Mielke and Houghton (1977, hereafter “MH”) wrote an interesting article. However, I found myself asking several questions their article failed to answer:

1) Why didn’t MH mention Byers and Braham’s (1949) excellent study? Byers and Braham studied 27 squall line cases, most of which were at least as large as the “echo systems” MH studied. Moreover, Byers and Braham calculated higher correlation coefficients than did MH. Suzuki and Saito (1974) also studied convective systems up to 70 km in diameter, obtaining good correlation between echo size and lifetime; however, they wrote in Japanese, so MH undoubtedly did not read their article! MH noted Barclay and Wilk (1970) but not Aavallelo (1967), who did virtually the same thing without computers.

2) If MH observed “the degree of deviation depended on how strongly the wind veered with height” [which I also noted (Fenner, 1974)], why didn’t they attempt to find a correlation? I found thunderstorm deviate motion well correlated with the veering in the sounding and thunderstorm motion more in the direction of the mean wind shear than the mean wind. The difference between the mean wind and mean wind shear directions gives a good measure of the maximum deviate motion.

3) Why did MH interpolate winds linearly between stations? Since winds in the troposphere are not normally linear phenomena, why didn’t MH use isogon/isotach analyses and take winds from them?

4) Which significance tests did MH use and why? If I do not know what tests they used, I’m not very likely to accept their results.

5) MH use the phrase “mechanical lifting” three times. Do they really believe the dynamic processes of “slope” convection in the troposphere is a mechanical lifting? I hope not!

6) How tall were the echoes? Why did MH use the wind levels they did, rather than, for example, the mean wind throughout the cloud layer? Unless all echoes were the same height, relating them all to a wind at one particular level is not very good.

7) In their Table 3, MH give the “mean direction of wind sample,” which shows very little veering. What was the average veering in the soundings? The average veering in the soundings is not the average veering in the average winds at each level. I found the echo direction departures (the “deviate motion”) excellently correlated with the difference between the direction of the mean wind and mean wind shear, what I called the “mean veering” in the sounding.

While these may all be rather minor comments, I wasn’t satisfied with MH because of them. I felt MH got bogged down in the statistical forest and ignored the sundry trees of interpretation.

REFERENCES


Reply

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It is important to respond to the comments of Fenner since it will help to clarify the intended focus of our paper. Our study was an attempt to identify some characteristics of general precipitation areas that would be relevant to assessing the potential for their explicit representation and prediction in numerical models. It was evident that the analysis had to consider the largest possible scale of coherent organization for these areas in order to be relevant.

Understanding the properties of convective precipitation areas was not an important goal of the study. In fact, a large part of the data sample was for warm front precipitation areas. No attempt was made to determine the depth of the precipitation systems or the details of vertical wind shear, or to examine physical relationships of the echo systems to the synoptic situations. The wind levels considered were intended to be representative of standard levels in a numerical model.

Fenner raised a few specific questions that should be answered. Strict objective linear interpolation was not used for the rawinsonde winds. A subjective approach was used. It was our experience that linear interpolation was quite accurate in most situations. In regions with large gradients second derivative effects were taken into consideration. The statistical F-test was used for determining significance. The term “mechanical lifting” was perhaps a poor choice of words for describing the upward vertical motion in frontal areas.

We hope the comment concerning the supposed failure by us to use a reference because it was in Japanese was only a touch of humor from the Continent. In reply we can note that an unpublished Ph.D. thesis is probably even less accessible and it is hoped that the insights found in the 1974 thesis of Fenner will become more available soon.

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CORRIGENDUM

The following errors were noted in the article “Ice Nucleation Mechanisms of Submicron Monodispersed Silver Iodide, 1,5-Dihydroxynaphthalene and Phloroglucinol Aerosol Particles” by authors Langer, Cooper, Nagamoto and Rosinski (J. Appl. Meteor., 17, 1039–1048):

1. Eq. (4) on p. 1043 should read
   \[ f_{\text{contact}} = f_{\text{experiment}} / f_{\text{capture}} \]

2. Figs. 4 and 6 were interchanged.

3. On Table 2 (p. 1043) in the third column from the left, the last value should be \(8.5 \times 10^{-2}\), not \(8.5 \times 10^{-1}\).