Comments on "Hail in the Vicinity of Organized Updrafts"

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In their paper, Auer and Marwitz (1972), and again Auer (1972), appear to have misinterpreted both Mason (1956) and Ludlam (1958) on the effects of hailstone melting during fall. The error has important implications for hail suppression theory, and possibly the results of the former paper. They state that "for hailstone diameters >1 cm, the effects of melting (Mason, 1956; Ludlam, 1958) are inconsequential. For example, a 1 cm hailstone is only reduced by melting to 0.9 cm when falling 5 km below the freezing level."

Under the conditions used by Mason, a 1 cm hailstone would, in fact, *completely* melt from such a fall. Ludlam's results are similar: for a fall from the 0°C level at 5400 m to the surface at sea level, he shows complete melting for hailstone diameters $\leqslant 1.46$ cm. For a 4.4 cm (diameter) hailstone, he calculates a diameter at the ground of 4.0 cm, with the fractional change in diameter due to melting decreasing still further with larger initial sizes. Hail suppression theory depends in part, on the benefits of increased melting by the reduction of potential hail sizes (i.e., the conversion of small hailstones to water). Thus the rates of melt suggested by Mason and Ludlam certainly encourage enthusiasm in this area. It appears that Auer and Marwitz mistook "radius" as "diameter" in the results of both Mason and Ludlam, though perhaps less melting would occur if the stone fell in the cold downdraft outflow of the storm.

Table 1 shows Auer and Marwitz's cloud base estimates of hail diameters (at 1.5 to 2 km), the maximum diameters reported at the surface, and extrapolated values of maximum sizes as predicted by Ludlam's results for a 1.5 km fall.

If the ground observations of the small sizes were "unknown" simply because no hail was found, then they may have strengthened their argument, as well as the results of Mason and Ludlam, by considering some hailstone melting.

If this is not so, then it poses the following questions:

1) Was their aircraft actually in the region of largest hail, or at least that hail which was being collected at the surface? The answer to this question may be implied in their reply to Geotis (1973), when they say "Perhaps we should have adjusted the flight paths so that encounters appeared more reasonable, i.e., in the *edge*" (my emphasis) "of the radar echo." This could mean that their aircraft was outside the region of large hail which was observed on the ground shortly afterward.

2) How accurate were their in-flight estimates of size?

3) Are the hailstone melt values given by Mason

TABLE 1. Comparison of in-flight estimates of hailstone diameters (mm) at 1.5 km observed by Auer and Marwitz (1972), the maximum sizes observed at the surface, and the maximum predicted sizes from results by Ludlam (1958).

Diameter range of hail encountered	Maximum observed diameter at surface	Maximum predicted diameter at surface
5– 7	unknown	1– 4
9–11	12	6– 9
9–13	unknown	6–11
12–16	12–20 (or unknown)	10–17
15–17	unknown	13–15
35–40	40	32–38
35–50	50	32–48

and Ludlam in error? In view of the incorrectly quoted melt rates, further justification is necessary.

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