partially unrealistic. By this token the partial justification provided by us for using $w=0$ at $z=z^*$ appears adequate for our preliminary effort.

Finally, Smith *et al.* question our handling of the diffusion terms and the surface boundary layer. We agree that these can be handled better, but we do not accept their contention that the treatment used in our study invalidates, or even seriously affects, our results. For the numerical scheme used we estimate the truncation error or numerical diffusion to be $U\Delta x/12$, U being the local advection velocity. This means that the numerical diffusion varied between 0 and 20 $m^2 s^{-1}$ which is the same order as K. Although we do not advocate using numerical diffusion for any serious calculation, it will be noticed that in our model the regions of high velocity often are also those of high shear and thus the numerical diffusion, albeit fortuitously, increases with the local velocity shear—which certainly is a welcome result. However, we certainly see the need for further numerical experiments with specified, fixed and variable diffusion coefficients. However, as pointed out by Fromm (1968), it is necessary that numerical diffusion be at least an order of magnitude smaller than eddy diffusion to simulate effects of such diffusion correctly.

The treatment of the boundary layer is only qualitatively unsatisfactory and aesthetically unpleasant. Quantitatively, however, there is no serious error in the particular example we have presented. This is so because the maximum inflow velocity under a steady-state initial field of solid rotation and with a value of $K=10$ $m^2 s^{-1}$ is less than 1 $m s^{-1}$. This magnitude pertains toward the lower right-hand edge of the model domain and decreases inward toward the axis. The outflow induced by the imposition of precipitation would easily suppress the inflow in the boundary layer near the axis and the radial velocity picture shown in Fig. 14 ($t=3.84$ min) of our paper will be very close to what we would have found if an initial boundary layer were imposed. From $t=3.84$ min through $t=6.50$ min one would expect that the dominant picture of radial motion would be what is presented in the paper. However, we recognize the caution that is required in accepting this interpretation in its entirety: the motion in the immediate vicinity of $r=0$, $z=0$ is poorly resolved in the present model and needs an improved numerical description.

In conclusion we want to assert that our paper provides an adequately correct picture of the vortex dynamics as is likely to develop from the physical processes and the initial conditions we envisaged. The boundary conditions are probably not the best but they are physically realistic and mathematically consistent. The treatment of diffusion is indirect but, hopefully, does not vitiate the results seriously. We want to make it clear, however, that our study should not be treated as one on the dynamics of a full-blown tornado; it is supposed to point up a triggering mechanism, possibly a very relevant one. The comments of Smith *et al.* (1977) strongly indicate need for further study which alone can clarify all the points raised by them.

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