

the juvenal female on the feeder. This was the first time that a juvenal had been seen to be fed at the feeder. The juvenal male was later fed by the adult in a maple and this juvenal then made several passes at the perch but did not alight on it. None of the birds sat on the perch although formerly they had used the clothes line in the same location for a perch. The birds were seen off and on until 9:10 A.M. when the adult shucked a few seeds and then flew away with the juvenals following her. There was a heavy thunder storm in the evening and no grosbeaks were seen again during the summer at this location despite constant observing.

Although no photographic record of these juvenals was possible, a number of reliable birders corroborated the presence of these birds and consequently the record is well substantiated. It had been thought previously that nesting records would be established in the Berkshires rather than in the Connecticut Valley but this has apparently not been so. The most surprising consideration is that 21 years had to elapse before the second Massachusetts breeding record was established and despite the great southern winter penetration of the Evening Grosbeak during 1958 there is still no record of juvenals south of Hadley, Massachusetts.

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GENERAL NOTES

Homing by a Female Cowbird.—A female Brown-headed Cowbird (*Molothrus ater*) was given band 55-187923 by me on April 21, 1958, at Groton, Mass. She repeated six times in May, but not thereafter.

On April 26, 1959 this bird returned, and repeated on May 23, 25, 26 (three times), 30 (twice) and 31. On the last date she was taken to New Ipswich, N. H.—about 18 miles NW—and released. On June 1 she was retrapped at Groton, and released at a point about 5 miles W. On June 2 she was retaken, and released near Fitchburg, Mass., about 12 miles W.

Back in her favorite trap on June 4, she was given the opportunity to accompany me to a forestry meeting at Wilmington, Vt.—about 68 miles WNW—and there turned loose at 6 P.M. Not to be daunted, this determined bird was again in her trap at 10:30 A.M. on June 7.

On June 14, a relative of mine was leaving Groton for his home in Ithaca, N. Y.—about 265 miles, a little south of due west—and agreed to take my bird with him. On the morning of June 15 he released her at his farm near Ithaca, and reported she took flight towards the east. Despite long spells of cloudy, rainy and foggy weather, on June 29 at 6 P.M. this canny bird was again in my trap here, and in good condition.—William P. Wharton, Groton, Mass.

Robin Banded in Massachusetts Recovered in Nova Scotia.—Up to the fall of 1958, no Robin (*Turdus migratorius*) banded at my station in Groton, Mass., had been recovered at any point north of there, with two exceptions—both in southern Maine, and both banded in October. A Robin banded by me as an adult male on October 21, 1957 was found dead on October 7, 1958 at North Sydney, Cape Breton County, Nova Scotia. This bird was evidently a migrant when banded,

spent the following winter in the South, and returned to Cape Breton Island for the nesting season. All other recoveries of this species from my station, amounting to 26, have occurred in the South.—William P. Wharton, Groton, Mass.

Sources of error in banding and homing studies.—All science strives to advance to greater understanding, avoiding past errors and pitfalls. Advances may come, like Darwin's, from long observation and reflection; but more often the investigator breaks from the beaten path more fully to exploit neglected lines of evidence or to open new lines, often using new techniques. No technique, however, can be used to full advantage until its possibilities and limitations are understood.

Though some unimaginative ornithologists scoffed at first, banding is by now recognized as one of ornithology's principal techniques. It has matured greatly, and unwarranted assumptions are less frequent. Authors once wrote calmly that any bird caught 100 miles from its point of banding after 20 days had moved 5 miles a day. Now ornithologists have begun to realize that this is a *minimum*, and unlikely, average movement, and that this or greater distances may well be flown in one or two long flights (not necessarily in a straight line). Actually, barring tiny broadcasting instruments, only the most phenomenal luck could ever enable us to time a wild bird's flight. This would require a color-banded bird known to two banders, both of whom knew its number, colors, and haunts and maintained a constant watch for it in their respective areas; these areas must be at the two ends of a single flight, and bird and bands must survive at least 1½ years! A better prospect would be to choose an exceptionally gregarious and easily trapped bird like the Chimney Swift, *Chaetura pelagica*; catch as many as possible at their autumn maximum at some northern concentration and mark them with electronic emitters, if these can be made tiny enough; and have automatic recording devices installed in nearby areas and farther away, especially at points of concentration to the south. Many men would be needed to handle the birds speedily and release them all simultaneously. All of this would be neither easy nor inexpensive! The same emission could be used only once; and even if all this were done, we would still not know the routes followed, and the results would apply only to that population, not to swifts nor birds in general. Pitfall number one in biology is the tendency to make sweeping generalizations from scanty, unrepresentative, or even carefully selected data, chosen to "prove" a preconceived point of view.

By now most ornithologists realize the fallacy of life-history studies that fail to mark the individual; and I have pointed out (*Wilson Bull.*, 63: 130, 1951) that migration studies can be equally misleading when individuals and populations are not critically analyzed. Authors should not calmly assume that they know everything without the necessity of painfully studying the fundamentals, weighing, measuring, and keeping detailed records. But it is just as painful to read authors who blithely assume that, prior to their own work in banding or "management," generalized studies had taught us *nothing*. Actually, ornithologists do know a good deal about a number of subjects; some, like migration and distribution, have been studied for centuries and are conveniently summarized (for North America) by periodic A.O.U. Check-lists, among other works. Banding or experimental data should normally fall within this framework of knowledge; if yours do not seem to, look closely for errors and unwarranted assumptions before claiming a new discovery. And in any case try always to consider all the many variables (a difficult feat, to be sure, in biology) instead of merely those that support your theory. Think whether your theory will raise more problems than it settles, if it lacks really strong supporting evidence. Above all, let us have an end to papers that state a "conclusion" which is not supported by the data they recite. I noticed three such in one issue of a leading ornithological journal some time ago!

To illustrate some of these pitfalls, let us consider one generally excellent paper: W. John Smith's leading article in the April *Bird-Banding* (30: 69-104) on the dispersal of Michigan Herring Gulls, *Larus argentatus*. He is quite right that northern Canada is thinly settled, and therefore a very small proportion of the banded birds reaching there will be reported (as compared to thickly settled, intensively hunted areas); but the same is true of many other regions, and the language and customs of the hunters, amount of hunting (and the governing economic factors, local wages and costs of guns and shells), and perhaps other factors are