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

important. Folklore, prices, travel and accessibility, seasons, concentration or dispersal of birds and their habitats, and game laws and efficiency of their enforcement all affect the probability of returns being received from an area, not to mention the efficiency of the local postal service. We badly need weighted charts or maps of this probability, for different kinds of birds, to provide a "handicapping" factor for the interpretation of raw banding data. I recommend this to some scientifically-minded linguist with a yen to travel!

The birds of Canada are hardly unknown, however, and there is surely no reason to postulate *wintering* of Michigan Herring Gulls in northern Canada. Water-birds' movements are, perforce, responsive to temperatures and consequent freezing of waters; sooner or later most northern water-birds move, in fall, toward warmer regions (the coasts or southward). Some, including most Laridae, tend to avoid long overland flights and move toward the closer coast. How can anyone deny that regular movements in definite directions are *migrations*, whatever the dates may be? If these gulls are merely wafted east by winds, why have we no evidence that the other soaring birds of Michigan are similarly displaced? We know very well that two soaring birds of the Great Plains, Swainson's Hawk *(Buteo swainsoni)* and Franklin's Gull *(Larus pipixcan)*, do not normally reach even Michigan. As to birds in general, far more birds migrate in autumn from the Great Plains or eastward to Arizona, *against* the prevailing winds, than the number of western United States or southwestern Canadian birds that straggle eastward to the Gulf of Mexico region. This is particularly true among the Fringillidae, and may be correlated with greater seed production (less leafage) in arid regions and other ecological factors, as I have pointed out (*Auk* **61**: 412, 1944) in the case of Cassin's Sparrow, *Aimophila cassini*, whose migrations the ornithologists of today still choose to ignore. Other birds that regularly move west during part or all of their fall migrations include the Western Grebe *(Aechmophorus occidentalis)*, certain geese, and the California Gull *(Larus californicus)*. Westward fall movements are common in Europe despite prevailing west winds, and eastward movements on the continent scarcer.

Winds, then, may aid a bird's movements, but there is no good proof that they determine them. In most cases the movements are regular and innate. Certainly this is so in the highly migratory Sterninae, and it is difficult to see why a comparison of 1936 recoveries of Herring Gulls and Caspian Terns, *Hydroprogne caspia*, should be considered enlightening (p. 91). East and southeast winds do occur, yet seem to produce no fall or early winter records of these young gulls on the Great Plains or in Manitoba or Saskatchewan. More significant yet is their complete absence west of the 100th Meridian, a dividing region whose importance has been known since the days of Baird and Coues. Authors seldom mention the importance of this region, though even a stranger to this continent like the Cattle Egret, *Bubulcus ibis*, has respected it to date! Advocates of "erratic dispersal" (p. 92) would have a stronger case if they showed random crossing of such significant faunal boundaries. The major lesson of banding returns, in most species of wide distribution, is that the populations of the Great Lakes and eastward are quite distinct from those west of the Great Plains.

Most banding studies have one great advantage over experiments in general: the bird's behavior is presumably normal (save for those that become trap-shy or develop the trap habit), its life undisturbed. This is not true of homing experiments where a greatly excited bird is released, perhaps half starved, after being assailed by strange sights and sounds. In such extreme cases as Griffin's work on Gannets *(Morus bassanus)* they were released in unsuitable habitat and promptly "buzzed" by an airplane. Starving men dropped into a desert or sea would seek food and shelter, and so no doubt did the poor Gannets. Why should they presume, in any case, that their nests had survived the *debacle?* The experimenter always forgets the basic fact that *he, not the bird, is conducting an experiment.* If this ever needed proof, it was proved years ago by one of Griffin's own Herring Gulls, which spent a few days resting and feeding at one point in the Great Lakes before continuing its return flight east to its home. Yet Griffin saw no lesson in this and continues to assume that his Gannets seek only the way home; he even speculates that random search is a factor in normal migration, in the face of all the evidence of directional regularity in most birds! Only domesticated birds are relatively undisturbed in homing experiments; and these apparently lack the sensory acuteness of their wild brethren. Similarly, sedentary birds are

less able, or inclined, to "home" accurately under experimental conditions than are regularly migratory species. Let us not forget that even if some recording device tells us when and by what route a bird flies to its nest, it cannot tell us *why* it followed that itinerary.

We need not regard as gospel truth every word in any book, even official Checklists. But let us strive to achieve some harmony between our own conclusions and the many facts already carefully established by the painful investigations of others; for otherwise it is apt to be our own work that is faulty. Above all let us stop and *think* about our own basic assumptions and root out those that are unwarranted. Complacent assumption, taking things for granted, is the main enemy of scientific progress.— Allan R. Phillips, Instituto de Biología, Universidad Nacional Autónoma de México, México, D.F.

Consolidation of Northward Extension of the Glossy Ibis's Breeding Range.—There is mounting evidence that the Glossy Ibis, *Plegadis falcinellus*, is steadily extending its breeding range northward (see Stewart, 1957 and references cited). On 6 June, 1959, my wife and I visited the heronry located in the center of the Pea Island National Wildlife Refuge on the outer banks of North Carolina, for the purpose of banding the young herons and egrets. Since the colony is relatively small, we decided not to band the young for fear of disturbing the birds. (Assistant Refuge Manager Phillips informed us that disturbance in prior years is thought to have been responsible for the decreasing numbers of nesting pairs.) While in the colony for a short time, we observed seven Glossy Ibis flying and perching with the adult herons and egrets which had left the nests. Although we did not find the Ibis nests, I presumed that nests existed at the southern end of the colony. Phillips informed us that eight pairs nested in the colony in 1959. This represents a marked increase over the two pairs known in 1958 by Manager Turner (Phillips, pers. comm.) and is in contrast to the general trend of decreasing numbers of herons in the colony. It appears that the Glossy Ibis is not only extending its breeding range northward, but also consolidating the areas in which it has begun to breed.

The number of breeding areas is also increasing. The *A.O.U. Checklist* (Wetmore *et al*, 1957) lists three breeding localities north of Georgia (p. 54), but a brief look at recent reports (Table I) indicates at least eight nesting localities, and probably more are being utilized. Perusal of recent issues of *Audubon Field Notes* also reveals increasing numbers of Glossy Ibis reported in spring, greater numbers being observed near known breeding sites in summer, and increasing numbers of birds seen in the post-breeding season. For instance, 63 individuals were seen

TABLE I. SOME KNOWN BREEDING LOCALITIES OF THE GLOSSY IBIS—NORTH OF GEORGIA

Locality	*Reference*
Brigantine N.W.R., New Jersey	Potter and Murray, 1957
Cape May County, N. J.	Potter and Murray, 1955, 1956; Stewart, 1957 Potter and Scott, 1958
Chincoteague Bay, Maryland	Stewart, 1957
Hog Island, Virginia	Bock and Terborgh, 1957 Potter and Murray, 1957
Pea Island N.W.R., North Carolina	This note
Starvation Is., N. C. (near Beaufort)	Chamberlain, 1956
Southport, N. C.	Chamberlain, 1956 Stewart, 1957
Waccamaw River, South Carolina (near Georgetown)	Chamberlain, 1957
Drum Is., Cooper River, S. C. (near Charleston)	Chamberlain, 1956, 1957